Work in progress. This version: December 12, 2018 6:00am

Three Dogmas of First-Order Logic

and some

Evidence-based Consequences

for

Constructive Mathematics

of differentiating between

Hilbertian Theism, Brouwerian Atheism

and

Finitary Agnosticism

Bhupinder Singh Anand

Author address:

#1003 Lady Ratan Tower, Dainik Shivner Marg, Gandhinagar, Upper Worli, Mumbai - 400 018, Maharashtra, India
E-mail address: bhup.anand@gmail.com

Contents

Abstract

We show how removing faith-based beliefs in current philosophies of classical and constructive mathematics admits formal, evidence-based, definitions of constructive mathematics; of a constructively well-defined logic of a formal mathematical language; and of a constructively well-defined model of such a language.

We argue that, from an evidence-based perspective, classical approaches which follow Hilbert's formal definitions of quantification can be labelled 'theistic'; whilst constructive approaches based on Brouwer's philosophy of Intuitionism can be labelled 'atheistic'.

We then adopt what may be labelled a finitary, evidence-based, 'agnostic' perspective and argue that Brouwerian atheism is merely a restricted perspective within the finitary agnostic perspective, whilst Hilbertian theism contradicts the finitary agnostic perspective.

We then consider the argument that Tarski's classic definitions permit an intelligence—whether human or mechanistic—to admit finitary, evidence-based, definitions of the satisfaction and truth of the atomic formulas of the first-order Peano Arithmetic PA over the domain $\mathbb{N}$ of the natural numbers in two, hitherto unsuspected and essentially different, ways.

We show that the two definitions correspond to two distinctly different—not necessarily evidence-based but complementary—assignments of satisfaction and truth to the compound formulas of PA over $\mathbb{N}$.

We further show that the PA axioms are true over $\mathbb{N}$, and that the PA rules of inference preserve truth over $\mathbb{N}$, under both the complementary interpretations; and conclude some unsuspected constructive consequences of such complementarity for the foundations of mathematics, logic, philosophy, and the physical sciences.

2010 *Mathematics Subject Classification.* Primary 03B10; Secondary 03F65.

Key words and phrases. Agnosticism, algorithmic computability, algorithmic verifiability, atheism, axiom of choice, Bell's inequalities, Bohm, Brouwer, categoricity, Classical Mechanics, Cohen, completeness, comprehension axiom, consistency, de Broglie, emergence, entanglement, *EPR* paradox, ε-calculus, finitary, formal quantification, first-order logic FOL, fundamental dimensionless constants, Gödel, Goodstein's Theorem, Heisenberg, Hilbert, interpretation, Intuitionism, Law of Excluded Middle LEM, Lucas' Gödelian argument, model, ω-consistency, ω-rule, Peano Arithmetic, Poincarè, Quantum Mechanics, Rosser's Rule C, second-order arithmetic, subsystem ACA_0, Schrödinger's cat paradox, Tarski, theism, truth assignments, uncertainty, undecidability, unspecified.

This work was inspired by, and is dedicated to, the memory of my teacher, late Professor Manohar S. Huzurbazar of Mumbai University, India.

Preface

I shall attempt to offer an integrated—albeit, pardonably, occasionally disjointed and naïve—*evidence-based* perspective of a lay, rather than professional, scholar encompassing fifty years of investigation into various inter-connected, but seemingly independent, grey areas in the foundations of mathematics, logic, philosophy, and the physical sciences.

This investigation is essentially rooted in the *evidence-based* perspective towards 'provability' and 'truth' introduced in the paper [**An16**], '*The Truth Assignments That Differentiate Human Reasoning From Mechanistic Reasoning: The Evidence-Based Argument for Lucas' Gödelian Thesis*'.

The paper appeared in the December 2016 issue of *Cognitive Systems Research*, and addressed the philosophical challenge—briefly, albeit arguably, highlighted in a contemporary, computational, context by Peter Wegner and Dina Goldin in [**WG06**]—that arises when an intelligence—whether human or mechanistic—accepts arithmetical propositions as true under an interpretation—either axiomatically or on the basis of subjective self-evidence—*without* any specified methodology for evidencing such acceptance in the sense of Chetan Murthy and Martin Löb:

> "It is by now folklore ... that one can view the *values* of a simple functional language as specifying *evidence* for propositions in a constructive logic ...".
> ... *Chetan. R. Murthy:* [**Mu91**], §*1 Introduction.*

> "Intuitively we require that for each event-describing sentence, $\phi_{o^\iota} n_\iota$ say (i.e. the concrete object denoted by n_ι exhibits the property expressed by ϕ_{o^ι}), there shall be an algorithm (depending on **I**, i.e. M^*) to decide the truth or falsity of that sentence."
> ... *Martin H Löb:* [**Lob59**], *p.165.*

By *evidence-based* reasoning (Chapter 1, Definition 1.1), I intend reasoning which accepts arithmetical propositions as true under an interpretation if, and only if, there is some specified methodology for objectively *evidencing* such acceptance.

For the purposes of the investigation I shall make (see Chapter 23) an arbitrary distinction between (compare [**Ma08**]; see also [**Fe99**]):

- The ***natural scientist's hat***, whose wearer's responsibility is recording—as precisely and as objectively as possible—our sensory observations (corresponding to computer scientist David Gamez's 'Measurement' in [**Gam18**], Fig.5.2, p.79) and their associated perceptions of a 'common' external world (corresponding to Gamez's 'C-report' in [**Gam18**], Fig.5.2, p.79; and to what some cognitive scientists, such as Lakoff and Núñez in [**LR00**], term as 'conceptual metaphors');

- The ***philosopher's hat***, whose wearer's responsibility is abstracting a coherent—albeit informal and not necessarily objective—holistic perspective of the external world from our sensory observations and their associated perceptions (corresponding to Carnap's *explicandum* in [**Ca62a**]; and to Gamez's 'C-theory' in [**Gam18**], F, p.79); and
- The ***mathematician's hat***, whose wearer's responsibility is providing the tools for adequately expressing such recordings and abstractions in a symbolic language of unambiguous communication (corresponding to Carnap's *explicatum* in [**Ca62a**]; and to Gamez's 'P-description' and 'C-description' in [**Gam18**], Fig.5.2, p.79).

That this distinction may not reflect conventional wisdom is highlighted in §25, where I argue that:

— if mathematics is to *serve* as a lingua franca for the physical sciences,

— then it can only represent physical phenomena *unambiguously* by insistence upon *evidence-based* reasoning (in the sense of Chapter 5)

— which, in some cases, may prohibit us from building a mathematical theory of a physical process

- based on the assumption that the limiting behaviour of *every* physical process which can be described by a Cauchy sequence
- *must* be taken to correspond to the behaviour of the classically defined Cauchy limit of the sequence.

The above attempts to crystalise Hermann Weyl's perspective that (see also Chapter 44):

> "...I believe the human mind can ascend toward mathematical concepts only by processing reality as it is given to us. So the applicability of our science is only a symptom of its rootedness, not a genuine measure of its value. It would be equally fatal for mathematics—this noble tree that spreads its wide crown freely in the ether, but draws its strength from the earth of real intuitions and perceptions (*Anschauungen und Vorstellungen*)—if it were cropped with the shears of a narrow-minded utilitarianism or were torn out of the soil from which it grew."
>
> *... Weyl:* [**We10**]*, p.10.*

Without attempting to address the issue in its broader dimensions, I shall also argue from the perspective that:

(i) Mathematics is to be considered as a set of precise, symbolic, languages;

(ii) Any language of such a set is intended to express—in a finitary, unambiguous, and communicable manner—relations between elements that are external to the language;

(iii) Moreover, each such language is two-valued if I assume that a specific relation either holds or does not hold externally under any valid, *evidence-based* interpretation of the (symbolic) language.

The importance of recognising mathematics as a language of expression and communication of *external*, *evidence-based*, content is that we cannot then admit arguments such as:

> *That our universe is approximately described by Sanskrit means that some but not all of its properties are Sanskrit. That it is Sanskrit means that all of its properties are Sanskrit; that it has no properties at all except Sanskrit ones.*

which highlights, for instance, the incongruity of Max Tegmark's perspective in [**Teg14**]:

> "The idea of spacetime does more than teach us to rethink the meaning of past and future. It also introduces us to the idea of a *mathematical universe*. Spacetime is a purely mathematical structure in the sense that it has no properties at all except mathematical properties, for example the number four, its number of dimensions. In my book *Our Mathematical Universe*, I argue that not only spacetime, but indeed our entire external physical reality, is a mathematical structure, which is by definition an abstract, immutable entity existing outside of space and time.
>
> What does this actually mean? It means, for one thing, a universe that can be beautifully described by mathematics. That this is true for our universe has become increasingly clear over the centuries, with evidence piling up ever more rapidly. The latest triumph in this area is the discovery of the Higgs boson, which, just like the planet Neptune and the radio wave, was first predicted with a pencil, using mathematical equations.
>
> That our universe is approximately *described* by mathematics means that some but not all of its properties are mathematical. That it *is* mathematical means that all of its properties are mathematical; that it has no properties at all except mathematical ones. If I'm right and this is true, then it's good news for physics, because all properties of our universe can in principle be understood if we are intelligent and creative enough. It also implies that our reality is vastly larger than we thought, containing a diverse collection of universes obeying all mathematically possible laws of physics."
> ... *Tegmark:* [**Teg14**].

From such an *evidence-based* perspective, eliminating ambiguity in critical cases—such as communication between mechanical artefacts, or a putative communication between terrestrial and/or extra-terrestrial intelligences (whether mechanical or organic)—seems to me to be the very raison d'être of mathematical activity.

I would view such activity:

(1) First, as the construction of richer and richer mathematical languages that can symbolically express those of our abstract concepts (corresponding to Lakoff's *conceptual metaphors* considered in Chapter 43, and Carnap's *explicandum* considered in Chapter §14) which can be subjectively addressed unambiguously.

Languages such as, for instance, the first-order Set Theory ZF, which can be well-defined formally but which have no constructively well-defined model (see Appendix A) that would admit *evidence-based* (in the sense of Chapter 5) assignments of 'truth' values to set-theoretical propositions by a mechanical intelligence.

By 'subjectively address unambiguously' I intend in this context that there is essentially a subjective acceptance of identity by me between:

- an abstract concept in my mind (corresponding to Lakoff and Núñez's 'conceptual metaphor' in [**LR00**], p.5) that I intended to express symbolically in a language; and

- the abstract concept created in my mind each time I subsequently attempt to understand the import of that symbolic expression (a process which can be viewed in engineering terms as analogous to my attempting to formalise the specifications, i.e., *explicatum*, of a proposed structure from a prototype; and which, by the 'Sapir-Whorf Hypothesis', then determines that my perception of the prototype is, to an extent, essentially rooted in the symbolic expression that I am attempting to interpret).

(2) Second, the study of the ability of a mathematical language to precisely express and objectively communicate the formal expression (corresponding to Carnap's *explicatum* considered in Chapter 14) of *some* such concepts effectively.

A language such as, for instance, the first order Peano Arithmetic PA, which can not only be well-defined formally, but which has a finitary model (Corollary 9.8 and Corollary 9.9) that admits *evidence-based* assignments of 'truth' values to arithmetical propositions by a mechanical intelligence, and which is categorical (albeit, with respect to algorithmic computability—Corollary 11.1).

By 'objectively communicate effectively' I intend in this context that there is essentially:

(a) first, an objective (i.e., on the basis of *evidence-based* reasoning in the sense of Chapter 5) acceptance of identity by another mind between:

- the abstract concept created in the other mind when first attempting to understand the import of what I have expressed symbolically in a language; and
- the abstract concept created in the other mind each time it subsequently attempts to understand the import of that symbolic expression (a process which can also be viewed in engineering terms as analogous to confirming that the formal specifications, i.e., *explicatum*, of a proposed structure do succeed in uniquely identifying the prototype, i.e., *explicandum*);

and:

(b) second, an objective acceptance of functional identity between abstract concepts that can be 'objectively communicated effectively' based on the *evidence* provided by a commonly accepted doctrine such as, for instance, the view that a simple functional language can be used for specifying *evidence* for propositions in a constructive logic ([**Mu91**]).

Moreover, I shall argue that we need to recognise explicitly the limitations imposed by *evidence-based* reasoning on:

– the ability of highly expressive mathematical languages such as ZF to effectively communicate abstract concepts (Lakoff and Núñez's conceptual metaphors), such as, for instance, those involving Cantor's first limit ordinal ω (as detailed in §43.2);

and:

– the ability of effectively communicating mathematical languages such as PA to adequately express such concepts (see §20.7).

I shall argue, further, that from an *evidence-based* perspective, the notorious semantic and logical paradoxes (Chapter 24) arise out of a blurring of this distinction, and an attempt to ask of a language more than it is designed to deliver.

They dissolve once we accept that the ontology of any interpretation of a language is determined not by the 'logic' of the language—which, contrary to conventional wisdom, I take as intended solely to assign unique 'truth' values to the declarative sentences of the language (in the sense of the proposed Definitions 21.3 to 21.7)—but by the rules (Theorem 11.10) that determine the 'terms' which can be admitted into the language without inviting contradiction, in the broader sense of how, or even whether, the brain—viewed as the language defining and logic processing part of any intelligence—can address contradictions (§23.11).

My concerns in these areas have been those commonly shared by scholars of all disciplines—including challenged graduate-level students—with a more than passing interest in the reliability, for their intended individual purposes, of the mathematical languages which any scientific enquiry—by implicit definition—finds essential for attempting unambiguous expression of abstract thought and, subsequently, its unequivocal communication to an other.

I shall argue the thesis—in relatively elementary terms—that the obstacles to such expression and communication are rooted in the disconcerting perceptions of mutual inconsistency between various 'classical' and 'constructive' philosophies of mathematics vis à vis the disquieting, and seemingly 'omniscient', status accorded classically to both mathematical truth and mathematical ontologies (highlighted by Krajewski in [**Kr16**] and Lakoff and Núñez in [**LR00**]); and that such perceptions are, at heart, illusions.

They merely reflect the circumstance that, to date, all such philosophies—whether due to explicitly or implicitly held beliefs—do not unambiguously *define* the relations between a language and the 'logic' (in the sense of Definitions 21.5, 21.6 and 21.7) that is *necessary* to assign unequivocal truth-values of 'satisfaction' and 'truth' to the propositions of the language under a well-defined interpretation.

I argue, moreover, that an epistemically grounded perspective of conventional wisdom—as articulated, for instance, in [**LR00**] or [**Shr13**]—ignores the distinction between the multi-dimensional nature of the logic of a formal mathematical language (Definition 21.5), and the one-dimensional nature of the veridicality of its assertions.

Similarly, classical conventional wisdom based on Hilbert's approach to, and development of, proof theory too fails (see [**RS17**]; also [**Mycl**]) to adequately distinguish that:

(α) Whereas the goal of classical mathematics, post Peano, Dedekind and Hilbert, has been:

– to uniquely characterise each informally defined mathematical structure S (e.g., the Peano Postulates and its associated classical predicate logic)

- by a corresponding formal first-order language L, and a set P of finitary axioms/axiom schemas and rules of inference (e.g., the first-order Peano Arithmetic PA and its associated first-order logic FOL)
- which assign unique provability values (provable/unprovable) to each well-formed proposition of the language L;

(β) The goal of constructive mathematics, post Brouwer and Tarski, has been:

- to assign unique, *evidence-based*, truth values (true/false) to each well-formed proposition of the language L,
- under a constructively well-defined interpretation I over the domain D of the structure S (when viewed as a 'conceptual metaphor' in the terminology of [**LR00**]),
- such that the provable formulas of L are true under the interpretation.

In other words, whilst the focus of proof theory can be viewed as seeking to ensure that any mathematical language intended to represent our conceptual metaphors is unambiguous, and free from contradiction, the focus of constructive mathematics can be viewed as seeking to ensure that any such representation does, indeed, uniquely identify such metaphors.

The goals of the two activities ought to, thus, be viewed as necessarily complementing each other, rather than being independent of, or in conflict with, each other as to which is more 'foundational'—as is implicitly argued, for instance, in the following remarks of constructivist Errett Bishop[1] (and also by Penelope Maddy's perspective in [**Ma18**], [**Ma18a**]):

> " The use of a formal mathematical system as a programming language presupposes that the system has a constructive interpretation. Since most formal systems have a classical, or nonconstructive, basis (in particular, they contain the law of the excluded middle), they cannot be used as programming languages.
>
> The role of formalisation in constructive mathematics is completely distinct from its role in classical mathematics. Unwilling—indeed unable, because of his education—to let mathematics generate its own meaning, the classical mathematician looks to formalism, with its emphasis on consistency (either relative, empirical, or absolute), rather than meaning, for philosophical relief. For the constructivist, formalism is not a philosophical out; rather it has a deeper significance, peculiar to the constructivist point of view. Informal constructive mathematics is concerned with the communication of algorithms, with enough precision to be intelligible to the mathematical community at large. Formal constructive mathematics is concerned with the communication of algorithms with enough precision to be intelligible to machines."
>
> *... Bishop:* [**Bi18**], *pp.1-2.*

In this investigation I shall, therefore, seek to establish such complementarity, culminating in the Provability Theorem for PA in Chapter 10, which bridges formal arithmetic provability and interpreted, *evidence-based*, arithmetic truth.

I shall then investigate some of its consequences, and how these relate to various unsettling philosophical issues, by identifying and removing the root of

[1]We note that Bishop erroneously (see Corollary 9.11) treats the law of the excluded middle—ergo the classical first-order logic FOL in which this law is a theorem—as 'nonconstructive'.

a critical ambiguity—essentially an ambiguity in Brouwer-Heyting-Kolmogorov realizability (as highlighted in Chapter 21)—which seems to inhibit recognition of the complementary roles of classical and constructive mathematics.

It is an ambiguity with far-reaching ramifications which, I argue, tolerates unsustainable beliefs whose illusory 'self-evidentiary' appeal (for instance, the 'obviousness' of an isomorphism between the structure of the natural numbers and that of the finite ordinals in Goodstein's *curious* argumentation in Chapter 22) could reasonably be viewed as owing more, perhaps, to psychological factors than to mathematical ones—as Bauer ([**Ba16**]) insightfully suggests in another context.

From a psychological perspective, I would thus argue (§23.2) that, both qualitatively and quantitatively, any piece of information (i.e., the perceived content of a well-defined declarative sentence) that we treat as a 'fact'[2] is necessarily associated with a suitably-defined truth assignation which must fall into one or more of the following three categories:

(a) information that we zealotly *believe* to be 'true' in an, absolute, Platonic sense, and have in common with others holding similar *beliefs* zealotly;

(b) information that we prophetically *hold* to be 'true'—short of Platonic *belief*[3]—since it *can* be treated as *self-evident*, and have in common with others who also *hold* it as similarly *self-evident*;

(c) information that we scientifically *agree* to *define* as 'true' on the basis of an *evidence-based convention*, and have in common with others who accept the same *convention* for assigning *truth* values to such assertions.

Clearly the three categories of information have associated truth assignations with increasing degrees of objective (i.e., on the basis of *evidence-based* reasoning) accountability that must, in turn, influence the perspective—and understanding (in the cognitive sense of §43.1)—of whoever is exposed to a particular category at a particular moment of time.

In mathematics, for instance, Platonists who hold even axioms which are not immediately self-evident as 'true' in some absolute sense—such as Gödel ([**Go51**]) and Saharon Shelah ([**She91**])—might be categorised as accepting all three of (a), (b) and (c) as definitive; those who hold axioms as reasonable hypotheses only if self-evident—such as Hilbert ([**Hi27**])—as holding only (b) and (c) as definitive; and those who hold axioms as necessarily *evidence-based* propositions—such as Brouwer ([**Br13**])—as accepting only (c) as definitive.

In the first case, it is obvious that contradictions between two intelligences, that arise solely on the basis of conflicting beliefs—such as, for instance, the classical debate between 'creationists' and 'evolutionists'[4] or, currently, that between proponents of the theory of 'alternative facts' and those of 'scientific facts', as addressed by physicists Steven Vigdor and Tim Londergan in their June 27, 2017,

---

[2]For the purposes of this investigation, we ignore the nuances involved in such a concept as detailed, for instance, in [**SP10**].

[3]But see also, for instance, §C.2.

[4]Typical of a phenomena whose topical dimensions are insightfully—and sensitively—addressed by Harvey Whitehouse for a lay audience—from the perspective of Cognition and Evolutionary Anthropolgy—in an interview in [**Gal18**].

blogpost '*Debunking Denial: The War Against Facts*'—cannot yield any productive insight on the nature of the contradiction.

Although not obvious, it is the second case—of contradictions between two intelligences that arise on the basis of conflicting 'reasonability', such as:

- the perceived conflict detailed in Chapter 4 between Hilbert's and Brouwer's interpretation of quantification; or
- the perceived conflict detailed in §9.3 between Hilbert and Poincaré on the finitary interpretability of the axiom schema of induction of the first-order Peano Arithmetic PA; or
- the perceived conflict detailed in Chapter 28 between Bohr and Einstein on whether the mathematical representation of some fundamental laws of nature can only be expressed in terms of functions that are essentially unpredictable, or whether all the laws of nature can be expressed in terms of functions that are essentially deterministic;

which yields the most productive insight on the nature of the contradiction.

Reason: Such conflicts compel us to address the element of implicit subjectivity in the individual conceptual metaphors (see [**LR00**]) underlying the contradictory perspectives that, then, motivates us to seek (c) for an appropriate resolution of the corresponding contradiction, as in the case of:

- the argument in [**An15**] that Hilbert's and Brouwer's interpretations of quantification are complementary and not contradictory; and
- the dissolving of the Hilbert-Poincaré debate by virtue of Lemma 9.4 and Corollary 9.11;
- the dissolving of the Bohr-Einstein debate by the argument in [**An13**] and [**An15a**] that any mathematical representation of a law of nature is necessarily expressed in terms of functions that are algorithmically *verifiable*—hence *deterministic*—but that such functions need not be algorithmically *computable*—and therefore *predictable*.

The third case (c) is thus the holy grail of communication (critically so in the search for extra-terrestrial intelligence—see §23.4)—one that admits unambiguous and effective communication without contradiction; and which is the focus of this investigation.

Specifically, I shall attempt to address, from the perspective of *stringently* constructive—in the sense (see Chapter 5) of *evidence-based*—mathematics, some grey areas in the standard interpretations of the formal reasoning and conclusions of classical first order theory:

— based primarily on the seminal works of Cantor, Hilbert, Brouwer, Gödel, Tarski, and Turing,

— which appeal to Tarski's Theorem (see §8.1) that arithmetical truth cannot be defined algorithmically, and

— which seem to implicitly, but misleadingly, assume that:

- The satisfiability and truth—as defined by Tarski—of the propositions of any formal mathematical language which is rich enough to express the arithmetic of the natural numbers is necessarily subjective,
- in the sense of being essentially unverifiable constructively,
- under *any* well-defined interpretation of the language.

However, in Chapter 7 I review the *evidence-based* definition of algorithmically *verifiable* arithmetical truth introduced in §5.1, and show that it is such 'verifiability' (corresponding to Hilbert's concept of 'verifiability' as analysed in §15.4) that actually underpins the classically standard—but what can now be seen to be a *weak*—interpretation $\boldsymbol{M}$ of PA (as introduced in [**An12**] and developed in [**An16**]).

I note as its immediate consequence that PA is *weakly* consistent (Theorem 7.7), and that Hilbert's and Bernays' 'informal' proof of the consistency of arithmetic in the *Grundlagen der Mathematik*—as analysed in [**SN01**] (see §15.4)—can be viewed as essentially outlining a proof of Theorem 7.7.

I then show in §8.1 the—hitherto unsuspected and, as I show in Chapter 14, also far-reaching—consequence (Theorem 8.5) that PA is not ω-consistent (an independent proof of which is given in Corollary 11.6).

Moreover, in Chapter 9 I detail an *evidence-based* definition of algorithmically *computable* arithmetical truth under a *strong* finitary interpretation $\boldsymbol{B}$ of PA, which not only establishes the consistency of PA *finitarily* (as sought by Hilbert in the second of his Millennium problems in [**Hi00**]), but establishes PA as a language of unambiguous expression and effective communication (in the sense of §23.1) for the physical sciences (as considered briefly in Chapter 27).

In other words, I conclude (from the Provability Theorem 10.2) that although a set theory such as ZF may be *the* appropriate language for the symbolic expression of Lakoff and Núñez's 'conceptual metaphors', by which an individual's 'embodied mind brings mathematics into being' (see [**LR00**]), it is the *strong* finitary interpretation of the first-order Peano Arithmetic PA (see Theorem 9.7) that makes PA a stronger contender for the role of a *lingua franca* of adequate expression and effective communication of such 'conceptual metaphors' in contemporary mathematics and its foundations.

Reason: PA allows us to bridge arithmetic provability and arithmetic computability, in the sense that a PA formula $[F(x)]$ is PA-provable if, and only if, $[F(x)]$ is algorithmically computable as true in $\mathbb{N}$ under $\boldsymbol{B}$ (Chapter 10).

Before considering the wider implications of the Provability Theorem 10.2, and to place this investigation in perspective against current and classical approaches towards determining the strictures that a formal system must embrace in order to be considered constructive, I review:

- first, from an *evidence-based* perspective, in Chapters 13 and 14, Andrej Bauer's unusual, psychologically oriented, recent survey of constructive mathematics; and,
- second, in Chapter 15, David Hilbert's, Paul Bernays', and Kurt Gödel's classical attempts to ground mathematical reasoning on a sound, finitary, footing as conceived originally in Hilbert's Programme which, for better

or worse, have not been pursued as aggressively after around 1939, when these three influential logicians apparently diluted their original vision as a result of the perceived (but misleading, as we establish in Corollary 11.9) implications of Gödel's unexpected 'undecidability theorems' in 1931.

I then aim in Chapters 16 to 29 of this investigation at a narrow analysis, rather than at a broad review, of some immediate consequences for constructive mathematics of the Provability Theorem 10.2—and of the significance of *evidence-based* reasoning for some grey areas in the foundations of classical logic, mathematics, philosophy and the physical sciences—from an applied, rather than abstract, perspective.

Amongst the more unsuspected—and startling—consequences of *evidence-based* reasoning for the applied sciences is the possibility of physical phenomena which are mathematically describable by Cauchy sequences where, however, the limiting behaviour of the phenomena need not correspond to the mathematical limit of the sequence (§24.3)!

In other words, for natural phenomena, the essential completability of metric spaces that obey Cauchy convergence—a bedrock of our mathematical representation of the real numbers used to describe physical phenomena—may, in the absence of *evidence* that such a limit either exists or must be accepted as existing, be merely a psychologically comforting mathematical myth (see §25) which lulls our psyche into an illusory sense of epistemological security within our intellectual comfort zone.

An equally unexpected consequence of *evidence-based* reasoning for the mathematical sciences is that explicit recognition of algorithmically *verifiable* number-theoretic functions which are not algorithmically *computable* admits—contrary to conventional wisdom—a proof that the prime divisors of an integer are mutually independent (Theorem 31.9; also, independently, Corollary 36.11).

The result has significant implications:

- for the $P\ v\ NP$ problem in Computational Complexity (Chapter 30.1),
  - since it immediately implies that factorisation is not polynomial-time (Corollary 32.5);
- for the *non-heuristical* estimation of prime counting functions in Number Theory (Chapter 33)—such as those that estimate:
  - the number $\pi(n)$ of primes less than a given integer n (Lemmas 37.5 and 37.8);
  - the number of primes in arithmetical progressions (Theorem 38.11);
  - the number of twin primes (Theorem 39.9).

An interesting consequence of *evidence-based* reasoning for cognitive science, which emerges from Lakoff and Núñez's analysis in [**LR00**], is (Thesis 44.1) that all the abstract mathematical concepts dissected in Chapters 5 to 14 of [**LR00**]—including concepts involving 'potential' and 'actual' infinities—can be viewed as conceptual metaphors which are expressible (if treated as Carnap's *explicandum*) in the language of the first-order Set Theory ZFC; a perspective that would lend legitimacy to conventional wisdom which—as addressed in Chapter 18—is that all 'mathematical' concepts are definable (even if only debatably unambiguously) in ZFC.

In conclusion, it may be pertinent to emphasise that the roots of *all* the ambiguities sought to be addressed in this investigation lie in the unquestioned, and *untenable* (Corollary 15.11) assumption that Aristotle's particularisation is valid over infinite domains.

Aristotle's particularisation is defined (Definition 3.1) as the postulation that, in any formal language L which subsumes the first-order logic FOL, the L-formula '$[\neg(\forall x)\neg F(x)]$—also denoted by $[(\exists x)F(x)]$—is provable in L' can *unrestrictedly* be interpreted as the assertion 'There exists an *unspecified* object a such that $F'(a)$ is true under any well-defined interpretation I of L', where $F'(x)$ is the interpretation of $[F(x)]$ under I.

Following Hilbert's formalisation of it in terms of his ε-operator in [**Hi25**], the assumption—as noted in §3.1 (footnote)—has been subsequently sanctified by prevailing wisdom in published literature and textbooks at such an early stage of any classical mathematical curriculum, and planted so deeply into students' minds, that thereafter most cannot even detect its presence—let alone need for its justification—in a proof sequence!

It would not be unreasonable to conclude that such a sub-conscious assumption, especially where provably invalid (see, for instance, Corollary 15.11), has continued for over ninety years to unconsciously dictate, mislead, and so limit the perspective of not only active, but also emerging, scientists of any ilk who have depended upon classical mathematics for providing a language of adequate representation and effective communication for their abstract concepts (in the sense of Chapter 23).

Since faith in the assumption can, also not unreasonably, be viewed as rooted in an *unreasonably* persisting influence of Hilbert's finitism (see §15.9 and §15.2), and his, apparently unquestioning, belief in the validity of Aristotle's particularisation over infinite domains—which he sought to formalise through his ε-operator (see §4.1)—the restricted availability of Hilbert's consolidated argumentation on finitism in only non-English editions of the *Grundlagen der Mathmatik* has been a handicap to those—such as the author—unfamiliar with the language of such editions.

Moreover, as the *Grundlagen* has apparently been considered *passé* for some time now, Professor Claus-Peter Wirth's labour of love in a better-late-than-never attempt to produce a definitive bi-lingual German-English translation [**HB34**] of the *Grundlagen* under the auspices of *The Hilbert Bernays Project* is all the more commendable, and deserves all encouragement and financial support of the academic community in ensuring that the Project overcomes its intermittent stoppages due to lack of resources and facilities, and that both Volume I—Preface and Sections 1-7 already reported as completed—and Volume II are brought to print.

Bhupinder Singh Anand
Mumbai
2^{nd} July 2018

CHAPTER 1

Overview

Please forget everything you have learned [sic] in school; for you haven't learned it. Please keep in mind at all times the corresponding portions of your school curriculum; for you haven't actually forgotten them. . . . my daughters have been studying (chemistry) for several semesters, think they have learned differential and integral calculus at school, and yet even today dont know why '$x.y = y.x$' is true.

. . . *Professor Yehezkel-Edmund Landau: (*[**La29**]*, Preface to the student).*

This investigation adopts, extends, and seeks to consider some constructive consequences—for the foundations of mathematics, logic, philosophy, and the physical sciences—of, the *evidence-based* perspective towards 'provability' and 'truth' introduced in the paper [**An16**], '*The Truth Assignments That Differentiate Human Reasoning From Mechanistic Reasoning: The Evidence-Based Argument for Lucas' Gödelian Thesis*'.

The paper appeared in the December 2016 issue of *Cognitive Systems Research*, and addressed the philosophical challenge[1] that arises when an intelligence—whether human or mechanistic—accepts arithmetical propositions as true under an interpretation—either axiomatically or on the basis of subjective self-evidence—*without* any specified methodology for evidencing such acceptance in the sense of Chetan Murthy and Martin Löb:

> "It is by now folklore . . . that one can view the *values* of a simple functional language as specifying *evidence* for propositions in a constructive logic . . . ".
>
> . . . *Chetan. R. Murthy:* [**Mu91**], §*1 Introduction.*
>
> "Intuitively we require that for each event-describing sentence, $\phi_{o^{\iota}} n_{\iota}$ say (i.e. the concrete object denoted by n_{ι} exhibits the property expressed by $\phi_{o^{\iota}}$), there shall be an algorithm (depending on **I**, i.e. M^*) to decide the truth or falsity of that sentence."
>
> . . . *Martin H Löb:* [**Lob59**]*, p.165.*

1.1. Part 1: *Evidence-based* reasoning

We attempt to fill this lacuna by defining:

Definition 1.1 (Evidence-based reasoning in Arithmetic). Evidence-based reasoning accepts arithmetical propositions as true under an interpretation if, and only if, there is some specified methodology for objectively *evidencing* such acceptance.

We then argue (in Chapters 3 and §4) that, from the *evidence-based* perspective of [**An16**], classical philosophies (e.g., that of Kurt Gödel in his seminal 1931 paper

[1]For a brief recent review of such challenges, see [**Fe06**], [**Fe08**]; also [**An04**] and Rodrigo Freire's informal essay on '*Interpretation and Truth in Cantorian Set Theory*'.

on formally undecidable arithmetical propositions [**Go31**]) which admit—either explicitly or implicitly—David Hilbert's formal, ε-operator based, definitions of quantification ([**Hi27**]; see also §4.1) can be labelled 'theistic', since they implicitly believe—without providing *evidence-based* criteria for interpreting quantification constructively—both that:

(*a*) the standard first-order logic FOL[2] is consistent; and

(*b*) Aristotle's particularisation (see Definition 3.1)—which we take as the postulation that the FOL formula '$[\neg\forall\neg F(x)]$'[3] can *unrestrictedly* be interpreted as 'there exists an *unspecified* instantiation of $F^*(x)$'—holds under any interpretation of FOL.

- The significance of the qualification 'unrestrictedly' is that it does not admit the—hitherto unsuspected—possibility (see §11.6) that an *unspecified* instantiation may sometimes be *unspecifiable*— in the sense of Definition 4.1 and Theorem 11.10—within the parameters of some formal system that subsumes FOL.

In sharp contrast, simply constructive philosophies (such as, for instance, Andrej Bauer's perspective of constructive mathematics in [**Ba16**]) which admit—either explicitly or implicitly—L. E. J. Brouwer's philosophy of Intuitionism, can be labelled 'atheistic' because they—also without providing *evidence-based* criteria for interpreting quantification constructively:

(*i*) deny the *belief* that FOL is consistent (since they deny the Law of The Excluded Middle LEM, which is a theorem of FOL), and:

(*ii*) deny the *belief* that Aristotle's particularisation holds under any interpretation of FOL that has an infinite domain.

However, we adopt what may be labelled a finitary, i.e. *evidence-based*[4], 'agnostic' perspective which will establish that:

(*1*) FOL is finitarily consistent (Corollary 9.11);

(*2*) although, if Aristotle's particularisation holds in an interpretation of FOL then LEM must also hold in the interpretation (since LEM is a theorem of FOL), the converse is not true, i.e., LEM does not entail Aristotle's particularisation (see §14.1);

(*3*) Aristotle's particularisation does not hold under any interpretation of FOL that has an infinite domain (an immediate consequence of Corollary 15.11).

[2]For purposes of this investigation we take FOL to be a first order predicate calculus such as the formal system K defined in [**Me64**], p.57.

[3]*Notation*: Following the practice briefly used by Gödel in his informal sketch of the main ideas of his formal proof of formally undecidable arithmetical propositions ([**Go31**], p.8, fn.13), we shall use square brackets to differentiate between a symbolic expression—such as $[(\exists x)P(x)]$—which denotes a formula of a formal language L (treated as an interpreted string without any associated meaning), and the symbolic expression—such as $(\exists x)P^*(x)$—that denotes its meaning under a well-defined interpretation; we find such differentiation useful in order to avoid the possibility of confusion between the two, particularly when (as is not uncommon) the same symbolic expressions are used to denote—or are common to—the two.

[4]*Notation*: In the rest of this investigation we shall treat the terms 'finitary' and 'evidence-based' as synonymous.

Our argument (in §3.4) is that:

- Brouwerian atheism is merely a restricted perspective within the finitary agnostic perspective; whilst
- Hilbertian theism contradicts the finitary agnostic perspective.

This conclusion reflects the fact (see Chapter 5; cf. [**An16**], §3) that Tarski's classic definitions[5] permit an intelligence—whether human or mechanistic—to admit *finitary* definitions of the satisfaction and truth of the *atomic* formulas of the first-order Peano Arithmetic PA over the domain $\mathbb{N}$ of the natural numbers in two, essentially different, ways:

(A) in terms of *weak* algorithmic verifiabilty (Definition 5.2; cf. [**An16**], Definition 1, p.37; compare also with Definition 21.1); and:

(B) in terms of *strong* algorithmic computability (Definition 5.2; cf. [**An16**], Definition 2, p.37; compare also with Definition 21.2).

We then note (in Chapter 6) how the two definitions correspond to two distinctly different—not necessarily *evidence-based*—assignments of satisfaction and truth, T_M and T_B respectively, to the *compound* formulas of PA over the domain $\mathbb{N}$ of the natural numbers.

1.2. Part 2: *Evidence-based* interpretations of PA

We further note (in Chapters 7 and 9 respectively) that the PA axioms interpret as true over $\mathbb{N}$, and that the PA rules of inference preserve truth over $\mathbb{N}$, under both T_M and T_B.

We conclude that:

(α) If we assume the satisfaction and truth of the *compound* formulas of PA are always *non-finitarily* decidable under T_M, then this assignment corresponds (Chapter 7) to the *weak* standard[6] interpretation $\boldsymbol{M}$ of PA over the domain $\mathbb{N}$; from which we may *constructively*, but not *finitarily*, conclude that PA is *weakly* consistent (Theorem 7.7).

We note, moreover, that Hilbert's and Bernays' 'informal' proof of the consistency of arithmetic in the *Grundlagen der Mathematik*—as analysed in [**SN01**] (see §15.4)—can be viewed as essentially outlining a proof of Theorem 7.7.

and that:

(β) The satisfaction and truth of the *compound* formulas of PA are always *finitarily* decidable under T_B, and so the assignment corresponds (Chapter 9) to a *strong* finitary interpretation $\boldsymbol{B}$ of PA over the domain $\mathbb{N}$; from which, however, we may *finitarily* conclude that (as sought by Hilbert in the second of his Millennium problems in [**Hi00**]) PA is *strongly* consistent (Theorem 9.10; cf. [**An16**], Theorem 6.8, p.41).

---

[5]For standardisation and convenience of expression, we follow the formal exposition of Tarski's definitions given in [**Me64**], p.50 (see §A, Appendix A); however, see also [**Ta35**] and [**Ho01**] for an explanatory exposition.

[6]As defined in §A, Appendix A; see also [**Me64**], p.49

We note that Lemma 9.4 and Corollary 9.11 appear to dissolve the Poincaré-Hilbert debate ([**Hi27**], p.472; also [**Br13**], p.59; [**We27**], p.482; [**Pa71**], p.502-503) since:

(i) the algorithmically *verifiable*, non-finitary, *weak* standard interpretation $\boldsymbol{M}$ of PA validates Poincaré's argument that the PA Axiom Schema of Finite Induction could not be justified finitarily (i.e., with respect to algorithmic computability) under the classical *weak* standard interpretation of arithmetic;

whilst:

(ii) the algorithmically *computable*, finitary, *strong* interpretation $\boldsymbol{B}$ of PA validates Hilbert's belief that a finitary justification of the Axiom Schema was possible under some *strong* finitary interpretation of an arithmetic such as PA.

We then note (in Chapter 10) how this yields a Provability Theorem for PA (Theorem 10.2; cf. [**An16**], Theorem 7.1, p.41) which formally corresponds arithmetical provability and arithmetical truth.

We note that this establishes PA as a language of unambiguous expression and effective communication (in the sense of §23.1) for the physical sciences (as considered briefly in Chapter 27).

1.3. Part 3: *Evidence-based* reasoning and the Church-Turing thesis

We conclude (in Chapters 11 and 12) some—also hitherto unsuspected, and seemingly heretical—consequences of the Provability Theorem for PA (Theorem 10.2 for *evidence-based* mathematics such as:

- PA is categorical with respect to algorithmic computability (Corollary 11.1);
- There are no formally undecidable arithmetical sentences (Corollary 11.9);
- The appropriate inference to be drawn from Gödel's 1931 paper on undecidable arithmetical propositions is that we can define PA formulas which—under interpretation—are algorithmically *verifiable* as always true over $\mathbb{N}$, but not algorithmically *computable* as always true over $\mathbb{N}$ (Corollary 11.5);
- PA is not ω-consistent (Corollary 11.6);
- It is always possible to determine whether a Turing machine will halt or not when computing any partial recursive function F (Theorem 12.6);
- The classical Church-Turing thesis is false (Corollary 12.8).

1.4. Part 4: *Evidence-based* reasoning and constructive mathematics

We further identify (in Chapters 13 to §20)—from the *evidence-based* perspective of [**An16**]—some grey areas in constructive mathematics, based specifically on logician Andrej Bauer's novel, and remarkably candid, psychological approach (in

[**Ba16**]) to the understanding of constructive mathematics through the five stages of: *Denial*, *Anger*, *Bargaining*, *Depression* and *Acceptance*.

We specifically address the necessity of some critical, self-imposed, constraints in—and their consequences for—Bauer's perspective of constructive mathematics (BPCM).

In particular, we note that the most noteworthy feature of BPCM is the, albeit tacit, acknowledgment that a major constraint of constructive mathematics—denial or acceptance of the law of excluded middle (LEM)—is an *optional* belief that is open to *persuasion*!

We endorse this view and—against the backdrop in Chapter 15.1 of the wider classical efforts to ground mathematical reasoning on only sound, finitary argumentation as envisaged in Hilbert's Programme—conclude (in Chapter 21) that such constraints merely reflect some commonly-held, albeit illusory, perceptions of an uncritically assumed mutual inconsistency between:

- Classical mathematical philosophies, and
- Constructive mathematical philosophies,

vis à vis their differing perspectives of mathematical truth and mathematical ontologies.

We argue, moreover, that such illusions reflect as much their *tacit* endorsement of uncritically-held, *faith-based*, beliefs, as their failure to explicitly—and unambiguously—demand *evidence-based* definitions of the relations between a language and the logic that is necessary to assign unequivocal truth-values to the propositions of the language.

We show how eliminating *faith-based* beliefs :

- Admits formal, *evidence-based*, definitions:
  - of a *constructively well-defined logic* of a formal language (Definition 21.5);
  - of *constructive mathematics* (Definition 21.6); and
  - of a *constructively well-defined model* of such a language (Definition 21.7);
- Eliminates the *self-imposed* limitations—chiefly the consequences of denying the Law of the Excluded Middle—within which constructive mathematics *strains* to justify its finitist rigour;
- Entails some far-reaching and unexpected consequences which challenge specific conventional wisdom that has, hitherto, been accepted as almost self-evident; consequences such as:
  - Rosser's implicit assumption of his Rule C in his proof of undecidability in [**Ro36**] is equivalent to Gödel's assumption of ω-consistency in [**Go31**] (§15.6);
  - Cohen's postulation of an *unspecified* element in his forced model 'N' of ZF in [**Co63**] is a stronger postulation than the Axiom of Choice (§18.2);

- $\aleph_0 \longleftrightarrow 2^{\aleph_0}$ in constructive mathematics (§19.3);
- Conventional arguments (e.g., [**Ka91**]) for non-standard structures under any interpretations of PA violate *evidence-based* reasoning (§20.1).

1.5. Part 5: *Evidence-based* reasoning and logic

In Chapters 21 to 23 we consider the significance of *evidence-based* reasoning for some grey areas in the foundations of logic, mathematics and philosophy, where:

- We highlight (in Chapter 21) an ambiguity that is implicit in the rules—such as those of Brouwer-Heyting-Kolmogorov realizability—which seek to constructively assign unique truth values to the quantified propositions of a mathematical language.
  - We show how removing the ambiguity allows us to formally define constructive mathematics and its goal (§21.2) by defining a finite set λ of rules as a constructively *well-defined logic* of a formal mathematical language $\mathcal{L}$ if, and only if, λ assigns unique, *evidence-based*, truth-values:
    (a) Of provability/unprovability to the formulas of $\mathcal{L}$; and
    (b) Of truth/falsity to the sentences of the Theory $T(\mathcal{U})$ which is defined semantically by the λ-interpretation of $\mathcal{L}$ over a given structure $\mathcal{U}$ that may, or may not, be constructively well-defined;

    such that

    (c) The provable formulas interpret as true in $T(\mathcal{U})$.
  - We then show that PA has a constructively well-defined logic (Theorem 21.17).
- We further challenge (in Chapter 22) specific conventional wisdom that has, hitherto, been accepted as almost self-evident, and consider some consequences of *evidence-based* reasoning such as:
  - The subsystem ACA_0 of second-order arithmetic is not a conservative extension of PA (Theorem 22.1);
  - Goodstein's sequence $G_o(m_o)$ over the finite ordinals in any putative model $\mathbb{M}$ of ACA_0 *terminates* with respect to the ordinal inequality '$>_o$' even if Goodstein's sequence $G(m)$ over the natural numbers *does not terminate* with respect to the natural number inequality '$>$' in $\mathbb{M}$ (Theorem 22.3).

1.6. Part 6: *Evidence-based* reasoning and effective communication

In Chapter 23 we briefly consider the significance of *evidence-based* reasoning for some inter-disciplinary philosophical issues such as:

- Is there a universal language that admits unambiguous and effective communication without contradiction (Query 23.1)?

- Can we responsibly seek communication with an extra-terrestrial intelligence actively (as in the 1974 Aricebo message) or is there a logically sound possibility that we may be initiating a process which could imperil humankind at a future date (Query 23.4)?
- How does the human brain address contradiction?

and argue that:

- We can only communicate with an essentially different form of extra-terrestrial intelligence in a platform-independent language of a mechanistically reasoning artificial intelligence (Premise 23.6);
- Nature is not malicious and so, for an ETI to be malevolent towards us, they must perceive us as an essentially different form of intelligence that threatens their survival merely on the basis of our communications (Premise 23.7);
- The language of algorithmically computable functions and relations is platform-independent (Premise 23.8);
- All natural phenomena which are observable by human intelligence, and which can be modelled by deterministic algorithms, are interpretable isomorphically by an extra-terrestrial intelligence (Premise 23.9);
- Every deterministic algorithm can be formally expressed by some formula of a first-order Peano Arithmetic, PA (Lemma 23.12);
- Any two mechanical intelligences will interpret the satisfaction, and truth, of the formulas of PA under a constructively well-defined interpretation of PA in precisely the same way without contradiction (§11.4, Corollary 11.1);
- Whilst human reasoning (and, presumably, other organic intelligences) can accommodate algorithmically *computable* truths which do not admit contradiction, it can also accommodate algorithmically *verifiable*, but not algorithmically computable, truths that admit contradictory statements without inviting inconsistency *until* it can be factually determined (by events that lie outside the database of the reasoning at any moment[7]) which of the two statements is to be treated as consistent with, and added to, the existing set of algorithmically *verifiable* truths, and which is not; whence:
  - all genuine contradictions—i.e., those which do not reflect contradictions in existing truth assignations—imply only a lack of sufficient knowledge (as argued by Einstein, Podolsky and Rosen in [**EPR35**]) within a system for assigning a truth assignment consistently (§23.11).
- We show (in Chapter 24) that the semantic and logical paradoxes—as also the seeming paradoxes associated with 'fractal' constructions such as the Cantor ternary set (§24.3)—seem to arise out of an attempt to ask of a language more than it is designed to deliver.

[7]Such as, for example, under the *weak* classical 'standard' interpretation of the first-order Peano Arithmetic PA defined in Chapter 7.

– For instance, we show (in §24.4 and §24.5) that—and why—the numerical values of some algorithmically computable Cauchy sequences may need to be treated as formally specifiable, first-order, non-terminating processes which cannot be uniquely identified with a putative 'Cauchy limit' *without* limiting the ability of such sequences to model phase-changing physical phenomena faithfully.

1.7. Part 7: *Evidence-based* reasoning and cosmology

We then illustrate in Chapter 25 the significance of §24.4 and §24.5 for cosmology by arguing that:

> (Thesis 25.3) The perceived barriers that inhibit mathematical modelling of a cyclic universe, which admits broken symmetries, dark energy, and an ever-expanding multiverse, in a mathematical language seeking unambiguous communication are illusory; they arise out of an attempt to ask of the language selected for such representation more than the language is designed to deliver.

In Chapter 26 we highlight the importance for cosmology of justifying the increasing abstractness of mathematical reasoning—and avoiding the consequent dangers of a gradual diminishing of its utility to societal imperatives—by insisting that such reasoning be *evidence-based* in its references to reality.

1.8. Part 8: *Evidence-based* reasoning and quantum physics

In Chapters 27 to 29 we illustrate the significance of such *evidence-based* reasoning for the physical sciences by briefly speculating upon some plausible consequences, such as:

- Lucas' Gödelian argument is validated if the assignment T_M can be treated as circumscribing the ambit of human reasoning about 'true' arithmetical propositions, and the assignment T_B as circumscribing the ambit of mechanistic reasoning about 'true' arithmetical propositions (Theorem 27.1);
- The concept of infinity is an emergent feature of any Turing-machine based mechanical intelligence founded on the first-order Peano Arithmetic PA (Thesis 27.4);
- The discovery and formulation of the laws of quantum physics lies within the algorithmically computable logic and reasoning of a mechanical intelligence whose logic is circumscribed by the first-order Peano Arithmetic (Thesis 27.5);
- Constructive mathematics can model a deterministic universe that is irreducibly probabilistic (§28.1).
- The paradoxical element which surfaced as a result of the *EPR* argument (due to the perceived conflict implied by Bell's inequality between the, seemingly essential, non-locality required by current interpretations of Quantum Mechanics, and the essential locality required by current interpretations of Classical Mechanics) may reflect merely lack of recognition

that any mathematical language which can adequately express and effectively communicate the laws of nature may be consistent under two, essentially different but complementary and not contradictory, logics for assigning truth values to the propositions of the language, such that the latter are capable of representing—as deterministic—the unpredictable characteristics of quantum behaviour (§28.1).

- The anomalous philosophical issues underlying some current concepts of quantum phenomena, such as:
  - — Indeterminacy (§29.1),
  - — Fundamental dimensionless constants (Thesis 29.1),
  - — Bell's inequalities and the *EPR* paradox (§29.3),
  - — Uncertainty (Thesis 29.3),
  - — Conjugate properties (Thesis 29.4),
  - — Entanglement (Thesis 29.5),
  - — Schrödinger's cat paradox (§29.14),

  dissolve if the Laws of Classical Mechanics are expressible formally as algorithmically computable (hence deterministic and predictable) functions and relations; whilst the Laws of Quantum Mechanics are expressible formally only as algorithmically *verifiable*, but not algorithmically *computable* (hence *deterministic* but not *predictable*), functions and relations (Chapter 29: Theses 29.1 to 29.5).

1.9. Part 9: *Evidence-based* reasoning and computational complexity

In Chapters 30.1 to 32 we highlight the surprising significance of *evidence-based* reasoning—and of the differentiation between algorithmically *verifiable* and algorithmically *computable* number-theoretic functions—for Computational Complexity by showing that:

- Conventional wisdom appears to *unreasonably* accept as definitive the patently counter-intuitive conclusion (addressed in Chapter 30.1) that whether or not a prime p divides an integer n is *not* independent of whether or not a prime $q \neq p$ divides the integer n;
  - Such a perspective is '*unreasonable*', since it appears based on seemingly self-imposed barriers that reflect, and are peculiar to, only the argument that:
    - ∗ There is no deterministic algorithm that, for any given n, and any given prime $p \geq 2$, will evidence that the probability $\mathbb{P}(p \mid n)$ that p divides n is $\frac{1}{p}$, and the probability $\mathbb{P}(p \not| n)$ that p does not divide n is $1 - \frac{1}{p}$ (Theorem 30.11).
  - Such a perspective does not consider the possibility that there can be algorithmically *verifiable* number-theoretic functions which are not algorithmically *computable*; and that:

* For any given n, there is a deterministic algorithm that, given any prime $p \geq 2$, will evidence that the probability $\mathbb{P}(p \mid n)$ that p divides n is $\frac{1}{p}$, and the probability $\mathbb{P}(p \not| n)$ that p does not divide n is $1 - \frac{1}{p}$ (Theorem 30.12).

Admitting the above distinction between algorithmically *verifiable* and algorithmically *computable* number-theoretic functions now allows us to conclude—contrary to conventional wisdom—that:

- The prime divisors of an integer n mutually independent (Theorem 31.9);

which allows us to conclude further that:

- *Integer Factorising* cannot be polynomial time (Theorem 32.5).

1.10. Part 10: *Evidence-based* reasoning and the theory of numbers

In Chapters 33 to 42 we highlight the, equally surprising, significance of *evidence-based* reasoning—and of the differentiation between algorithmically *verifiable* and algorithmically *computable* number-theoretic functions—for the Theory of Numbers by showing that:

- Conventional number theory wisdom appears to be that the distribution of primes suggested by the Prime Number Theorem, $\pi(n) \sim \frac{n}{log_e n}$, is such that the probability $\mathbb{P}(n \in \{p\})$ of an integer n being a prime p can *only* be *heuristically* estimated as $\frac{1}{log_e n}$; and is also *not capable* of being well-defined statistically independently of the Theorem.
  - Moreover—whilst conceding that the *heuristic* probability of an integer n being prime *could* also be naïvely assumed as $\prod_{i=1}^{\sqrt{n}}(1 - \frac{1}{p_i})$—such a perspective seems to argue against such naïvety, by concluding (*erroneously*, as we show in §37.1, Lemma 37.5) that the number $\pi(n)$ of primes less than or equal to n suggested by such probability would then be approximated *erroneously* by the prime counting function:
    $\pi_H(n) = \sum_{j=1}^{n} \prod_{i=1}^{\pi(\sqrt{n})}(1 - \frac{1}{p_i}) = n.\prod_{i=1}^{\pi(\sqrt{n})}(1 - \frac{1}{p_i}) \sim \frac{2.e^{-\gamma}n}{log_e n}$.
- From an *evidence-based* perspective, however, such reasoning could raise an *illusory* barrier in seeking *non-heuristic* estimations of $\pi(n)$—and possibly of $|Li(x) - \pi(x)|$—if, as in the case of Lemma 33.2, the following theorem too is accepted as unsurpassable:
  - There is no algorithm which, for any given n, will allow us to conclude that the probability $\mathbb{P}(n \in \{p\})$ of determining that n is prime is $\prod_{i=1}^{\pi(\sqrt{n})}(1 - \frac{1}{p_i})$ (Theorem 34.2).
- *Illusory*, because it follows immediately from Theorem 32.1 that:
  - For any given n, there is an algorithm which will allow us to conclude that the probability $\mathbb{P}(n \in \{p\})$ of determining that n is prime is $\prod_{i=1}^{\pi(\sqrt{n})}(1 - \frac{1}{p_i})$ (Theorem 34.3).

The significance of Theorem 34.3 is that, by considering the asymptotic density (see Chapter 37) of the set of all integers that are not divisible by the first k primes $p_1, p_2, \ldots, p_k$ we shall show that the expected number of such integers in any interval of length $(p^2_{\pi(\sqrt{n})+1} - p^2_{\pi(\sqrt{n})})$ is:

$$\{(p^2_{\pi(\sqrt{n})+1} - p^2_{\pi(\sqrt{n})}) \prod_{i=1}^{k}(1 - \frac{1}{p_i})\}.$$

This then allows us to define and estimate various prime counting functions *non-heuristically*, such as:

(a) For each n, the expected number of primes in the interval $(1,\ n)$ is (as illustrated in §35, Fig.1):

$\pi_H(n) = n \prod_{i=1}^{\pi(\sqrt{n})}(1 - \frac{1}{p_i})$.

- The number $\pi(n)$ of primes $\leq n$ is thus approximated *non-heuristically* (Lemma 37.5 and Corollary 37.14) by:

  $\pi(n) \approx \pi_H(n) = n \prod_{i=1}^{\pi(\sqrt{n})}(1 - \frac{1}{p_i}) \sim 2.e^{-\gamma}.\frac{n}{log_e n} \rightarrow \infty$.

(b) For each n, the expected number of primes in the interval $(p^2_{\pi(\sqrt{n})},\ p^2_{\pi(\sqrt{n})+1})$ is (as illustrated in §35, Fig.2):

$\pi_L(p^2_{\pi(\sqrt{n})+1}) - \pi_L(p^2_{\pi(\sqrt{n})}) = \{(p^2_{\pi(\sqrt{n})+1} - p^2_{\pi(\sqrt{n})}) \prod_{i=1}^{\pi(\sqrt{n})}(1 - \frac{1}{p_i})\}$.

- The number $\pi(n)$ of primes $\leq n$ is also thus approximated *non-heuristically* (Lemma 37.8 and Corollary 37.13) for $n \geq 4$ by the cumulative sum:

  $\pi(n) \approx \pi_L(n) = \sum_{j=1}^{n} \prod_{i=1}^{\pi(\sqrt{j})}(1 - \frac{1}{p_i}) \sim a.\frac{n}{log_e n} \rightarrow \infty$ for some constant $a > 2.e^{-\gamma}$.

(c) For each n, the expected number of Dirichlet primes—of the form $a + m.d$ for some natural number $m \geq 1$—in the interval $(p^2_{\pi(\sqrt{n})},\ p^2_{\pi(\sqrt{n})+1})$ is:

$\{(p^2_{\pi(\sqrt{n})+1} - p^2_{\pi(\sqrt{n})}) \prod_{i=1}^{k} \frac{1}{q_i^{\alpha_i}}. \prod_{i=1}^{k}(1 - \frac{1}{q_i})^{-1}. \prod_{j=1}^{\pi(\sqrt{n})}(1 - \frac{1}{p_j})\}$

where $1 \leq a < d = q_1^{\alpha_1}.q_2^{\alpha_2} \ldots q_k^{\alpha_k}$ and $(a, d) = 1$.

- The number $\pi_{(a,d)}(n)$ of Dirichlet primes $\leq n$ is thus approximated *non-heuristically* (Lemma 38.10) for all $n \geq q_k^2$ by the cumulative sum:

  $\pi_{(a,d)}(n) \approx \prod_{i=1}^{k} \frac{1}{q_i^{\alpha_i}}. \prod_{i=1}^{k}(1 - \frac{1}{q_i})^{-1}. \sum_{l=1}^{n} \prod_{j=1}^{\pi(\sqrt{l})}(1 - \frac{1}{p_j}) \rightarrow \infty$.

(d) For each n, the expected number of $\mathbb{TW}$ primes—such that n is a prime and $n + 2$ is either a prime or $p^2_{\pi(\sqrt{n})+1}$—in the interval $(p^2_{\pi(\sqrt{n})},\ p^2_{\pi(\sqrt{n})+1})$ is:

$\{(p^2_{\pi(\sqrt{n})+1} - p^2_{\pi(\sqrt{n})}) \prod_{i=2}^{\pi(\sqrt{n})}(1 - \frac{2}{p_i})\}$.

- The number $\pi_2(p^2_{k+1})$ of twin primes $\leq p^2_{k+1}$ is thus approximated *non-heuristically* (Lemma 39.8) for all $k \geq 1$ by the cumulative sum:

  $\pi_2(p^2_{k+1}) \approx \sum_{j=9}^{p^2_{k+1}} \prod_{i=2}^{\pi(\sqrt{j})-1}(1 - \frac{2}{p_i}) \rightarrow \infty$.

In Chapter 40 we show that the argument of Theorem 39.9 in Chapter 39 is a special case of the behaviour as $n \rightarrow \infty$ of the Generalised Prime Counting Function $\sum_{j=1}^{n} \prod_{i=a}^{\pi(\sqrt{j})}(1 - \frac{b}{p_i})$, which estimates the number of integers $\leq n$ such that there are b values that cannot occur amongst the residues $r_{p_i}(n)$ for $a \leq i \leq \pi(\sqrt{j})$[8]:

- $\sum_{j=1}^{n} \prod_{i=a}^{\pi(\sqrt{j})}(1 - \frac{b}{p_i}) \rightarrow \infty$ as $n \rightarrow \infty$ if $p_a > b \geq 1$ (Theorem 40.1)

where $0 \leq r_i(n) < i$ is defined for all $i > 1$ by:

$$n + r_i(n) \equiv 0 \ (mod\ i).$$

1.11. Part 11: *Evidence-based* reasoning and the cognitive sciences

Finally, in Chapters 43 and 44, we informally—albeit critically—consider Lakoff and Núñez's attempt to address the nature of what is commonly accepted as the body of knowledge intuitively viewed as the domain of *abstract* mathematical ideas, by introducing the concept of *mathematical idea analysis* and enquiring:

QUERY 1.2. How can cognitive science bring systematic *scientific rigor* to the realm of human mathematical ideas, which lies outside the rigor of mathematics itself?

where they clarify that:

> " The purpose of of mathematical idea analysis is to provide a new level of understanding in mathematics. It seeks to explain *why* theorems are true on the basis of what they mean. It asks what ideas—especially what metaphorical ideas—are built into axioms and definitions. It asks what ideas are implicit in equations and how *ideas* can be expressed by mere numbers. And finally it asks what is the ultimate grounding of each complex idea. That, as we shall see, may require some complicated analysis:
>
> 1. tracing through a complex mathematical idea network to see what the ultimate grounding metaphors in the network are;
> 2. isolating the linking metaphors to see how basic grounded ideas are linked together;
> 3. figuring out how the immediate understanding provided by the individual grounding metaphors permits one to comprehend thye complex idea as a whole."
>
> *. . . Lakoff and Núñez:* [**LR00**], *Chapter 15, p.338.*

Without engaging in technical niceties regarding cognition and cognitive semantics, we attempt to informally extend Lakoff and Núñez's intent on the nature of *understanding* by an individual mind of a concept created in the mind by differentiating as below (compare §23.2 in Chapter 23):

(a) *Subjective understanding*: which we view as an individual mind's perspective involving pattern recognition of a selected set of truth assignments by the individual to declarative sentences of a symbolic language, based on the individual's *uncritical personal* beliefs of a correspondence between:

[8]Thus $b = 1$ yields an estimate for the number of primes $\leq n$, and $b = 2$ an estimate for the number of TW primes (Definition 39.1) $\leq n$.

- what is *believed* as true (as reflected by the truth assignments); and
- what is perceived or pronounced as 'factual' (reflecting *uncritical* conclusions drawn from individual cognitive experience) in a common external world;

(b) *Projective understanding*: which we view as an individual mind's perspective involving pattern recognition of a selected set of truth assignments by the individual to declarative sentences of a symbolic language, based on the individual's *critical plausible* belief of a correspondence between:

- what is *assumed*, or *postulated*, as true (as reflected by the truth assignments); and
- what is perceived or projected as 'factual' (reflecting *plausible* conclusions drawn from individual cognitive experience) in a common external world;

(c) *Collaborative (objective) understanding*: which we view as an individual mind's perspective involving pattern recognition of a selected set of truth assignments by the individual to declarative sentences of a symbolic language, based on the individual's *shared evidence-based* belief of a correspondence between:

- what is *accepted by convention* as true (as reflected by *evidence-based* truth assignments—such as those in Chapter 7, Chapter 8, and Chapter 9); and
- what is perceived or conjectured as 'factual' (reflecting shared *evidence-based* cognitive experiences) in a common external world.

In other words, from an *evidence-based* perspective, the 'understanding' of an abstract mental concept—whether *subjective*, *projective*, or *collaborative*—is not limited, as Lakoff and Núñez appear to suggest, in merely identifying the conceptual metaphors that are used to describe the concept within a language; it must encompass, further, awareness of the *evidence-based* assignments of truth values to the declarative sentences of the language—in which the conceptual metaphors are expressed—that correspond, or are believed to correspond, to what is perceived or conjectured as 'factual' cognitive experiences in a common external world.

Accordingly, we treat Lakoff and Núñez's *mathematical ideas* to refer not to some putative *content* of some abstract structure, conceived by an individual mind in a platonic domain of *ideas* some of which can be termed as of a mathematical nature, but to the pattern recognition of some selected set of 'truth' assignments to (presumed faithful[9]) representations—of conceptual metaphors grounded in sensory motor perceptions—by an individual mind in an artificially constructed symbolic language that can be termed as 'mathematical'.

'Mathematical' in the sense that the language—in sharp contrast to languages of common discourse, which embrace ambiguity as essential for capturing and expressing the full gamut of any cognitive experience of our common external

[9]By some effective procedure such as, for example, Tarski's inductive definitions of the satisfiability and truth of the formulas of a formal mathematical language under a Tarskian interpretation (as detailed in Chapter 6).

world[10]—is designed to facilitate unambiguous pattern recognition of a narrowly selected aspect of a cognitive experience—and its effective communication to another mind—between the limited perception which was sought to be represented, and its representation at any future recall.

Thus, the significance for *evidence-based* reasoning of Lakoff and Núñez's analysis of those conceptual metaphors which are most appropriately represented in a mathematical language, lies in their conclusion that all representations of physical phenomena in a mathematical language are ultimately grounded not in any 'abstract, transcendent', genetically inherited, knowledge, but in conceptual metaphors that import modes of reasoning reflecting, and endemic to, human sensory-motor-experience.

Based on our above interpretation of Lakoff and Núñez's analysis in [**LR00**], we venture to express two tacit theses of this investigation as:

- Those of our conceptual metaphors which we commonly accept as of a mathematical nature—whether grounded directly in an external reality, or in an internally conceptualised Platonic universe of conceived concepts (such as, for example, Cantor's first transfinite ordinal ω)—when treated as Carnap's *explicandum*, are expressed most naturally in the language of the first-order Set Theory ZFC (Thesis 44.1).
  - This reflects the *evidence-based* perspective of this investigation that (see §21.4; also Chapter 23):
    * Mathematics is a set of symbolic languages;
    * A language has two functions—to express and to communicate mental concepts[11];
    * The language of a first-order Set Theory such as ZFC is sufficient to adequately represent (Carnap's *explicatum*: see Chapter 14) those of our mental concepts (Carnap's *explicandum*: see Chapter 14) which can be communicated unambiguously; whilst the first-order Peano Arithmetic PA best communicates such representations to an other categorically.
- The need for adequately expressing such conceptual metaphors in a mathematical language reflects an evolutionary urge of an organic intelligence to determine which of the metaphors that it is able to conceptualise can be unambiguously communicated to another intelligence—whether organic or mechanical—by means of *evidence-based* reasoning and, ipso facto, can be treated as faithful representations of a commonly accepted external reality (universe) (Thesis 44.2).

[10]The absurd extent to which languages of common discourse need to tolerate ambiguity; both for ease of expression and for practical—even if not theoretically unambiguous and effective—communication in non-critical cases amongst intelligences capable of a lingua franca, is briefly addressed in Chapter 24.

[11]Qn: Is this reflected in the structure or activity of the brain?

Part 1

The significance of *evidence-based* reasoning for the foundations of Philosophy and Classical Mathematics

CHAPTER 2

Theological metaphors in mathematics

The significance of the theological distinction sought to be made in this investigation is highlighted by philosopher Stanislaw Krajewski in a recent review of the unsettling 'omniscient theological' claims that mathematics has sought—and yet seeks—to impose upon those whom it should seek to serve[1].

2.1. Brouwer's intuitionism seen as mysticism

For instance we note that, from Krajewski's perspective:

> "Brouwer created mathematical intuitionism and was a mystic. The relationship between the two must not be excluded even though Brouwer seemed to deny any connection. In 1915, he wrote that neither practical nor theoretical geometry can have anything to do with mysticism. (after van Dalen, 1999, 287) On the other hand, in a 1948 lecture Consciousness, Philosophy, and Mathematics, he summed up his famous picture of the mental or, indeed, is it mystical? origins of arithmetic, and eventually of the whole of mathematics:
>
>> 'Mathematics comes into being, when the two-ity created by a move of time is divested of all quality by the subject, and when the remaining empty form of the common substratum of all two-ities, as a basic intuition of mathematics is left to an unlimited unfolding, creating new mathematical entities ...' (Brouwer, 1949, 1237; or 1975, 482)".
>
> . . . *Krajewski:* [**Kr16**].

Whereas the ephemeral nature of Brouwer's 'mysticism'—and the relevance of his, by conviction 'mathematically inarticulable'[2], intuitionistic beliefs for the foundations of mathematics—may escape rational articulation, we show in §3.2 that Brouwer's philosophy could, at the very least, be labeled 'atheistic' in that it sought to deny mathematical principles, such as the Law of the Excluded Middle, as an article of faith *without* providing sufficient *evidence-based* grounds for such denial.

2.2. The unsettling consequences of belief-driven mathematics

In his review Krajewski stresses the disquieting consequences of such belief-driven mathematics:

[1]'Serve' in the sense sought to be elaborated in §21.4 and §23

[2]According to van Atten and Tragesser in [**AT03**], which illuminates the dramatically contrasting ways in which not only Brouwer, but also Gödel—although at opposite philosophical poles from an objective perspective—perceived their own mystical beliefs and vainly strained— in the absence of a common evidential yardstick for defining arithmetical truth—to seek subjectively sustainable bases for their respective dogmas: namely, Brouwer's rejection of LEM as non-constructive, and Gödel's believing all formal arithmetics to be 'omnisciently' ω-consistent, both of which we show as mistaken (the first as an immediate consequence of Corollary 9.11; and the second by Theorem 8.5 and, independently, Corollary 11.6).

> "Examples of possible theological influences upon the development of mathematics are indicated. The best known connection can be found in the realm of infinite sets treated by us as known or graspable, which constitutes a divine-like approach. Also the move to treat infinite processes as if they were one finished object that can be identified with its limits is routine in mathematicians, but refers to seemingly super-human power. For centuries this was seen as wrong and even today some philosophers, for example Brian Rotman, talk critically about "theological mathematics". Theological metaphors, like "God's view", are used even by contemporary mathematicians. While rarely appearing in official texts they are rather easily invoked in "the kitchen of mathematics". There exist theories developing without the assumption of actual infinity the tools of classical mathematics needed for applications (For instance, Mycielski's approach). Conclusion: mathematics could have developed in another way. Finally, several specific examples of historical situations are mentioned where, according to some authors, direct theological input into mathematics appeared: the possibility of the ritual genesis of arithmetic and geometry, the importance of the Indian religious background for the emergence of zero, the genesis of the theories of Cantor and Brouwer, the role of Name-worshipping for the research of the Moscow school of topology. Neither these examples nor the previous illustrations of theological metaphors provide a certain proof that religion or theology was directly influencing the development of mathematical ideas. They do suggest, however, common points and connections that merit further exploration."
> … *Krajewski:* [**Kr16**].

The disquieting, 'reality-denying', consequences of Krajewski's point that:

> "…the move to treat infinite processes as if they were one finished object that can be identified with its limits is routine in mathematicians, but refers to seemingly super-human power."

is seen in §24.3, where we are confronted with 2-dimensional geometrical models, of infinite processes expressing plausible real-world examples, that have well-defined geometrical limits which do not, however, correspond to their 'limiting' configurations in a putative 'completion' of Euclidean Space.

As we argue in Chapter 19, since every real number is specifiable in PA (Theorem 19.7), instead of defining real numbers as the putative limits of putatively definable Cauchy sequences[3] which 'exist' in some omniscient Platonic sense in the interpretation of an arithmetic, we can alternatively define—from the perspective of constructive mathematics, and seemingly without any loss of generality—such numbers instead by their *evidence-based*, algorithmically *verifiable*, number-theoretic functions (as defined in Chapter 5) that formally express—in the sense of Carnap's 'explication' —the corresponding Cauchy sequences, viewed now as non-terminating *processes* in the standard interpretation of the arithmetic that may, sometimes, tend to a discontinuity (see §24.3, Case 2(a) and 2(b); also Case 25.1).

Moreover, as Krajewski further notes—and implicitly questions—the dichotomy in accepting omniscient 'limits' on the basis of, seemingly subjective, 'self-evidence' comes at an unacceptable price: it compels the prevalent double-standards in addressing mathematical and logical concepts that are defined in terms of 'infinite' processes:

---

[3]'putatively definable' since not all Cauchy sequences are algorithmically computable (Theorem 5.4). The significance of this distinction for the physical sciences is highlighted in §29.6 and §29.7

> "Up to the 18th century only potential infinity was considered meaningful. For example, Leibniz believed that "even God cannot finish an infinite calculation." (Breger, 2005, 490) Since the 19th century we have been using actually infinite sets, and for more than a hundred years we have been handling them without reservations. Nowadays students are convinced that this is normal and self-evident as soon as they begin their study of modern mathematics. This constitutes the unbelievable triumph of Georg Cantor. There may have been precursors of Cantor, and as early as five centuries before him there had been ideas about completing infinite additions—as documented in the paper by Zbigniew Król in the present volume—but clearly it was Cantor who opened to us the realm of actually infinite structures.
>
> As is well known, we handle, or at least we pretend we can handle, with complete ease the following infinite sets (and many other ones): the set of (all) natural numbers, real numbers etc.; the transfinite numbers—even though the totality of all of them seems harder to master; the set of (all) points in a given space, the sets of (all) functions, etc.
>
> It is apparent that we behave in the way described by Boethius or Burley as being proper to God. Infinite structures are everyday stuff for mathematicians. What is more, we are used to handling infinite families of infinite structures. Thus the set (class) of all models of a set of axioms is routinely taken into account as is the category of topological spaces and many other categories approached as completed entities. In addition, in mathematical logic one unhesitantly considers such involved sets as the set of all sentences true in a specific set theoretical structure or in each member of an arbitrary family of structures.
>
> Such behavior is so familiar that no mathematician sees it as remarkable. But the fact is that this is like being omniscient. We do play the role of God or, rather, the role not so long ago deemed appropriate only for God!
>
> From where could the idea of actual infinity in mathematics have arisen? The only other examples of talk that remind of actual infinity are religious or theological, as the just mentioned verses from the psalms indicate. This fact is suggestive but it does not constitute a proof that post-Cantorial mathematics was derived from theology. Actually, we know that Cantor was stimulated by internal mathematical problems of iterating the operation of the forming of a set of limit points and performing the "transfinite" step in order to continue the iteration. This fact leads to a more general issue of infinite processes."
>
> ... *Krajewski:* [**Kr16**].

2.3. Does mathematics *really* 'need' to be omniscient?

The 'need' for an omniscience that permits 'reification' of a putative infinite process—as in the postulation of an Axiom of Choice—is frowned upon by Krajewski (also shown as dispensable from a cognitive perspective by Lakoff and Núñez in [**LR00**]), since it merely obscures the lack of well-definedness—in the sense of *evidence-based* justification—of the infinite process and, ergo, of any consequences that appeal to the Axiom:

> "Another historically important example of a reification of an infinite action is provided by the Axiom of Choice. Choosing one element from each set of an arbitrary family of (disjoint) sets must constitute a series of movements; if the family is infinite it must be an infinite series of operations.
>
> If there is a single rule according to which the choice is done then the resulting set of representatives can be defined and can be relatively safely assumed to exist. In the case of an arbitrary family of sets there is no such definition, and it is necessary to postulate the existence of the selection set.

Its existence is not self-evident. The first uses of the Axiom of Choice were unconscious, but seemed natural to the advocates of unrestricted infinite mathematics. However, when the use of this axiom became understood, opposition against it arose. Among the opponents were important mathematicians, like the French "semi-intuitionists", who did handle infinite operations, but felt that some limitations were necessary. For example, in 1904 Emile Borel claimed that arbitrary long transfinite series of operations would be seen as invalid by every mathematician. According to him the objection against the Axiom of Choice is justified since "every reasoning where one assumes an arbitrary choice made an uncountable number of times ... is outside the domain of mathematics". Interestingly, against Borel, Hadamard saw no difference between uncountable and countable infinite series of choices. He rejected, however, an infinity of dependent choices when the choice made depends on the previous ones. (Borel 1972, 1253) All the just mentioned choice principles are considered obviously acceptable and innocent by contemporary mathematicians. The former opposition was clearly derived from the realization that an infinite number of operations is impossible. Or, it is impossible if our power is not divine.

Another familiar example of handling the result of an infinite process as if it was unproblematic is found in mathematical logic. Namely, we often consider the set of all logical consequences of a set of propositions. Of course, it is impossible to "know" all of them. It is also impossible to write down all of them—their number is infinite and most of these consequences are too long to be practically expressible—although when the initial set is recursive a program can produce the list (in a given language) if it runs infinitely long or infinitely fast. Thus, by assuming suitable idealizations we can assume that the set of all logical consequences can be seen as "given". Many similar moves are routinely done in contemporary mathematical logic. An infinite process of deriving subsequent consequences is seen as one step. We behave as if we knew all the logical consequences. This is like being omniscient."
... Krajewski: [**Kr16**].

2.4. Mathematicians ought to practice what they preach

Echoing Melvyn B. Nathanson's disquiet expressed in another context (see §24), Krajewski notes with concern the fact that there is an unhealthy divide between what mathematicians do and what they preach:

"Occasionally traces of this way of talking can be retained in an "official" text. Thus, as mentioned before, we can talk about performing infinitely many acts (or even a huge finite number of steps that is practically inaccessible) as if we had an unlimited, "divine" mind; we can refer to a complete knowledge (for instance, taking the set of all sentences true in a given interpretation) as if we were actually omniscient. We can also refer to paradise in Hilbert's sense. This paradise was challenged by Wittgenstein who built upon the metaphor saying that rather than fear expulsion we should leave the place. "I would do something quite different: I would try to show you that it is not a paradise—so that you'll leave of your own accord." (Wittgenstein, 1976, 103)

One could say that all such figurative utterances using, directly or indirectly, theological terms are irrelevant and should be ignored in reflections about the nature of mathematics; they are mere chatting, present around mathematics, but not part of it.

Yet this loose conversation does constitute a part of real mathematics, says Reuben Hersh in (1991). His argument is ingenious: let us consider seriously the fact that mathematics, like any other area of human activity, has a front and a back, a chamber and a kitchen. The back is of no less importance since the product is made there. The guests or customers enter the front door but the professionals use the back door. Cooks do not show the patrons of their restaurant how the meals are prepared. The same can be said about mathematics, and for this reason its mythology reigns supreme.

It includes, says Hersh, such “myths” as the unity of mathematics, its objectivity, universality, certainty (due to mathematical proofs). Hersh is not claiming that those features are false. He reminds, however, that each one has been questioned by someone who knows mathematics from the perspective of its kitchen. Real mathematics is fragmented; it relies on esthetic criteria, which are subjective; proofs can be highly incomplete, and some of them have been understood in their entirety by nobody. And it is here where the ancient or primitive references can be retained. It is deep at “the back” that we could say that only God knows the entire decimal representation of the number π. If we were to say that “at the front”, we would stress it was just a joke.

In the kitchen, mathematicians borrow liberally from religious language. One telling example is the saying of Paul Erdös, the famous author of some 1500 mathematical papers (more than anyone else), according to which there exists the Book in which God has written the most elegant proofs of mathematical theorems. Erdös was very far from standard religiosity, but he reportedly said in 1985, “You don’t have to believe in God, but you should believe in The Book.” (Aigner & Ziegler, 2009) Probably the most famous example of direct use of theology in mathematics can be found in the reaction, in 1888, of Paul Gordan to Hilbert’s non-constructive proof of the theorem on the existence of finite bases in some spaces. Gordan said, “Das ist nicht Mathematik. Das ist Theologie.” It is worth adding that later, having witnessed further accomplishments of Hilbert, he would admit that even “theology” could be useful (Reid, 1996, 34, 37).

One can easily dismiss such examples. Almost everyone would say that while the criticism of a non-constructive approach to mathematics is a serious matter, the use of theological language is just a rhetorical device and has no deeper significance. The same would be said about Hilbert’s mention of “the paradise” in his lecture presenting “Hilbert’s Program”. However, in another classic exposition of a foundational program, Rudolf Carnap, in 1930, while talking about logicism, used the phrase “theological mathematics.” According to him, Ramsey’s assumption of the existence of the totality of all properties should be called “theological mathematics” in contradistinction to the “anthropological mathematics” of intuitionists; in the latter, all operations, definitions, and demonstrations must be finite. When Ramsey “speaks of the totality of properties he elevates himself above the actually knowable and definable and in certain respects reasons from the standpoint of an infinite mind which is not bound by the wretched necessity of building every structure step by step.” (Benacerraf & Putnam, 1983, 50)

Carnap’s statement brings us back to the issue of being omniscient, considered above in Section II. There are other examples of religious references which do not deal directly with infinity. In the 19th century, the trend arose to provide foundations for mathematics, and it turned out to be very fruitful. The very idea of the foundations of mathematics assumes the presence of an absolute solid rock on which the building of mathematics is securely built. This image has been challenged, and the vision of mathematics without foundations is now favored by many philosophers of mathematics. The question that can be asked in our context is, Whence did the idea of foundations come from? It could have come from everyday experience. However, the idea of absolute certainty has a theological flavor. In our world, in our lives, foundations are hardly absolute, unchanging, unquestionable. As soon as we hope for absolutely secure foundations we invoke a religious dimension. The metaphor of the rock on which we can firmly stand is as

> much common human experience as it is a Biblical image: God is called the Rock, truth means absolute reliability, etc."
>
> ... *Krajewski:* [**Kr16**].

2.5. Mathematicians must always *know* what they are talking about

Krajewski notes with concern how such perspectives could be leading mathematicians into a false sense of security concerning structures whose putative existence they are able to conceive, but whose logic may not be constructively well-defined (in the sense of the proposed Definitions 21.3 to 21.7):

> "The mathematicians who established the Moscow school of mathematics, Dimitri Egorov, Nikolai Luzin, and Pavel Florensky (who was also a priest), unlike their French colleagues, were not afraid of infinities and contributed in a decisive way to the creation of descriptive set theory. ...
>
> The connection of this practice to mathematics is supposedly to be seen in the fact that objects like transfinite numbers exist "just from being named." Naming a certain infinite set using appropriate logical formula makes sure that the set exists. Although to a modern skeptic there is hardly a special connection between those theological views and mathematics, the fact is that Luzin, Egorov, and some others saw the connection. In addition, a somewhat similar view was later expressed by another mathematical genius, Alexander Grothendieck; he stressed the importance of naming things in order to isolate the right entities from the complex scene of mathematical objects and "keep them in mind". "Grothendieck, like Luzin, placed a heavy emphasis on 'naming,' seeing it as a way to grasp objects even before they have been understood." (Graham & Kantor, 2009, 200)"
>
> ... *Krajewski:* [**Kr16**].

He deplores the implicit Creationism underlying the 'creation' of Cantor's paradise of transfinite sets in terms of, ultimately, a null set (nothingness), rather than treating sets from an Evolutionary perspective as successors of a postulated fundamental unit set (an undefined something):

> "A well-known foundational approach to mathematics uncovers the role of theological categories: the void and infinite power. In standard set theory zero is identified with the empty set, and then 1 is defined as 0, 2 as 0, 0, and, in general, n + 1 as 0, 1, 2, . . . , n.
>
> This construction, introduced by John von Neumann, is the most convenient one, but not the only way to define natural numbers as sets. Other numbers—integers, rationals, reals, complex numbers—can be easily defined.
>
> Actually, in a similar way all mathematical entities investigated in traditional mathematics—functions, structures, spaces, operators, etc.—can be defined as "pure" sets, that is, sets constructed from the empty set.
>
> The construction must be performed in a transfinite way. Note that the universe of pure sets arises via a transfinite induction, indexed by ordinal numbers.
>
> In other words, from zero we can create "everything," or rather the universe of sets sufficient for the foundations of mathematics. The construction assumes the reality of the infinity of ordinal numbers, which means that in order to create from zero we need infinite power. Nothing, emptiness, is combined with infinite power and a kind of unrestricted will to continue the construction ad infinitum. Together they give rise to the realm of sets where mathematics can be developed. This is a rather normal way of describing the situation. Mathematicians would reject suggestions that this has something

to do with theology. Yet terms like "infinite power," "all-powerful will" are unmistakably theological. If Leibniz had known modern set theory, he would have rejoiced, both as a theologian and as a mathematician. He claimed that "all creatures derive from God and nothing." (Breger, 2005, 491) When he introduced the binary notation, he gave theological significance to zero and one: "It is true that as the empty voids and the dismal wilderness belong to zero, so the spirit of God and His light belong to the all-powerful One.""
... Krajewski: [**Kr16**].

2.6. Explicit omniscience in set theory

Such visions of omniscience are also reflected in the following remarks, where it is not obvious whether set-theorist Saharon Shelah makes a precise distinction between:

- the authority that derives from vision-based, intuitive 'truth' (in the sense of paragraph (i) in §23.2); and
- the authority that derives from Tarski's formal, classical, definitions of the 'truth' of the formulas of a formal system under a constructively well-defined, i.e., *evidence-based*, interpretation (in the sense, for instance, of Chapters 5 and 6; as also of Definitions 21.5 to 21.7),

since he remarks that:

"I am in my heart a card-carrying Platonist seeing before my eyes the universe of sets ... (regarding) the role of foundations, and philosophy ... I do not have any objection to those issues per se, but I am suspicious ... My feeling, in an overstated form, is that beauty is for eternity, while philosophical value follows fashion."
... Shelah: [**She91**].

As we seek to establish in this investigation, Shelah's faith—in the ability of intuitive truth to faithfully reflect relationships between elements of a seemingly Platonic universe of sets—may be as misplaced as his assumption that such truth cannot be expressed in a constructive, and effectively verifiable, manner (see §8.1).

In other words, the question of intuitive truth may be linked to that of the consistent introduction of mathematical concepts into first-order languages such as ZF, through axiomatic postulation, in ways that—as explicated by cognitive scientists Lakoff and Núñez in [**LR00**] (see also Chapter 43)—may not be immediately obvious to a self-confessed Platonist such as Shelah; even if we grant him the vision that is implicit in his following remarks:

"From the large cardinal point of view: the statements of their existence are semi-axioms, (for extremists - axioms). Adherents will probably say: looking at how the cumulative hierarchy is formed it is silly to stop at stage ω after having all the hereditarily finite sets, nor have we stopped with Zermelo set theory, having all ordinals up to $\aleph_\omega$, so why should we stop at the first inaccessible, the first Mahlo, the first weakly compact, or the first of many measurables? We are continuing the search for the true axioms, which have a strong influence on sets below (even on reals) and they are plausible, semi-axioms at least.

A very interesting phenomenon, attesting to the naturality of these axioms, is their being linearly ordered (i.e., those which arise naturally), though we get them from various combinatorial principles many of which imitate $\aleph_0$, and from consistency of various "small" statements. It seems that all "natural" statements are equiconsistent[4] with some large cardinal in this scale; all of this prove their naturality.

This raises the question:

> ISSUE: Is there some theorem explaining this, or is our vision just more uniform than we realize?
>
> Intuition tells me that the power set and replacement axioms hold, as well as choice (except in artificial universes), whereas it does not tell me much on the existence of inaccessibles. According to my experience, people sophisticated about mathematics with no knowledge of set theory will accept ZFC when it is presented informally (and well), including choice but not large cardinals. You can use collections of families of sets of functions from the complex field to itself, taking non-emptiness of cartesian products for granted and nobody will notice, nor would an ω-fold iteration of the operation of forming the power set disturb anybody. So the existence of a large cardinal is a very natural statement (and an interesting one) and theorems on large cardinals are very interesting as implications, not as theorems (whereas proving you can use less than ZFC does not seem to me very interesting).

. . . *Shelah:* [**She91**].

That Shelah's Platonism is reflective of a continuing widespread practice, if not belief—decried by Krajewski[5]—is seen in this 1997 observation by mathematician Reuben Hersh:

> "The working mathematician is a Platonist on weekdays, a formalist on weekends. On weekdays, when doing mathematics, he's a Platonist, convinced he's dealing with an objective reality whose properties he's trying to determine. On weekends, if challenged to give a philosophical account of this reality, it's easiest to pretend he doesn't believe in it. He plays formalist, and pretends mathematics is a meaningless game."

. . . *Hersh* [**Hr97**].

which echoed an unusually frank—seemingly unrepentant—confession of double standards made 27 years earlier by Jean Dieudonné:

> "On foundations we believe in the reality of mathematics, but of course, when philosophers attack us with their paradoxes, we rush to hide behind formalism and say 'mathematics is just a combination of meaningless symbols,'... Finally we are left in peace to go back to our mathematics and do it as we have always done, with the feeling each mathematician has that he is working with something real. The sensation is probably an illusion, but it is very convenient."

. . . *Dieudonné* [**Di70**].

---

[4]We note that if—as Shelah appears to imply—we may treat the subsystem ACA_0 of second-order arithmetic as a conservative extension of PA that is *equiconsistent* with PA, then we are led to the curious conclusion—since PA is *finitarily* consistent by Theorem 9.10—that (see Theorem 22.3 in Chapter 22) Goodstein's sequence $G_o(m_o)$ over the finite ordinals in ACA_0 *terminates* with respect to the ordinal inequality '$>_o$' *even if* Goodstein's sequence $G(m)$ over the natural numbers in ACA_0 *does not terminate* with respect to the natural number inequality '$>$' in any putative model of ACA_0.

[5]And uneasily accepted by Bauer in [**Ba16**] (see §13.4).

2.7. Do mathematicians practice a 'faith-less' platonism?

An intriguing perspective on the implicit 'platonism' of a practicing mathematician is offered by philosopher John Corcoran in his thought-provoking 1973 paper [**Cor73**]: 'Gaps between logical theory and mathematical practice'.

> "The view of mathematics adopted here can be called neutral platonism. It understands mathematics to be a class of sciences each having its own subject-matter or universe of discourse. Set theory is a science of objects called sets. Number theory is about the natural numbers. Geometry pre supposes three universes of objects: points, lines and planes. String theory or *Semiotik* is about strings of ciphers (digits or characters). Group theory presupposes the existence of complex objects called groups.
>
> Following Bourbaki, Church, Hardy, Gödel and many other mathematicians, it holds that these objects exist and that they are independent of the human mind in the sense that
>
> (1) their properties are fixed and not subject to alteration and
>
> (2) they are not created by any act of will.
>
> In a word: mathematical truth is discovered, not invented; mathematical objects are apprehended, not created.
>
> According to this view the unsettled propositions of mathematics (Goldbach's problem, the twin prime problem, the continuum problem and the like) are each definitely true or definitely false and when their truth-values are derived it will be by discovery and not by *con*vention and not by *in*vention.
>
> Foundations of mathematics is usually discussed in a metalanguage of mathematical languages, as has been the case here. Platonism, purely and simply, makes in the metalanguage the presuppositions that mathematicians make in their object languages. What the mathematician lets his object language variables range over the platonist lets *his* metalanguage variables range over. The neutral platonist differs from the platonist by distinguishing the foundations of the *foundations* of mathematics from the foundations of mathematics. With regard to foundations, simply, the neutral platonist is a platonist, simply. With regard to the foundations of the foundations the neutral platonist is neutral. Using the metalanguage the neutral platonist agrees that numbers exist but adds, using the meta-metalanguage, that he does not know how such assertions should be *ultimately* understood. The question of the existence of mathematical objects is answered affirmatively but the question of the ultimate nature of that existence is not answered at all. To the neutral platonist the various philosophies of mathematics which have been offered are all considered as interesting hypotheses concerning foundations of foundations each of which may be true, false or meaningless—indeed the neutral platonist admits that foundations of foundations may be meaningless. Contrast neutral platonism with extreme formalism. The extreme formalist claims that foundations of mathematics is contentful but that mathematics itself is meaningless. The neutral platonist claims that both foundations and mathematics are meaningful but offers no view on foundations of foundations."
>
> *... Corcoran:* [**Cor73**], *§1, pp.23-25.*

Viewed from the evidence-based perspective of a thesis (Thesis 44.1) of this investigation—that the objects of mathematics can broadly be identified as the terms (Carnap's explicatum in [**Ca62a**]), of a first-order mathematical language which seeks to faithfully express what Lakoff and Nunez ([**LR00**]) term as the conceptual metaphors (Carnap's explicandum in [**Ca62a**]) of an individual intelligence—the question arises:

- Could one today generically substitute a term such as, for instance, 'subjective platonism' for 'neutral platonism', whose domain/s may then be taken as those conceptual metaphors of an individual intelligence which can be faithfully expressed in a first-order mathematical language such as the set theory ZFC; and
- Reserve the term 'neutral platonism' or, say, 'objective platonism' for only those conceptual metaphors of an individual intelligence that can be both faithfully expressed and unambiguously communicated to an other intelligence in a categorical first-order mathematical language such as the Peano Arithmetic PA?

If so, could one then justifiably claim that the philosophy underlying the practice of mathematics is a 'faith-less' platonism (in Corcoran's foundational sense) since it admits of mathematical objects that:

(a) their properties are fixed by the immutable symbols (semiotic strings) in which an individual intelligence's conceptual metaphors are grounded, and are therefore not subject to alteration; and

(b) they are not created by any act of will of an individual intelligence, but by an agreed upon convention (for the generation of the semiotic strings);

(c) mathematical truth is discovered (as a property assigned by convention to the semiotic strings), not re-invented;

(d) mathematical objects (semiotic strings) are apprehended, not created?

Or would this stretch an analogy too far from the intent of the original?

CHAPTER 3

Three perspectives of logic

We shall now argue that the common perceptions of a mutual inconsistency between classical and constructive mathematical philosophies—vis à vis 'omniscient' mathematical truth, and 'omniscient' mathematical ontologies decried by Krajewski—are illusory; they merely reflect the circumstance that, to date, all such philosophies do not explicitly—and unambiguously—define the relations between a language and the logic that is necessary to assign unequivocal, *evidence-based*, truth-values to the propositions of the language (in the sense of the proposed Definitions 21.3 to 21.7).

We shall argue, for instance, that classical perspectives which admit Hilbert's formal definitions of quantification can be labelled 'theistic', since they implicitly assume—without providing objective (i.e., on the basis of *evidence-based* reasoning) criteria for interpreting quantification constructively—both that:

(a) the first-order logic FOL is consistent, and that

(b) Aristotle's particularisation (see Definition 3.1)—which postulates that '$[\neg\forall\neg x]$' can unrestrictedly be interpreted as 'there exists an *unspecified* instantiation of x'—holds under any interpretation of FOL.

In sharp contrast, constructive perspectives based on Brouwer's philosophy of Intuitionism can be labelled 'atheistic' because they:

(i) deny that FOL is consistent (since they deny the Law of The Excluded Middle LEM, which is a theorem of FOL) and

(ii) deny that Aristotle's particularisation holds under any interpretation of FOL that has an infinite domain.

However, we shall adopt what may be labelled as an 'agnostic', finitary, perspective by showing that:

(1) FOL is finitarily consistent (Theorem 9.10); and

(2) although, if Aristotle's particularisation holds in an interpretation of FOL then LEM must also hold in the interpretation (since LEM is a theorem of FOL), the converse is not true, i.e., LEM does not entail Aristotle's particularisation (see §14.1);

(3) Aristotle's particularisation does not hold under any interpretation of FOL that has an infinite domain (an immediate consequence of Corollary 15.11).

We shall further argue that perspectives based on Brouwerian atheism are merely restricted perspectives within the finitary agnostic perspective; whilst perspectives based on Hilbertian theism—when shorn of Hilbert's ε-based formalisation of Aristotle's particularisation—actually complement the agnostic, finitary, perspective.

We shall conclude that the former yield a *strong* finitary interpretation $\boldsymbol{B}$ of PA over the domain $\mathbb{N}$ of the natural numbers, which can be viewed as circumscribing the ambit of *finitary* mechanistic reasoning about 'true' arithmetical propositions; whilst the latter yield the *weak* standard interpretation $\boldsymbol{M}$ of PA over $\mathbb{N}$, which can be viewed as circumscribing the ambit of *non-finitary* human reasoning about 'true' arithmetical propositions.

3.1. Hilbertian Theism: Embracing Aristotle's particularisation

We note that, in a 1925 address ([**Hi25**]), Hilbert had shown that the axiomatisation $\mathcal{L}_\varepsilon$ of classical predicate logic proposed by him as a formal first-order ε-predicate calculus —in which he used a primitive choice-function symbol, 'ε', for defining the quantifiers '$\forall$' and '$\exists$'—would adequately express and yield, under a suitable interpretation, classical predicate logic if the ε-function was interpreted to yield Aristotlean particularisation, which we define as (cf. [**Hi25**], pp.382-383; [**Hi27**], pp.465-466):

Definition 3.1. (*Aristotle's particularisation*) If the formula $[\neg(\forall x)\neg F(x)]$ of a formal first order language L is true under an interpretation, then we may always conclude *unrestrictedly* that there must be some *unspecified* object s in the domain D of the interpretation such that, if the formula $[F(x)]$ interprets as the relation $F^*(x)$ in D, then the proposition $F^*(s)$ is true under the interpretation.

> The significance of the qualification 'unrestrictedly' is that it does not admit the—hitherto unsuspected—possibility (see §11.6) that an *unspecified* instantiation may sometimes be *unspecifiable*— in the sense of Definition 4.1 and Theorem 11.10—within the parameters of some formal system that subsumes FOL.

Classical approaches to mathematics—essentially following Hilbert—can be labelled 'theistic' in that they implicitly assume—without providing adequate objective (i.e., *evidence-based*) criteria for interpreting quantification constructively—both that:

(a) First order logic FOL[1] is consistent; and
(b) Aristotle's particularisation holds *unrestrictedly* under any interpretation of FOL.

The significance of the label 'theistic'[2] is that conventional wisdom 'omnisciently' believes that Aristotle's particularisation remains valid—sometimes without qualification—even over infinite domains; a belief that is not unequivocally self-evident, but must be appealed to as an article of unquestioning faith[3].

---

[1]For the purposes of this investigation we take FOL to be Mendelson's formal theory $\mathcal{K}$ ([**Me64**], p.56) or its equivalent.

[2]Although intended to highlight an entirely different distinction, that the choice of the label 'theistic' may not be totally inappropriate is suggested by Tarski's reported point of view to the effect (Franks: [**Fr09**], p.3): "... that Hilbert's alleged hope that meta-mathematics would usher in a 'feeling of absolute security' was a 'kind of theology' that 'lay far beyond the reach of any normal human science' ...".

[3]See: Whitehead/Russell: [**WR10**], p.20; Hilbert: [**Hi25**], p.382; Hilbert/Ackermann [**HA28**], p.48; Skolem: [**Sk28**], p.515; Gödel: [**Go31**], p.32; Carnap: [**Ca37**], p.20; Kleene: [**Kl52**], p.169;

3.2. Brouwerian Atheism: Denying the Law of Excluded Middle

In sharp contrast, constructive approaches based on Brouwer's philosophy of Intuitionism can be labelled 'atheistic'[4] because they deny—also without providing adequate objective (i.e., *evidence-based*) criteria for interpreting quantification constructively—both that[5]:

(a) FOL is consistent (since they deny that the Law of The Excluded Middle LEM—which is a theorem of FOL—holds under any interpretation of FOL[6]); and

(b) Aristotle's particularisation holds under any interpretation of FOL that has an infinite domain.

Although Brouwer's explicitly stated objection appeared to be to the Law of the Excluded Middle as expressed and interpreted at the time (Brouwer: [**Br23**], p.335-336; Kleene: [**Kl52**], p.47; Hilbert: [**Hi27**], p.475), some of Kleene's remarks ([**Kl52**], p.49), some of Hilbert's remarks (e.g., in [**Hi27**], p.474) and, more particularly, Kolmogorov's remarks (in [**Ko25**], fn. p.419; p.432) suggest that the intent of Brouwer's fundamental objection can also be viewed today as being limited only to the (yet prevailing) belief—as an article of Hilbertian faith—that the validity of Aristotle's particularisation can be extended without qualification to infinite domains.

The significance of the label 'atheistic' is that whereas intuitionistic approaches to mathematics deny the faith-based belief in the unqualified validity of Aristotle's particularisation over infinite domains, their denial of the Law of the Excluded Middle is itself an 'omniscient' belief that is also not unequivocally self-evident, and must be appealed to as an article of unquestioning faith[7].

3.3. Finitary Agnosticism

We shall seek to avoid avoid such 'omniscience' in this investigation, by adopting what may be labelled as a finitarily 'agnostic' perspective in noting that although, if Aristotle's particularisation holds in an interpretation of a FOL then LEM must also hold in the interpretation, the converse is not true.

The significance of the label 'agnostic' is that we shall:

Rosser: [**Ro53**], p.90; Bernays/Fraenkel: [**BF58**], p.46; Beth: [**Be59**], pp.178 & 218; Suppes: [**Su60**], p.3; Luschei: [**Lus62**], p.114; Wang: [**Wa63**], p.314-315; Quine: [**Qu63**], pp.12-13; Kneebone: [**Kn63**], p.60; Cohen: [**Co66**], p.4; Mendelson: [**Me64**], p.52(ii); Novikov: [**Nv64**], p.92; Lightstone: [**Li64**], p.33; Shoenfield: [**Sh67**], p.13; Davis: [**Da82**], p.xxv; Rogers: [**Rg87**], p.xvii; Epstein/Carnielli: [**EC89**], p.174; Murthy: [**Mu91**]; Smullyan: [**Sm92**], p.18, Ex.3; Awodey/Reck: [**AR02b**], p.94, Appendix, Rule 5(i); Boolos/Burgess/Jeffrey: [**BBJ03**], p.102; Crossley: [**Cr05**], p.6.

[4]As can other 'constructive' approaches such as those analysed by Posy in [**Pos13**] (p.106, §5.1).

[5]But see also Maietti: [**Mt09**] and Maietti/Sambin: [**MS05**].

[6]cf. [**Kl52**], p.513: "The formula $\forall x(A(x) \vee \neg A(x))$ is classically provable, and hence under classical interpretation true. But it is unrealizable. So if realizability is accepted as a necessary condition for intuitionistic truth, it is untrue intuitionistically, and therefore unprovable not only in the present intuitionistic formal system, but by any intuitionistic methods whatsoever".

[7]Lending justification to Krajewski's comment in [**Kr16**]: "Brouwer created mathematical intuitionism and was a mystic" see §2.1.

(a) Neither share an ascetic Brouwerian faith which unnecessarily denies appeal to LEM—and, ipso facto, to the consistency of FOL—since we shall show that such consistency follows immediately from a finitary proof of consistency of the first order Peano Arithmetic PA (Theorem 9.10; cf. [**An16**], Theorem 6.8, p.41);

(b) Nor share a libertarian Hilbertian faith that admits Aristotle's particularisation over infinite domains (see Corollary 15.11).

3.4. Two complementary, but seemingly contradictory, perspectives

We shall argue, instead, the thesis that the perceived conflict between classical and intuitionistic interpretations of quantification is illusory; and that the differing perspectives merely reflect two complementary facets of an unappreciated ambiguity—whose roots trace back to antiquity—in the non-finitary postulation of an *unspecified* element in classical predicate logic.

This is the postulation that:

- If it is not the case that, for any specified x, $F(x)$ does not hold, then there exists an *unspecified* x[8], such that $F(x)$ holds;

where 'holds' is to be understood in Tarski's sense ([**Ta35**]) that:

- 'Snow is white' holds as a true assertion if, and only if, it can be determined on the basis of some *agreed*-upon[9] evidence that snow *is* white.

We shall show that recognition, and removal, of the ambiguity has significant consequences for the, not uncommon, perception[10] that Gödel's Incompleteness Theorems limit the effective assignments of truth values to the formulas of a mathematical language such as the first-order Peano Arithmetic PA.

Formally, we shall show that both the classical and intuitionistic interpretations of quantification yield interpretations of the first-order Peano Arithmetic PA—over the structure $\mathbb{N}$ of the natural numbers—that are complementary, not contradictory.[11]

---

[8]We note that, in the case of a first-order Peano Arithmetic such as PA, for instance, it follows from Corollary 11.5 that the PA numeral corresponding to such a putative, *unspecified*, natural number q may not be explicitly definable, by any PA formula, as a first-order term of PA which can be individually denoted within a PA formula.

[9]The significance of viewing mathematical 'truth' as an unequivocal, well-defined, convention is highlighted in the analysis of Tarski's definitions of the satisfaction and truth of the formulas of a formal mathematical language under an interpretation in Chapter 6

[10]Addressed in [**An04**].

[11]Of interest is the following perspective ([**Wl03**], §1.6, p.5), which particularly emphasises the need for such a unified, constructive, foundation for the mathematical representation of elements of reality such as those considered in §27.4: "Our investigations lead us to consider the possibilities for 'reuniting the antipodes'. The antipodes being classical mathematics (CLASS) and intuitionism (INT). . . . It therefore seems worthwhile to explore the 'formal' common ground of classical and intuitionistic mathematics. If systematically developed, many intuitionistic results would be seen to hold classically as well, and thus offer a way to develop a strong constructive theory which is still consistent with the rest of classical mathematics. Such a constructive theory can form a conceptual framework for applied mathematics and information technology. These sciences now use an ad-hoc approach to reality since the classical framework is inadequate. . . . [*and can*] easily use the richness of ideas already present in classical mathematics, if classical mathematics were to

The former yields the *weak* standard interpretation $\boldsymbol{M}$ of PA over $\mathbb{N}$, which is *well-defined* with respect to *weak* non-finitary assignments of algorithmically verifiable Tarskian truth values T_M to the formulas of PA under $\boldsymbol{M}$; and which can be viewed as circumscribing the ambit of *non-finitary* human reasoning about 'true' arithmetical propositions.

The latter yields a *strong* finitary interpretation $\boldsymbol{B}$ of PA over $\mathbb{N}$, which is *constructively well-defined* with respect to *strong* finitary assignments of algorithmically computable Tarskian truth values T_B to the formulas of PA under $\boldsymbol{B}$; and which can be viewed as circumscribing the ambit of *finitary* mechanistic reasoning about 'true' arithmetical propositions, where (see also §21.2):

Definition 3.2. An interpretation $\mathcal{I}$ of a formal language L, over a domain D of a structure $\mathcal{S}$, is constructively well-defined relative to an assignment of truth values $T_{\mathcal{I}}$ to the formulas of L if, and only if, the provable formulas of L interpret as true over D under $\mathcal{I}$ relative to the assignment of truth values $T_{\mathcal{I}}$.

be systematically developed along the common grounds before the unconstructive elements are brought in."

CHAPTER 4

Hilbert's and Brouwer's interpretations of quantification

We begin by noting that, in [**Hi27**], Hilbert defined a formal logic L_{ε} in which he sought to capture the essence:

— of Aristotle's *unspecified* x in Definition 3.1,

— as an *unspecified* term $[\varepsilon_x(F(x))]$.

Hilbert then defined:

- $[(\forall x)F(x) \leftrightarrow F(\varepsilon_x(\neg F(x)))]$
- $[(\exists x)F(x) \leftrightarrow F(\varepsilon_x(F(x)))]$

and showed that Aristotle's logic is a well-defined interpretation of L_{ε}:

— if $[\varepsilon_x(F(x))]$ can be interpreted as some, *unspecified*, x satisfying $F(x)$.

4.1. Hilbert's interpretation of quantification

Formally, Hilbert interpreted quantification in terms of his ε-function as follows:

> "IV. The logical ε-axiom
>
> 13. $A(a) \to A(\varepsilon(A))$
>
> Here $\varepsilon(A)$ stands for an object of which the proposition $A(a)$ certainly holds if it holds of any object at all; let us call ε the logical ε-function.
>
> 1. By means of ε, "all" and "there exists" can be defined, namely, as follows:
>
>   (i) $(\forall a)A(a) \leftrightarrow A(\varepsilon(\neg A))$
>
>   (ii) $(\exists a)A(a) \leftrightarrow A(\varepsilon(A)) \ldots$
>
>   On the basis of this definition the ε-axiom IV(13) yields the logical relations that hold for the universal and the existential quantifier, such as:
>
> $(\forall a)A(a) \to A(b)$ ... (Aristotle's dictum),
>
>   and:
>
> $\neg((\forall a)A(a)) \to (\exists a)(\neg A(a))$ ... (principle of excluded middle)."
>
> ... *Hilbert:* [**Hi27**].

Thus, Hilbert's interpretation (i) of universal quantification—under any objective (i.e., *evidence-based*) method T_H of assigning truth values to the sentences of a formal logic L—is that the sentence $(\forall x)F(x)$ can be defined as holding (presumably under a well-defined interpretation $\boldsymbol{H}$ of L with respect to T_H) if, and only if, $F(a)$ holds whenever $\neg F(a)$ holds for some *unspecified* a (under $\boldsymbol{H}$); which would imply that $\neg F(a)$ does not hold for any specified a (since $\boldsymbol{H}$ is well-defined), and so $F(a)$ holds for any specified a (under $\boldsymbol{H}$).

Further, Hilbert's interpretation (ii) of existential quantification, with respect to T_H, postulates that $(\exists x)F(x)$ holds (under $\boldsymbol{H}$) if, and only if, $F(a)$ holds for some *unspecified* a (under $\boldsymbol{H}$).

4.2. Brouwer's objection

Brouwer's objection to such an *unspecified* and 'postulated' interpretation of quantification was that, for an interpretation to be considered *constructively well-defined* relative to T_H when the domain of the quantifiers under an interpretation is infinite, the decidability of the quantification under the interpretation must be constructively verifiable in some intuitively, and mathematically acceptable, sense of the term 'constructive' ([**Br08**]).

Two questions arise:

(a) Is Brouwer's objection relevant today?
(b) If so, can we interpret quantification finitarily?

4.3. Is the PA-formula $[(\forall x)F(x)]$ to be interpreted *weakly* or *strongly*?

The perspective we choose for addressing these issues is that of the structure $\mathbb{N}$, defined by:

- $\{\mathbb{N}$ (*the set of natural numbers*);
- $=$ (*equality*);
- S (*the successor function*);
- $+$ (*the addition function*);
- $*$ (*the product function*);
- 0 (*the null element*)$\}$

which serves for a definition (see §A, Appendix A) of today's standard interpretation $\boldsymbol{M}$ of the first-order Peano Arithmetic PA.

However, if we are to avoid intuitionistic objections to the admitting of *unspecified* natural numbers in the definition of quantification under $\boldsymbol{M}$, we are faced with the ambiguity where if:

— $[(\forall x)F(x)]$ and $[(\exists x)F(x)]$ denote PA-formulas; and
— The relation $F^*(x)$ denotes the interpretation in the standard interpretation $\boldsymbol{M}$ of the PA-formula $[F(x)]$ under an inductive assignment of Tarskian truth values T_M; where
— The underlying first-order logic FOL of PA favours *evidence-based* interpretation (as introduced in [**An12**] and [**An16**]; see also Chapter 5),

then the question arises (see also Chapter 21):

(a) Is the PA-formula $[(\forall x)F(x)]$ to be interpreted *weakly* as:
  - '*For any n, $F^*(n)$*',

— which holds if, and only if,

— for *any specified* n in $\mathbb{N}$,

— there is *algorithmic evidence* that $F^*(n)$ holds in $\mathbb{N}$,

or:

(b) is the formula $[(\forall x)F(x)]$ to be interpreted *strongly* as:

- '*For all n,* $F^*(n)$',

— which holds if, and only if,

— there is *algorithmic evidence* that,

— for *any specified* n in $\mathbb{N}$,

— $F^*(n)$ holds in $\mathbb{N}$?

where:

DEFINITION 4.1. A natural number n in $\mathbb{N}$ is defined as *specifiable* if, and only if, it can be explicitly denoted as a PA-numeral by a PA-formula that interprets as an algorithmically computable constant.

We note that, if we accept the Church-Turing Thesis (see Chapter 12), then admitting a natural number as *unspecified* in $\mathbb{N}$ (as in Definition 3.1) implies that, by definition 4.1, it is *specifiable* in PA and, ipso facto, *specified* under any well-defined interpretation of PA.

In other words (compare with the conclusions in §15.2) to §15.7):

THEOREM 4.2. *The Church-Turing Thesis is stronger than Aristotle's particularisation.* □

4.4. The standard interpretation $\boldsymbol{M}$ of PA interprets $[(\forall x)F(x)]$ *weakly*

Keeping the above distinction in mind, it would seem that classically, under the standard interpretation $\boldsymbol{M}$ of PA:

(1a) The formula $[(\forall x)F(x)]$ is *defined* as true in $\boldsymbol{M}$ relative to T_M if, and only if, for any *specified* natural number n, we may conclude on the basis of *evidence-based* reasoning that the proposition $F^*(n)$ holds in $\boldsymbol{M}$;

(1b) The formula $[(\exists x)F(x)]$ is an abbreviation of $[\neg(\forall x)\neg F(x)]$, and is *defined* as true in $\boldsymbol{M}$ relative to T_M if, and only if, it is not the case that, for any *specified* natural number n, we may conclude on the basis of *evidence-based* reasoning that the proposition $\neg F^*(n)$ holds in $\boldsymbol{M}$;

(1c) The proposition $F^*(n)$ is *postulated* as holding in $\boldsymbol{M}$ for some *unspecified* natural number n if, and only if, it is not the case that, for any *specified* natural number n, we may conclude on the basis of *evidence-based* reasoning that the proposition $\neg F^*(n)$ holds in $\boldsymbol{M}$.

If we *assume* that Aristotle's particularisation holds under the standard interpretation $\boldsymbol{M}$ of PA (as defined in §A, Appendix A), then (1a), (1b) and (1c) together interpret $[(\forall x)F(x)]$ and $[(\exists x)F(x)]$ under $\boldsymbol{M}$ *weakly* as intended by Hilbert's ε-function; whence they attract Brouwer's objection.

This would, then, answer question §4.2(a).

4.5. A finitary interpretation $\boldsymbol{B}$ of PA which interprets $[(\forall x)F(x)]$ *strongly*

Now, our thesis is that the implicit target of Brouwer's objection[1] is the unqualified semantic *postulation* of Aristotle's particularisation entailed by §4.4(1c), which appeals to Platonically non-constructive, rather than intuitively constructive, plausibility.

We note that this conclusion about Brouwer's essential objection apparently differs from conventional intuitionistic wisdom (i.e., perspectives based essentially on Brouwer's explicitly stated objection to the Law of the Excluded Middle as expressed in [**Br23**], p.335-336):

— which would presumably deny appeal to §4.4(1c) in an interpretation of FOL by denying the FOL theorem $[P\ v\ \neg P]$ (*Law of the Excluded Middle*);

— even though denying appeal to §4.4(1c) in an interpretation of FOL does *not* entail denying the FOL theorem $[P\ v\ \neg P]$ (*Law of the Excluded Middle*).

We can thus re-phrase question §4.2(b) more specifically:

- Can we define an interpretation of PA over $\mathbb{N}$ that does not appeal to (1c)?

We note that we *can*, indeed, define another—hitherto unsuspected—*evidence-based* interpretation $\boldsymbol{B}$ of PA under an inductive assignment of Tarskian truth values T_B over the structure $\mathbb{N}$, where (see Chapter 9):

(2a) The formula $[(\forall x)F(x)]$ is *defined* as true in $\boldsymbol{B}$ relative to T_B if, and only if, we may conclude on the basis of *evidence-based* reasoning that, for any *specified* natural number n, the proposition $F^*(n)$ holds in $\boldsymbol{B}$;

(2b) The formula $[(\exists x)F(x)]$ is an abbreviation of $[\neg(\forall x)\neg F(x)]$, and is *defined* as true in $\boldsymbol{B}$ relative to T_B if, and only if, we may conclude on the basis of *evidence-based* reasoning that it is not the case, for any *specified* natural number n, that the proposition $\neg F^*(n)$ holds in $\boldsymbol{B}$.

We note that $\boldsymbol{B}$ is a *strong* finitary interpretation of PA since—when interpreted suitably—all theorems of first-order PA interpret as *finitarily* true in $\boldsymbol{B}$ relative to T_B (see §9.1, Theorem 9.7).

This answers question §4.2(b).

[1] And perhaps of parallel objections perceived generically as "*Limitations of first-order logic*"; [**AR02b**], p.78, §2.1.

CHAPTER 5

Evidence-based reasoning

We shall now proceed to justify that the structure $\mathbb{N}$ can, indeed, be used to define both the *weak* standard interpretation $\boldsymbol{M}$ as outlined in §4.4, and a *strong* finitary interpretation $\boldsymbol{B}$ of PA as outlined in §4.5.

We shall show that, from the PA-provability of $[\neg(\forall x)F(x)]$, we may *only* conclude under the finitary interpretation $\boldsymbol{B}$, on the basis of *evidence-based* reasoning, that it is not the case that $[F(n)]$ interprets as always true in $\mathbb{N}$.

We may *not* conclude further, in the absence of *evidence-based* reasoning, that $[F(n)]$ interprets as false in $\mathbb{N}$ for some numeral $[n]$.

More precisely, we may not conclude from the PA-provability of $[\neg(\forall x)F(x)]$, in the absence of *evidence-based* reasoning, that the proposition $F^*(n)$ does not hold in $\mathbb{N}$ for some *unspecified* natural number n, since we shall show that PA is *not* ω-consistent (Corollary 11.6).

We therefore address the question:

Query 5.1. Are both the interpretations $\boldsymbol{M}$ and $\boldsymbol{B}$ of PA over the structure $\mathbb{N}$ *well-defined*, in the sense that the PA axioms interpret as true, and the rules of inference preserve truth, relative to each of the assignments of truth values T_M and T_B respectively?

5.1. Are *both* interpretations $\boldsymbol{M}$ and $\boldsymbol{B}$ of PA over $\mathbb{N}$ *well-defined*?

We begin by noting that the two interpretations $\boldsymbol{M}$ and $\boldsymbol{B}$ of PA over the structure $\mathbb{N}$ can be viewed as complementary, since (see [**An16**], §3, p.37; also Chapter 6) Tarski's classic definitions permit an intelligence—whether human or mechanistic—to admit finitary, *evidence-based*, inductive definitions of the satisfaction and truth of the *atomic* formulas of the first-order Peano Arithmetic PA, over the domain $\mathbb{N}$ of the natural numbers, in two, hitherto unsuspected and essentially different, ways:

(1) in terms of *weak* algorithmic *verifiabilty*; and

(2) in terms of *strong* algorithmic *computability.*

Thus the PA formula $[(\forall x)F(x)]$, if intended to be read as *'For any x, $F(x)$'* (see §4.3), must be consistently interpreted *weakly* in terms of algorithmic *verifiability*, defined as follows (cf. Definition 21.1):

Definition 5.2. A number-theoretical relation $F^*(x)$ is algorithmically *verifiable* if, and only if, for any *specified* natural number n, there is a deterministic algorithm $AL_{(F,\ n)}$ which can provide evidence for deciding the truth/falsity of each proposition in the finite sequence $\{F^*(1), F^*(2), \ldots, F^*(n)\}$.

Whereas if $[(\forall x)F(x)]$ is intended to be read as *'For all x, $F(x)$'*, then it must be consistently interpreted *strongly* in terms of algorithmic *computability*, defined as follows (cf. Definition 21.2):

DEFINITION 5.3. A number theoretical relation $F^*(x)$ is algorithmically *computable* if, and only if, there is a deterministic algorithm AL_F that can provide evidence for deciding the truth/falsity of each proposition in the denumerable sequence $\{F^*(1), F^*(2), \ldots\}$.

We note that *strong* algorithmic *computability* implies the existence of an algorithm that can *finitarily* decide the truth/falsity of each proposition in a constructively well-defined denumerable sequence of propositions, whereas *weak* algorithmic *verifiability* does not imply the existence of an algorithm that can *finitarily* decide the truth/falsity of each proposition in a constructively well-defined denumerable sequence of propositions[1].

Comment: We note that since a deterministic algorithm computes a mathematical function which has a unique value for any input in its domain, and the algorithm is a process that produces this particular value as output, it can be suitably defined as a '*realizer*' in the sense of the *Brouwer-Heyting-Kolmogorov* rules (see [**Ba16**], p.5).

Although in a mathematically more rigorous treatment the two Definitions 5.2 and 5.3 may need to be expressed more precisely in terms, for instance, of 'verifiable realizability' and 'algorithmic realizability'—as suggested in §13.3.1—instead of 'algorithmic verifiability' and 'algorithmic computability', we have preferred the latter terminology as more illuminating from the perspective of this introductory investigation into the philosophical and mathematical significance of *evidence-based* reasoning.

5.2. Algorithmically *verifiable* but not algorithmically *computable*

The following argument now confirms that although every algorithmically *computable* relation is algorithmically *verifiable*, the converse is not true:

THEOREM 5.4. *There are number theoretic functions that are algorithmically verifiable but not algorithmically computable.*

PROOF. We note that:

(a) Since any real number R is mathematically definable as the unique limit of a correspondingly unique Cauchy sequence[2]:

$$\{\Sigma_{i=0}^{n} r(i).2^{-i} : n = 0, 1, \ldots;\ r(i) \in \{0, 1\}\}$$

of rational numbers in binary notation:

- Let $r(n)$ denote the n^{th} digit in the decimal expression of the real number $R = Lt_{n\to\infty}\Sigma_{i=0}^{n} r(i).2^{-i}$ in binary notation.

---

[1] The distinction between the concepts of *weak* 'algorithmic verifiability' and *strong* 'algorithmic computability' seeks to eliminate an implicit ambiguity in the classical concept of 'realizability' in [**Ba16**], p.5 (see §21; also [**Kl52**], p.503).

[2] As defined in §A.

- Then, for any *specified* natural number n, Gödel's β-function (see §19.2) defines an algorithm $AL_{(R,\ n)}$ that can verify the truth/falsity of each proposition in the finite sequence:

  $\{r(0) = 0, r(1) = 0, \ldots, r(n) = 0\}$.

Hence, for any real number R, the relation $r(x) = 0$ is algorithmically *verifiable* trivially by Definition 5.2.

(b) Since it follows from Alan Turing's Halting argument ([**Tu36**], p.132, §8) that there are algorithmically uncomputable real numbers:

- Let $r(n)$ denote the n^{th} digit in the decimal expression of an algorithmically *uncomputable* real number R in binary notation.
- By (a), the relation $r(x) = 0$ is algorithmically verifiable trivially.
- However, by definition there is no algorithm AL_R that can decide the truth/falsity of each proposition in the denumerable sequence:

  $\{r(0) = 0, r(1) = 0, \ldots\}$.

Hence, although the relation $r(x) = 0$ is algorithmically *verifiable*, it is not algorithmically *computable* by Definition 5.3. □

5.3. From a Brouwerian perspective

We note that the distinction between algorithmically *verifiable* number-theoretic functions (and the real numbers defined by them) and algorithmically *computable* number-theoretic functions (and the real numbers defined by them) is, prima facie, similar to the one that, according to Mark van Atten, Brouwer sought to make explicit in his 1907 PhD thesis:

> The distinction between a construction proper and a construction project was well known to Brouwer. It is essential to his notion of denumerably unfinished sets:
>
> > [H]ere we call a set denumerably unfinished if it has the following properties: we can never construct in a well-defined way more than a denumerable subset of it, but when we have constructed such a subset, we can immediately deduce from it, following some previously defined mathematical process, new elements which are counted to the original set. But from a strictly mathematical point of view this set does not exist as a whole, nor does its power exist; however we can introduce these words here as an expression for a known intention. [10, p.148; trl. 45, p.82]
>
> But in the quotations from 1947 and 1954 above we do not see Brouwer say, analogously, that sequences that are not completely defined do from a strictly mathematical point of view not exist as objects, but that terms for them are introduced as expressions for a known intention (namely, to begin and continue a construction project of a certain kind). This explains the fact noted in the latter half of Gielen, De Swart, and Veldman's reflection.
>
> Still, the distinction at the basis of De Iongh's view between construction processes that are governed by a full definition of the object under construction and those that, as a matter of principle, cannot be thus governed, is a principled one of mathematical relevance, and it is important to realise that, if a proposed axiom turns out not to hold in general, it may still hold for one of these two subclasses.
> [...]
>
> [10] L. E. J. Brouwer. Over de grondslagen der wiskunde. PhD thesis, Universiteit van Amsterdam, 1907.
>
> *... van Atten:* [**At18**]*, pp.67-68.*

Of interest here is van Atten's remark that:

> "...if a proposed axiom turns out not to hold in general, it may still hold for one of these two subclasses".

In the case of the Poincaré-Hilbert debate (see §9.3) on whether the PA Axiom Schema of Induction can be labelled 'finitary' or not, the Axiom Schema not only turns out to be algorithmically *computable* as true (i.e., 'hold in general' over the domain $\mathbb{N}$ of the natural numbers) under a *strong* finitary interpretation of PA, but also to be algorithmically *verifiable* as true (i.e., 'hold' in any finite subset of $\mathbb{N}$) under the *weak* standard interpretation of PA!

This suggests that for a proposition of a theory S to be termed as an 'axiom' that meets a minimum level of what we would intuitively label as 'constructive' in either the *proof-theoretic* or the *model-theoretic* logic of S (in the sense of the definitions of these terms in Appendix A; and of the Definitions 21.5, 21.6 and 21.7), it should be appropriately true in both senses.

CHAPTER 6

Tarski's assignment of truth-values under an interpretation

We show next that the two definitions, Definition 5.2 and Definition 5.3, correspond to two distinctly different, hitherto unsuspected, assignments of satisfaction and truth to the *compound* formulas of PA over $\mathbb{N}$—T_M and T_B—such that:

— The PA axioms are true over $\mathbb{N}$, and

— The PA rules of inference preserve truth over $\mathbb{N}$,

under both the corresponding interpretations $\boldsymbol{M}$ and $\boldsymbol{B}$.

We essentially follow Mendelson's ([**Me64**], pp.51-53) standard exposition of Tarski's inductive definitions on the 'satisfiability' and 'truth' of the formulas of a formal language under an interpretation[1] where:

Definition 6.1. If $[A]$ is an atomic formula $[A(x_1, x_2, \ldots, x_n)]$ of a formal language S, then the denumerable sequence $(a_1, a_2, \ldots)$ in the domain $\mathbb{D}$ of an interpretation $\mathcal{I}_{S(\mathbb{D})}$ of S satisfies $[A]$ if, and only if:

(i) $[A(x_1, x_2, \ldots, x_n)]$ interprets under $\mathcal{I}_{S(\mathbb{D})}$ as a unique relation $A^*(x_1, x_2, \ldots, x_n)$ in $\mathbb{D}$ for any witness $\mathcal{W}_{\mathbb{D}}$ of $\mathbb{D}$;

(ii) there is a Satisfaction Method that provides *evidence* by which any witness $\mathcal{W}_{\mathbb{D}}$ of $\mathbb{D}$ can **define** for any atomic formula $[A(x_1, x_2, \ldots, x_n)]$ of S, and any specified denumerable sequence $(b_1, b_2, \ldots)$ of $\mathbb{D}$, whether the proposition $A^*(b_1, b_2, \ldots, b_n)$ holds or not in $\mathbb{D}$;

(iii) $A^*(a_1, a_2, \ldots, a_n)$ holds in $\mathbb{D}$ for any $\mathcal{W}_{\mathbb{D}}$.

Witness: From a constructive perspective, the existence of a 'witness' as in (i) above is implicit in the usual expositions of Tarski's definitions.

Satisfaction Method: From a constructive perspective, the existence of a Satisfaction Method as in (ii) above is also implicit in the usual expositions of Tarski's definitions.

[1] *Tarski's inductive definitions*: When interpreted constructively, these are essentially *evidence-based* truth-assignments to the formulas of a first-order theory S which correspond to the *Brouwer-Heyting-Kolmogorov* rules—cited in [**Ba16**], p.5—for assigning truth-values to the interpreted propositions of S; where the truth values of 'satisfaction', 'truth', and 'falsity' are assignable inductively (but, as we shall show for the *weak* standard interpretation $\boldsymbol{M}$ of PA, not necessarily *finitarily*) to the compound formulas of a first-order theory S under an interpretation $\mathcal{I}_{S(\mathbb{D})}$ in terms of *only* the satisfiability of the atomic formulas of S over $\mathbb{D}$ (see [**Me64**], p.51; [**Mu91**]).

A constructive perspective: We highlight the word '***define***' in (ii) above to emphasise the constructive perspective underlying this investigation[2]; which is that the concepts of 'satisfaction' and 'truth' under an interpretation are to be explicitly viewed as *evidence-based* assignments by a convention that is witness-independent. A Platonist perspective would substitute 'decide' for 'define', thus implicitly suggesting that these concepts can 'exist', in the sense of needing to be discovered by some witness-dependent means—eerily akin to a 'revelation'—if the domain $\mathbb{D}$ is $\mathbb{N}$.

We further define the truth values of 'satisfaction', 'truth', and 'falsity' for the compound formulas of a first-order theory S under the interpretation $\mathcal{I}_{S(\mathbb{D})}$ in terms of *only* the satisfiability of the atomic formulas of S over $\mathbb{D}$ as follows:

Definition 6.2. A denumerable sequence s of $\mathbb{D}$ satisfies $[\neg A]$ under $\mathcal{I}_{S(\mathbb{D})}$ if, and only if, s does not satisfy $[A]$;

Definition 6.3. A denumerable sequence s of $\mathbb{D}$ satisfies $[A \rightarrow B]$ under $\mathcal{I}_{S(\mathbb{D})}$ if, and only if, either it is not the case that s satisfies $[A]$, or s satisfies $[B]$;

Definition 6.4. A denumerable sequence s of $\mathbb{D}$ satisfies $[(\forall x_i)A]$ under $\mathcal{I}_{S(\mathbb{D})}$ if, and only if, specified any denumerable sequence t of $\mathbb{D}$ which differs from s in at most the i'th component, t satisfies $[A]$;

Definition 6.5. A well-formed formula $[A]$ of $\mathbb{D}$ is true under $\mathcal{I}_{S(\mathbb{D})}$ if, and only if, specified any denumerable sequence t of $\mathbb{D}$, t satisfies $[A]$;

Definition 6.6. A well-formed formula $[A]$ of $\mathbb{D}$ is false under $\mathcal{I}_{S(\mathbb{D})}$ if, and only if, it is not the case that $[A]$ is true under $\mathcal{I}_{S(\mathbb{D})}$.

We then have that (cf. [**Me64**], pp.51-53):

Theorem 6.7. *(Satisfaction Theorem) If, for any interpretation $\mathcal{I}_{S(\mathbb{D})}$ of a first-order theory S, there is an evidence-based Satisfaction Method SM for assigning truth values to the atomic formulas of S, then:*

(i) *The Δ_0 formulas of S are decidable as either true or false (with respect to SM) over $\mathbb{D}$ under $\mathcal{I}_{S(\mathbb{D})}$;*
(ii) *If the Δ_n formulas of S are decidable as either true or as false over $\mathbb{D}$ under $\mathcal{I}_{S(\mathbb{D})}$, then so are the $\Delta(n+1)$ formulas of S.*

Proof. It follows from the above definitions that:

(a) If, for any specified atomic formula $[A(x_1, x_2, \ldots, x_n)]$ of S, it is decidable by $\mathcal{W}_{\mathbb{D}}$ whether or not a sequence $(a_1, a_2, \ldots, a_n)$ of $\mathbb{D}$ satisfies $[A(x_1, x_2, \ldots, x_n)]$ in $\mathbb{D}$ under $\mathcal{I}_{S(\mathbb{D})}$ then, for any specified compound formula $[A^1(x_1, x_2, \ldots, x_n)]$ of S containing any one of the logical constants $\neg, \rightarrow, \forall$, it is decidable by $\mathcal{W}_{\mathbb{D}}$ whether or not the sequence $(a_1, a_2, \ldots, a_n)$ of $\mathbb{D}$ satisfies $[A^1(x_1, x_2, \ldots, x_n)]$ in $\mathbb{D}$ under $\mathcal{I}_{S(\mathbb{D})}$;

[2]Compare with Löb's remarks on 'Constructive Truth': "Intuitively we require that for each event-describing sentence, $\phi_{o^\iota} n_\iota$ say (i.e. the concrete object denoted by n_ι exhibits the property expressed by ϕ_{o^ι}), there shall be an algorithm (depending on **I**, i.e. M^*) to decide the truth or falsity of that sentence." [**Lob59**], p.165.

(b) If, for any specified compound formula $[B^n(x_1, x_2, \ldots, x_n)]$ of S containing n of the logical constants $\neg, \rightarrow, \forall$, it is decidable by $\mathcal{W}_{\mathbb{D}}$ whether or not a sequence $(a_1, a_2, \ldots, a_n)$ of $\mathbb{D}$ satisfies $[B^n(x_1, x_2, \ldots, x_n)]$ in $\mathbb{D}$ under $\mathcal{I}_{S(\mathbb{D})}$ then, for any specified compound formula $[B^{(n+1)}(x_1, x_2, \ldots, x_n)]$ of S containing $n+1$ of the logical constants $\neg, \rightarrow, \forall$, it is decidable by $\mathcal{W}_{\mathbb{D}}$ whether or not the sequence $(a_1, a_2, \ldots, a_n)$ of $\mathbb{D}$ satisfies $[B^{(n+1)}(x_1, x_2, \ldots, x_n)]$ in $\mathbb{D}$ under $\mathcal{I}_{S(\mathbb{D})}$.

The theorem follows. $\square$

In other words, if the atomic formulas of of S interpret under $\mathcal{I}_{S(\mathbb{D})}$ as decidable over $\mathbb{D}$ with respect to the Satisfaction Method *SM*, then the propositions of S (i.e., the Π_n and Σ_n formulas of S in the arithmetical hierarchy) also interpret as decidable over $\mathbb{D}$ with respect to *SM*.

6.1. Decidability in PA

We note in particular that:

THEOREM 6.8. *A well-formed formula $[F(x)]$ of PA is decidable as true or false under Tarski's truth assignments if, and only if, $[F(x)]$ is algorithmically verifiable.*

PROOF. The proof follows immediately from Definitions 6.5 and 6.6, since Tarski's definitions are inductive, and a well-formed formula $[F(x)]$ of PA is decidable as true or false under the *weak* standard interpretation $\boldsymbol{M}$ of PA over $\mathbb{N}$ if, and only if, each instantiation $[F(n)]$ of $[F(x)]$ is decidable in $\mathbb{N}$. $\square$

We cannot, however, assume that the satisfaction and truth of the compound formulas of PA are always *finitarily* decidable—in the sense of being algorithmically *computable*—under the *weak* standard interpretation $\boldsymbol{M}$ of PA over $\mathbb{N}$ (as defined in §A, Appendix A), since we cannot prove *finitarily* from *only* Tarski's definitions and the assignment T_M of algorithmically *verifiable* truth values to the atomic formulas of PA under $\boldsymbol{M}$ whether, or not, a given quantified PA formula $[(\forall x_i)R]$ is algorithmically *verifiable* as true under $\boldsymbol{M}$.

We now show how Tarski's definitions yield two distinctly different, well-defined and unique, interpretations of the first-order Peano Arithmetic PA over the domain $\mathbb{N}$ of the natural numbers—contrary to perspectives as expressed, for instance, in [**Mur06**]:

> ""The above theorems show that the axiomatic characterization of satisfaction and truth is non-unique. The reason is that Tarskis conditions put on satisfaction classes are too weak and do not uniquely determine the satisfaction and truth. What more, they admit various interpretations, even mutually inconsistent on sentences! Hence the classical principle of bivalency is not any longer valued for nonstandard languages. Moreover, one can find mutually inconsistent satisfaction classes being elementarily equivalent, i.e., having the same elementary properties in the language L(PA) with predicate **S**.
>
> Let us turn to conclusions. As Gaifman (2004, p. 15) wrote:
>
> > Intended interpretations are closely related to realistic conceptions of mathematical theories. By subscribing to the standard model of natural numbers, we are committing ourselves to the objective truth or falsity of number-theoretic statements, where

these are usually taken as statements of first-order arithmetic. The standard model is supposed to provide truth-values for these statements.

Deductive systems can only yield recursively enumerable sets of theorems and therefore they can only partially capture truth in the standard model. Even more, the truth in the standard model is not arithmetically definable.

On the other hand there are nonstandard (hence unintended) models (not only for Peano arithmetic but even for the theory of the standard model N0). This shows an essential shortcoming of a formalized approach: the failure to fully determine the intended model.

An attempt to define arithmetical truth (truth for arithmetic) in a higher order theory, for example in the second-order arithmetic or its appropriate fragment where its existence can be proved, does not give a satisfactory solution. Indeed second-order arithmetic as a deductive system is incomplete and, additionally, there appears the problem of nonstandard models and interpretations.

So we are forced to attempt to characterize the concept of truth (for PA or for other theories) in an axiomatic way. But here again we encounter the phenomenon of nonstandardness. In fact, considering a nonstandard[10] model $\langle \mathcal{M}, \mathcal{S} \rangle$ for the theory Γ-PA(**S**) or its fragment we have that $\mathcal{M}$ is a nonstandard model of PA and $\mathcal{S}$ is the appropriate satisfaction class over $\mathcal{M}$, hence the satisfaction class for formulas of the language Form($\mathcal{M}$) consisting of all those elements of the universe M (standard and nonstandard numbers) that (from the point of view of $\mathcal{M}$) are (i.e., behave like) formulas (identified here with their Gödel numbers). Among them there are also nonstandard formulas, i.e., objects that formally behave like formulas but have no proper metamathematical meaning (they are formulas from the point of view of the world of $\mathcal{M}$, but not from the point of view of the real metamathematical world). Of course L(PA) $\subseteq$ Form($\mathcal{M}$) and

$S_{tr} = \{(\ulcorner \phi \urcorner, a) : \phi$ standard formula of L(PA) a M-valuation for $\phi, \mathcal{M} \models \phi[a] \subseteq S$.

But this "real" satisfaction S_{tr} (and consequently also "real" truth) cannot be arithmetically defined in ("cut" from) the satisfaction class S. Indeed, the notion of being standard is not arithmetically definable.

Theories of the type Γ-PA(**S**) have a rich variety of models. But on the other hand not every model $\mathcal{M}$ of PA can be extended to a model $\langle \mathcal{M}, \mathcal{S} \rangle$ of Γ-PA(**S**)—indeed, the structure $\mathcal{M}$ must satisfy appropriate conditions that can be characterized in the language of consistency of certain systems of ω-logic or of the transfinite induction. This shows also that the usage of satisfaction (truth) in proving theorems about natural numbers (i.e., proving properties of natural numbers in theories of the type PA$^{\Gamma - PA(S)}$) can be in a certain sense approximated by transfinite induction or by adding certain consistency statements concerning appropriate systems of ω-logic.

Moreover, even for a fixed model $\mathcal{M}$ of Peano arithmetic for which there exists a satisfaction class, the concept of satisfaction and truth cannot be uniquely determined and, even worse, not always can be defined in such a way that the required (and expected because useful) nice metamathematical properties would be satisfied. There is no uniqueness and no bivalency (for nonstandard models). But nonstandard models and nonstandard languages (generated by such models and by axiomatic approach to the concept of truth) turn out to be useful and to have an impressive spectrum of applications. In particular they can be used to establish properties of deductive systems, provide insight into fragments of Peano arithmetic as well as into (second-order) expansions of it. They can also serve as a heuristic guide for behavior of the infinity (one can code by nonstandard objects appropriate infinite sets, in particular infinite sets of standard formulas).

Note also that considering satisfaction classes and truth for the language of Peano arithmetic and attempting to characterize them axiomatically we use the whole time at the metatheoretical level Tarskis definition with respect to structures of the type $\langle \mathcal{M}, \mathcal{S} \rangle$ and the latter is understood as being defined in a non-formalized metasystem.

A general moral of our considerations is that semantics needs infinitistic means and methods. Hence finitistic tools and means proposed by Hilbert in his programme are essentially insufficient.

[10] [Footnote] It is impossible to exclude nonstandard models and to restrict ourselves to the standard one only since the latter cannot be characterized arithmetically (in an axiomatic way)."

... *Murawski:* [**Mur06**], *pp.301-302.*

6.2. An ambiguity in the standard interpretation $\boldsymbol{M}$ of PA

We note that, classically, the standard interpretation $\boldsymbol{M}$ of PA (as defined in §A, Appendix A) is taken to be the one where, in $\mathcal{I}_{S(\mathbb{D})}$:

(a) we define S as PA with the standard first-order predicate calculus FOL[3] as the underlying logic;

(b) we define $\mathbb{D}$ as the set $\mathbb{N}$ of natural numbers;

(c) we assume for any atomic formula $[A(x_1, x_2, \ldots, x_n)]$ of PA, and any specified sequence $(b_1^*, b_2^*, \ldots, b_n^*)$ in $\mathbb{N}$, that the proposition $A^*(b_1^*, b_2^*, \ldots, b_n^*)$ is decidable in $\mathbb{N}$;

(d) we define the witness $\mathcal{W}_{\mathbb{N}}$ informally as the 'mathematical intuition' of a human intelligence for whom, classically, (c) has been *implicitly* accepted as 'decidable' in $\mathbb{N}$.

We note, further, that the implicit acceptance in (d) conceals an ambiguity that needs to be eliminated by making explicit that:

LEMMA 6.9. *Any atomic formula $A^*(x_1, x_2, \ldots, x_n)$ of PA is both algorithmically verifiable, and algorithmically computable, in $\mathbb{N}$ by $\mathcal{W}_{\mathbb{N}}$.*

PROOF. We have that:

(i) It follows from Gödel's definition of the primitive recursive relation xBy ([**Go31**], p. 22(45))—where x is the Gödel number of a proof sequence in PA whose last term is the PA formula with Gödel-number y—that, if $[A(x_1, x_2, \ldots, x_n)]$ is an atomic formula of PA, we can algorithmically verify which of the instantiations $[A(a_1, a_2, \ldots, a_n)]$ and $[\neg A(a_1, a_2, \ldots, x_a)]$ is necessarily PA-provable and, ipso facto, true under $\boldsymbol{M}$. Hence $A^*(x_1, x_2, \ldots, x_n)$ is algorithmically *verifiable* in $\mathbb{N}$ by $\mathcal{W}_{\mathbb{N}}$.

(ii) If $[A(x_1, x_2, \ldots, x_n)]$ is an atomic formula of PA then, for any specified sequence of numerals $[b_1, b_2, \ldots, b_n]$, the PA formula $[A(b_1, b_2, \ldots, b_n)]$ is an atomic formula of the form $[c = d]$, where $[c]$ and $[d]$ are atomic PA

[3]We note that in FOL the string $[(\exists \ldots)]$ is defined as—and is to be treated as an abbreviation for—the PA string $[\neg(\forall \ldots)\neg]$. We do not consider the case where the underlying logic is Hilbert's formalisation of classical predicate logic in terms of his ϵ-operator ([**Hi27**], pp.465-466).

formulas that denote PA numerals. Since $[c]$ and $[d]$ are recursively defined formulas in the language of PA, it follows from a standard result[4] that $[c = d]$ is algorithmically computable as either true or false in $\mathbb{N}$ since there is an algorithm that, for any specified sequence of numerals $[b_1, b_2, \ldots, b_n]$, will give evidence whether $[A(b_1, b_2, \ldots, b_n)]$ interprets as true or false in $\mathbb{N}$. Hence $A^*(x_1, x_2, \ldots, x_n)$ is algorithmically *computable* in $\mathbb{N}$ by $\mathcal{W}_{\mathbb{N}}$.

The lemma follows.[5] □

Accordingly, in this investigation we take the usual standard interpretation $\boldsymbol{M}$ of PA to be the one where the decidability in §6.2(c) is defined *weakly* by:

DEFINITION 6.10. An atomic formula $[A]$ of PA is satisfiable under the standard interpretation $\boldsymbol{M}$ of PA if, and only if, $[A]$ is algorithmically *verifiable* as true under $\boldsymbol{M}$.

We then show that there is, additionally, a finitary interpretation $\boldsymbol{B}$ of PA (as sought by Hilbert in [**Hi00**]), where the decidability in §6.2(c) is defined *strongly* by:

DEFINITION 6.11. An atomic formula $[A]$ of PA is satisfiable under the interpretation $\boldsymbol{B}$ if, and only if, $[A]$ is algorithmically *computable* as true under $\boldsymbol{B}$.

---

[4]For any natural numbers $m,\ n$, if $m \neq n$, then PA proves $[\neg(m = n)]$ ([**Me64**], p.110, Proposition 3.6). The converse is obviously true.

[5]*Comment*: We note that, in [**An16**] (immediately after Lemma 4.1 there which corresponds to Lemma 6.9 of this investigation)—and also in [**An15**] (implicitly)—the author mistakenly postulates:

"...without proof, that (i) is consistent with, whilst (ii) is inconsistent with, the assumption of Aristotle's particularisation".

However, the ω-*in*consistency of PA implies (Corollary 15.11) that the assumption of Aristotle's particularisation does not hold in any model of PA and is, ipso facto, inconsistent with both (i) and (ii) in the proof of Lemma 6.9.

Part 2

Evidence-based interpretations of PA

CHAPTER 7

The *weak* standard interpretation $\boldsymbol{M}$ of PA

We begin by noting (cf. [**An16**], §5, p.38) that, by Definition 6.10:

THEOREM 7.1. *The atomic formulas of PA are algorithmically verifiable under the weak standard interpretation* $\boldsymbol{M}$ *of PA (as defined in* §*A, Appendix A).*

PROOF. See Lemma 6.9. □

7.1. The PA axioms are algorithmically *verifiable* as true under $\boldsymbol{M}$

The significance of defining satisfaction in terms of algorithmic *verifiability* under $\boldsymbol{M}$ is that:

LEMMA 7.2. *The PA axioms* PA_1 *to* PA_8 *(as detailed in* §*A, Appendix A) are algorithmically verifiable as true under the interpretation* $\boldsymbol{M}$*.*

PROOF. Since $[x+y]$, $[x \star y]$, $[x=y]$, $[x']$ are defined recursively (cf. [**Go31**], p.17), the PA axioms PA_1 to PA_8 interpret as recursive relations that do not involve any quantification. It follows straightforwardly from Theorem 7.1 and Tarski's definitions that, in each case, we can define a deterministic algorithm that, for any substitution of numerals for the variables in the axiom, will evidence the substituted formula as true under $\boldsymbol{M}$. □

LEMMA 7.3. *For any specified PA formula* $[F(x)]$*, the Induction axiom schema* $[F(0) \rightarrow (((\forall x)(F(x) \rightarrow F(x'))) \rightarrow (\forall x)F(x))]$ *interprets as an algorithmically verifiable true formula under* $\boldsymbol{M}$*.*

PROOF. We have that:

(a) If $[F(0)]$ interprets as an algorithmically *verifiable* false formula under $\boldsymbol{M}$ the lemma is proved.

> *Reason*: Since $[F(0) \rightarrow (((\forall x)(F(x) \rightarrow F(x'))) \rightarrow (\forall x)F(x))]$ interprets as an algorithmically *verifiable* true formula under $\boldsymbol{M}$ if, and only if, either $[F(0)]$ interprets as an algorithmically *verifiable* false formula or $[((\forall x)(F(x) \rightarrow F(x'))) \rightarrow (\forall x)F(x)]$ interprets as an algorithmically *verifiable* true formula under $\boldsymbol{M}$.

(b) If $[F(0)]$ interprets as an algorithmically *verifiable* true formula, and $[(\forall x)(F(x) \rightarrow F(x'))]$ interprets as an algorithmically *verifiable* false formula, under $\boldsymbol{M}$, the lemma is proved.

(c) If $[F(0)]$ and $[(\forall x)(F(x) \rightarrow F(x'))]$ both interpret as algorithmically *verifiable* true formulas under $\boldsymbol{M}$ then, for any natural number n, there is an algorithm which (by Definition 5.2) will evidence that $[F(n) \rightarrow F(n')]$ is an algorithmically *verifiable* true formula under $\boldsymbol{M}$.

(d) Since $[F(0)]$ interprets as an algorithmically *verifiable* true formula under $\boldsymbol{M}$, it follows for any natural number n that there is an algorithm which will evidence that each of the formulas in the finite sequence $\{[F(0),\ F(1),\ \ldots,\ F(n)\}]$ is an algorithmically *verifiable* true formula under the interpretation.

(e) Hence $[(\forall x)F(x)]$ is an algorithmically *verifiable* true formula under $\boldsymbol{M}$.

Since the above cases are exhaustive, the lemma follows. □

Comment: We note that if $[F(0)]$ and $[(\forall x)(F(x) \rightarrow F(x'))]$ both interpret as algorithmically *verifiable* true formulas under $\boldsymbol{M}$, then we can only conclude that, for any natural number n, there is an algorithm which will evidence for any $m \leq n$ that the formula $[F(m)]$ is true under $\boldsymbol{M}$.

We cannot conclude that there is an algorithm which, for any natural number n, will give evidence that the formula $[F(n)]$ is true under $\boldsymbol{M}$.

Lemma 7.4. *Generalisation preserves algorithmically verifiable truth under* $\boldsymbol{M}$.

Proof. The two meta-assertions:

'$[F(x)]$ interprets as an algorithmically *verifiable* true formula under $\boldsymbol{M}$';

and

'$[(\forall x)F(x)]$ interprets as an algorithmically *verifiable* true formula under $\boldsymbol{M}$'

both mean:

$[F(x)]$ is algorithmically *verifiable* as true under $\boldsymbol{M}$.

The lemma follows. □

It is also straightforward to see that:

Lemma 7.5. *Modus Ponens preserves algorithmically verifiable truth under* $\boldsymbol{M}$. □

We thus have that:

Theorem 7.6. *The axioms of PA are algorithmically verifiable as true under the interpretation* $\boldsymbol{M}$*, and the rules of inference of PA preserve the properties of algorithmically verifiable satisfaction/truth under* $\boldsymbol{M}$. □

Since, by Theorem 7.6, the PA-theorems interpret as algorithmically *verifiable* truths under the *weak* standard interpretation $\boldsymbol{M}$ of PA (as defined in §A, Appendix A), we further conclude by Theorem 7.1 that (see also §15.4 where we conclude that Hilbert's 'informal' proof of the consistency of arithmetic in the *Grundlagen der Mathematik*—as analysed in [**SN01**] (pp.144-145)—reasons essentially along the same lines as the preceding, and can be viewed as also establishing the following):

Theorem 7.7. *PA is weakly consistent.* □

We note that, unlike Gentzen's debatably[1] 'constructive' consistency proof for formal number theory, Theorem 7.7 is an unarguably 'constructive' proof even though it does not yield a 'finitary' proof of consistency for PA (since—as noted in §6—we cannot conclude from Theorem 7.1 whether or not a quantified formula of PA is 'finitarily' decidable as true or false under the *weak* standard interpretation $\boldsymbol{M}$).

7.2. Is the standard interpretation $\boldsymbol{M}$ of PA *finitary*?

We note, however, that the *weak* standard interpretation $\boldsymbol{M}$ of PA cannot claim to be *finitary* since (see also Corollary 11.8), by Theorem 5.4, we cannot conclude *finitarily* from Tarski's definitions whether or not a quantified PA formula $[(\forall x_i)F]$ is algorithmically *verifiable* as true under $\boldsymbol{M}$ if $[F]$ is algorithmically *verifiable* but not algorithmically *computable* under the interpretation.

Although a proof that such a PA formula exists is not obvious, we shall show (Corollary 11.5) that Gödel's 'undecidable' arithmetical formula $[R(x)]$ is algorithmically *verifiable*, but not algorithmically *computable*, under the *weak* standard interpretation $\boldsymbol{M}$ of PA.

We also note that, under the *weak* standard interpretation $\boldsymbol{M}$ of PA, the PA-provability of the formula $[\neg(\forall x)F(x)]$ entails *only* the meta-mathematical assertion:

(i) We cannot *mathematically* conclude from the axioms and rules of inference of PA that:

> For any given natural number n, there is always some deterministic algorithm which will compute $[F(n)]$ and provide evidence that the interpretation $F^*(n)$ of $[F(n)]$ under $\boldsymbol{M}$ is an algorithmically *verifiable* true arithmetical proposition in $\mathbb{N}$.

and—contrary to conventional wisdom which embraces Aristotle's particularisation (Definition 3.1)—not the meta-mathematical assertion:

(ii) We can *mathematically* conclude from the axioms and rules of inference of PA that:

> There is some deterministic algorithm which will compute $[\neg F(n)]$ and provide evidence that the interpretation $\neg F^*(n)$ of $[\neg F(n)]$ under $\boldsymbol{M}$ is an algorithmically *verifiable* true arithmetical proposition in $\mathbb{N}$.

---

[1] As Schirn and Niebergall remark in [**SN01**], p.151: "Gentzen argues in favour of the finitist admissibility of TI[ε_0] by appeal to its allegedly constructive character. We think that his line of argument depends crucially on his 'finitist' interpretation of universal quantification and that it lacks persuasive power precisely for this reason"; adding in a footnote that: "Under 'TI[ε_0]' we understand here the schema of transfinite induction up to ε_0 in the language L_{PA} of PA".

CHAPTER 8

A *weak* 'Wittgensteinian' interpretation M^{syn} of PA

Before considering the finitary interpretation $\boldsymbol{B}$ of PA where the decidability in §6.2(c) is defined *strongly* by Definition 6.11, we note that there is also a *weak* 'Wittgensteinian' interpretation $\boldsymbol{M}^{syn}$ of PA where where the decidability in §6.2(c) is defined by:

DEFINITION 8.1. An atomic formula $[A(x)]$ of PA is satisfiable under the interpretation $\boldsymbol{M}^{syn}$ if, and only if, for any substitution of a *given* PA-numeral $[n]$ for the variable $[x]$, the formula $[A(n)]$ is *provable* in PA.

The interpretation $\boldsymbol{M}^{syn}$ of PA reflects in essence the views Ludwig Wittgenstein emphasised in his 'notorious paragraph' ([**Wi78**], Appendix III 8; see also §21.3), where he seems to suggest that the 'truth' of a proposition of a mathematical system must be definable in terms of its 'provability' within the system.

8.1. Interpreting Tarski's Theorem constructively

The significance of the interpretation $\boldsymbol{M}^{syn}$ is that standard expositions of Tarski's Theorem ([**Ta35**]) appear to *implicitly* suggest[1] that—contrary to Definition 8.1—a verifiable *evidence-based* truth of the formulas of a first-order Arithmetic such as PA, under a well-defined interpretation, cannot be defined algorithmically in the Arithmetic.

> ***Tarski's Theorem***: *The set Tr of Gödel numbers of wfs of S which are true in the standard model is not arithmetical, i.e., there is no wf $A(x)$ of S such that Tr is the set of numbers k for which $A(\overline{k})$ is true in the standard model.*
> *... Mendelson:* [**Me64**], *p.151, Corollary 3.38.*

However, we now show why it follows from Gödel's reasoning in [**Go31**] that such an *implicit* inference cannot be justified by appeal to Tarski's Theorem.

8.2. Tarski's definitions of *satisfiability* and *truth* under the *weak* standard interpretation M of PA

We note first that Tarski's definitions are mathematically significant only if, for any PA-formula $[A(x)]$ and any given n in $\mathbb{N}$, we can effectively determine whether or not the interpretation $A^*(n)$ of $[A(n)]$ holds under the *weak* standard interpretation $\boldsymbol{M}$ of PA.

[1] We note that both John Lucas ([**Lu61**]) and Roger Penrose ([**Pe90**], [**Pe94**]) accept this seeming implication unquestioningly, and use it explicitly as an arguable cornerstone of their respective defence of the former's Gödelian Thesis (see also Chapter 27).

Classically, such determination is implicitly assumed to be algorithmically computable by appeal to the Church and Turing Theses. However, in this investigation we argue that, by the principle of Occam's Razor:

(i) there is no justification for such a presumption of *strong* algorithmic computability when we can define 'effective computability' in terms of *weak* algorithmic verifiability as in Definition 12.1;

(ii) the requirement of Tarski's definitions under the *weak* standard interpretation M of PA (as defined in §A, Appendix A) ought only to be *weak* algorithmic verifiability, as detailed in Chapter 7.

Thus, a formula $[A(x)]$ of PA is defined as *satisfied* under $\boldsymbol{M}$ if, and only if, for any assignment of a value n that lies within the range of the variable x in the domain $\mathbb{N}$ of $\boldsymbol{M}$, the interpretation $A^*(n)$ of $[A(n)]$ *holds* under $\boldsymbol{M}$ (Definition 6.10).

The formula $[(\forall x)A(x)]$ of PA is then defined as true under$\boldsymbol{M}$ if, and only if, $[A(x)]$ is *satisfied* under$\boldsymbol{M}$. Other definitions follow as usual (see Chapter 6).

8.3. A Tarskian definition of *satisfiability* and *truth* under a *weak* 'Wittgensteinian' interpretation $\boldsymbol{M}^{syn}$ of PA

We note next that, just as we can interpret PA without relativisation in ZF (in the sense indicated by Feferman in [**Fe92**]), we can also interpret PA in PA where—also under Tarski's standard definitions—we now define the *satisfiability* and *truth* of the formulas of PA under a *weak* 'Wittgensteinian' interpretation $\boldsymbol{M}^{syn}$ of PA over the structure of the PA numerals by appeal to the *provability* of a formula in PA.

Thus, a formula $[A(x)]$ of PA is defined as *satisfied* under $\boldsymbol{M}^{syn}$ if, and only if, for any substitution of a *given* PA-numeral $[n]$ for the variable $[x]$, the formula $[A(n)]$ is *provable* in PA (Definition 8.1).

We note that—as in the case of the *weak* standard interpretation $\boldsymbol{M}$ of PA—the requirement of Tarski's definitions under the *weak* 'Wittgensteinian' interpretation $\boldsymbol{M}^{syn}$ of PA is also only *weak* algorithmic verifiability (see Chapter 7).

The formula $[(\forall x)A(x)]$ of PA is then defined as true under $\boldsymbol{M}^{syn}$ if, and only if, $[A(x)]$ is *satisfied* under $\boldsymbol{M}^{syn}$. Other definitions follow as usual.

8.4. *Weak* arithmetic truth under $\boldsymbol{M}$ is equivalent to *weak* arithmetic truth under $\boldsymbol{M}^{syn}$

It follows that:

THEOREM 8.2. *The interpretations* $\boldsymbol{M}$ *and* $\boldsymbol{M}^{syn}$ *of PA are isomorphic.*

PROOF. By definition, the domain of the PA numerals under $\boldsymbol{M}^{syn}$ is isomorphic to the domain $\mathbb{N}$ of the natural numbers under $\boldsymbol{M}$.

Further, both $\boldsymbol{M}$ and $\boldsymbol{M}^{syn}$ are interpretations of PA such that:

(i) each predicate letter A_j^n of PA under $\boldsymbol{M}^{syn}$ interprets as an n-place relation under $\boldsymbol{M}$ in $\mathbb{N}$;

(ii) each function letter f_j^n of PA under $\boldsymbol{M}^{syn}$ interprets as an n-place operation under $\boldsymbol{M}$ in $\mathbb{N}$ (i.e., a function from $\mathbb{N}$ into $\mathbb{N}$);

(iii) each individual constant a_i of PA under $\boldsymbol{M}^{syn}$ interprets as some fixed element under $\boldsymbol{M}$ in $\mathbb{N}$;

(iv) the provable formulas of PA are locally 'true' respectively by definition under each of the interpretations $\boldsymbol{M}$ and $\boldsymbol{M}^{syn}$.

The theorem follows. □

It further follows that:

Corollary 8.3. *A formula of PA is true under the weak standard interpretation $\boldsymbol{M}$ of PA if, and only if, it is true under the weak 'Wittgensteinian' interpretation $\boldsymbol{M}^{syn}$ of PA.* □

Moreover, it also follows that, by the classical definition of a 'model' (see §A):

Corollary 8.4. *The weak standard interpretation $\boldsymbol{M}$, and the weak 'Wittgensteinian' interpretation $\boldsymbol{M}^{syn}$, are both weak models of PA.*

Proof. By Theorem 7.6, the axioms of PA interpret as true, and the PA rules of inference preserve such truth, under $\boldsymbol{M}$, which thus defines a *weak* standard model of PA. By Corollary 8.3, the axioms of PA interpret as true, and the PA rules of inference preserve such truth, under $\boldsymbol{M}^{syn}$, which too is thus a *weak* model of PA. □

8.5. PA is not ω-consistent

We note that, in order to avoid intuitionistic objections to his reasoning in his seminal 1931 paper on formally undecidable arithmetical propositions, Gödel introduced the syntactic property of ω-consistency as an explicit assumption in his formal reasoning ([**Go31**], p.23 and p.28).

> *ω-consistency*: A formal system S is ω-consistent if, and only if, there is no S-formula $[F(x)]$ for which, first, $[\neg(\forall x)F(x)]$ is S-provable and, second, $[F(a)]$ is S-provable for any specified S-term $[a]$.

We shall address the significance of such an assumption of ω-consistency for constructive mathematics in §15.1. Meanwhile, we note here that it follows from Corollary 8.4 that (see also Corollary 11.6 for an independent proof of Theorem 8.5):

Theorem 8.5. *PA is not ω-consistent.*

Proof. Assume PA is ω-consistent.

(i) If $[(\forall x)A(x)]$ is a provable formula of PA, then $[A(0)], [A(1)], [A(2)], \ldots,$ are all PA-provable and so $[(\forall x)A(x)]$ is true under $\boldsymbol{M}^{syn}$.

(ii) Hence $[\neg(\forall x)A(x)]$ cannot be PA-provable if PA is ω-consistent.

(iii) By Gödel's reasoning in [**Go31**], if PA is ω-consistent, then there is a PA-formula $[R(x)]$ such that both $[(\forall x)R(x)]$ and $[\neg(\forall x)R(x)]$ are not provable in PA, even though $[(\forall x)R(x)]$ is true under $\boldsymbol{M}^{syn}$.

(iv) Hence $[\neg(\forall x)R(x)]$ can be added to PA as an axiom without inviting inconsistency.

(v) However, if $[\neg(\forall x)R(x)]$ were to be added as a PA axiom, it would follow that $[(\forall x)R(x)]$ is not true under $\boldsymbol{M}^{syn}$—a contradiction.

The theorem follows. $\square$

CHAPTER 9

A *strong* finitary interpretation $\boldsymbol{B}$ of PA

We consider next a *strong* finitary interpretation $\boldsymbol{B}$ of PA, where the decidability in §6.2(c) is defined *strongly* by Definition 6.11, and note that (cf. [**An16**], §6, p.40):

THEOREM 9.1. *The atomic formulas of PA are algorithmically computable under the strong finitary interpretation* $\boldsymbol{B}$.

PROOF. See Lemma 6.9. □

We note that the interpretation $\boldsymbol{B}$ is finitary since:

LEMMA 9.2. *The closed formulas of PA are algorithmically computable finitarily as true or as false under* $\boldsymbol{B}$.

PROOF. The Lemma follows by finite induction from Definition 5.3, Tarski's definitions, and Theorem 9.1. □

9.1. The PA axioms are algorithmically computable as true under $\boldsymbol{B}$

The significance of defining satisfaction in terms of algorithmic computability under $\boldsymbol{B}$ as above is that:

LEMMA 9.3. *The PA axioms PA_1 to PA_8 are algorithmically computable as true under the interpretation* $\boldsymbol{B}$.

PROOF. Since $[x + y]$, $[x \star y]$, $[x = y]$, $[x']$ are defined recursively (cf. [**Go31**], p.17), the PA axioms PA_1 to PA_8 interpret as recursive relations that do not involve any quantification. It follows straightforwardly from Theorem 9.1 and Tarski's definitions that, in each case, we can define a deterministic algorithm that, for any substitution of numerals for the variables in the axiom, will evidence the substituted formula as true under $\boldsymbol{B}$. □

LEMMA 9.4. *For any specified PA formula $[F(x)]$, the Induction axiom schema $[F(0) \rightarrow (((\forall x)(F(x) \rightarrow F(x'))) \rightarrow (\forall x)F(x))]$ interprets as an algorithmically computable true formula under* $\boldsymbol{B}$.

PROOF. By Tarski's definitions:

(a) If $[F(0)]$ interprets as an algorithmically computable false formula under $\boldsymbol{B}$ the lemma is proved.

Reason: Since $[F(0) \rightarrow (((\forall x)(F(x) \rightarrow F(x'))) \rightarrow (\forall x)F(x))]$ interprets as an algorithmically computable true formula if, and only if, either $[F(0)]$ interprets as an algorithmically computable false formula, or $[((\forall x)(F(x) \rightarrow F(x'))) \rightarrow (\forall x)F(x)]$ interprets as an algorithmically computable true formula, under $\boldsymbol{B}$.

(b) If $[F(0)]$ interprets as an algorithmically computable true formula, and $[(\forall x)(F(x) \rightarrow F(x'))]$ interprets as an algorithmically computable false formula, under $\boldsymbol{B}$, the lemma is proved.

(c) If $[F(0)]$ and $[(\forall x)(F(x) \rightarrow F(x'))]$ both interpret as algorithmically computable true formulas under $\boldsymbol{B}$, then by Definition 5.3 there is an algorithm which, for any natural number n, will evidence that the formula $[F(n) \rightarrow F(n')]$ is an algorithmically computable true formula under $\boldsymbol{B}$.

(d) Since $[F(0)]$ interprets as an algorithmically computable true formula under $\boldsymbol{B}$, it follows that there is an algorithm which, for any natural number n, will evidence that $[F(n)]$ is an algorithmically computable true formula under the interpretation.

(e) Hence $[(\forall x)F(x)]$ is an algorithmically computable true formula under $\boldsymbol{B}$.

Since the above cases are exhaustive, the lemma follows. □

LEMMA 9.5. *Generalisation preserves algorithmically computable truth under* $\boldsymbol{B}$.

PROOF. The two meta-assertions:

'$[F(x)]$ interprets as an algorithmically computable true formula under $\boldsymbol{B}$';

and

'$[(\forall x)F(x)]$ interprets as an algorithmically computable true formula under $\boldsymbol{B}$'

both mean:

$[F(x)]$ is algorithmically computable as true under $\boldsymbol{M}$.

The lemma follows. □

It is also straightforward to see that:

LEMMA 9.6. *Modus Ponens preserves algorithmically computable truth under* $\boldsymbol{B}$. □

We thus have (without appeal, moreover, to Aristotle's particularisation) that:

THEOREM 9.7. *The axioms of PA are algorithmically computable as true under the interpretation* $\boldsymbol{B}$*, and the rules of inference of PA preserve the properties of algorithmically computable satisfaction/truth under* $\boldsymbol{B}$*.* □

9.2. A finitary proof of Hilbert's Second Problem

Since algorithmic computability and PA-provability are both finitary, it follows that:

COROLLARY 9.8. *The assignment T_B of algorithmically computable truth values to the formulas of PA under* $\boldsymbol{B}$ *is finitarily decidable.* □

COROLLARY 9.9. *The PA-theorems interpret as finitary truths under* $\boldsymbol{B}$*.* □

We thus have a finitary proof that (compare with Theorem 7.7):

THEOREM 9.10. *PA is strongly consistent.* □

We note—but do not consider further as it is not germane to the intent of this investigation—that Theorem 9.10 offers a partial resolution to Hilbert's Second Problem ([**Hi00**]), which asks for a finitary proof that the second order Arithmetical axioms are consistent:

> "When we are engaged in investigating the foundations of a science, we must set up a system of axioms which contains an exact and complete description of the relations subsisting between the elementary ideas of that science. ... But above all I wish to designate the following as the most important among the numerous questions which can be asked with regard to the axioms: To prove that they are not contradictory, that is, that a definite number of logical steps based upon them can never lead to contradictory results. In geometry, the proof of the compatibility of the axioms can be effected by constructing a suitable field of numbers, such that analogous relations between the numbers of this field correspond to the geometrical axioms. ... On the other hand a direct method is needed for the proof of the compatibility of the arithmetical axioms."
> ... *Newson:* [**Nw02**].

Since the subsumed logic of PA is the standard first-order logic FOL, we further conclude that:

COROLLARY 9.11. *The standard first-order logic FOL is consistent.*

9.3. The Poincaré-Hilbert debate

We note that Lemma 9.4 and Corollary 9.11 appear to dissolve the Poincaré-Hilbert debate ([**Hi27**], p.472; also [**Br13**], p.59; [**We27**], p.482; [**Pa71**], p.502-503) since:

(i) the algorithmically verifiable, non-finitary, *weak* standard interpretation $\boldsymbol{M}$ of PA validates Poincaré's argument that the PA Axiom Schema of Finite Induction could not be justified finitarily (i.e., with respect to algorithmic computability) under the classical *weak* standard interpretation of arithmetic;

whilst:

(ii) the algorithmically computable *strong* finitary interpretation $\boldsymbol{B}$ of PA validates Hilbert's belief that a finitary justification of the Axiom Schema was possible under some *strong* finitary interpretation of an arithmetic such as PA.

It now follows from Corollary 8.4 (also independently of Corollary 15.11) and Theorem 9.10 that although the *weak* standard interpretation $\boldsymbol{M}$ of PA is a model of PA (Theorem 7.6), it is not a finitary model[1] in the sense of Definition 21.7 (for an independent proof see Corollary 11.8):

[1]We note that finitists of all hues—ranging from Brouwer [**Br08**], to Wittgenstein [**Wi78**], to Alexander Yessenin-Volpin [**He04**]—have persistently questioned the assumption that the 'standard' interpretation $\boldsymbol{M}$ can be treated as a constructively well-defined model of PA (see also [**Brm07**], [**Pos13**]).

COROLLARY 9.12. *The weak standard interpretation $\boldsymbol{M}$ of PA is not a constructively well-defined model of PA.* $\square$

CHAPTER 10

Bridging Arithmetic Provability and Arithmetic Computability

> "A paradigm shift is necessary in our notion of computational problem solving, so it can provide a complete model for the services of today's computing systems and software agents."
> ... *Peter Wegner and Dina Goldin:* [**WG03**].

We note that Wegner and Goldin's arguments, in support of their above thesis in [**WG03**], seem to reflect an extraordinarily eclectic view of mathematics, combining both an implicit acceptance of, and implicit frustration at, the standard interpretations and dogmas of classical mathematical theory:

> "... Turing machines are inappropriate as a universal foundation for computational problem solving, and ... computer science is a fundamentally non-mathematical discipline. ...
>
> (Turing's) 1936 paper ... proved that mathematics could not be completely modeled by computers. ...
>
> ... the Church-Turing Thesis ... equated logic, lambda calculus, Turing machines, and algorithmic computing as equivalent mechanisms of problem solving.
>
> Turing implied in his 1936 paper that Turing machines ... could not provide a model for all forms of mathematics. ...
>
> ... Gödel had shown in 1931 that logic cannot model mathematics ... and Turing showed that neither logic nor algorithms can completely model computing and human thought."
> ... *Wegner and Goldin:* [**WG03**].

These remarks vividly illustrate the dilemma with which not only theoretical computer sciences, but all applied sciences that depend on mathematics for providing a verifiable, *evidence-based*, language to express their observations precisely, are faced:

QUERY 10.1. Are formal classical theories essentially unable to adequately express the extent and range of human cognition, or does the problem lie in the way formal theories are classically interpreted at the moment?

The former addresses the question of whether there are absolute limits on our capacity to express human cognition unambiguously; the latter, whether there are only temporal limits—not necessarily absolute—to the capacity of classical interpretations to communicate unambiguously that which we intended to capture within our formal expression.

Prima facie, applied science continues, perforce, to interpret mathematical concepts Platonically[1], whilst waiting for mathematics to provide suitable, and hopefully reliable, answers as to how best it may faithfully express its observations verifiably.

This dilemma is also reflected in Lance Fortnow's on-line rebuttal of Wegner and Goldin's thesis, and of their reasoning.

Thus Fortnow divides his faith between the standard interpretations of classical mathematics (and, possibly, the standard set-theoretical models of formal systems such as standard Peano Arithmetic), and the classical computational theory of Turing machines.

He relies on the former to provide all the proofs that matter:

> "Not every mathematical statement has a logical proof, but logic does capture everything we can prove in mathematics, which is really what matters";
> *... Fortnow: Computational Complexity, Tuesday, April 08, 2003.*

and, on the latter to take care of all essential, non-provable, truth:

> "... what we can compute is what computer science is all about".
> *... Fortnow: Computational Complexity, Tuesday, April 08, 2003.*

However, as we shall argue in §12.1, Fortnow's faith in a classical Church-Turing Thesis that ensures:

> "... Turing machines capture everything we can compute",
> *... Fortnow: Computational Complexity, Tuesday, April 08, 2003.*

may be as misplaced as his faith in the infallibility of standard interpretations of classical mathematics.

Reason: There are, prima facie, reasonably strong arguments for a Kuhnian paradigm shift; not, as Wegner and Goldin believe, in the notion of computational problem solving, but in the standard interpretations of classical mathematical concepts.

Wegner and Goldin could, though, be right in arguing that the direction of such a shift must be towards the incorporation of non-algorithmically computable effective methods into classical mathematical theory; presuming, from the following remarks, that this is, indeed, what 'external interactions' are assumed to provide beyond classical Turing-computability:

> "... that Turing machine models could completely describe all forms of computation ... contradicted Turing's assertion that Turing machines could only formalize algorithmic problem solving ... and became a dogmatic principle of the theory of computation. ...
>
> ... interaction between the program and the world (environment) that takes place during the computation plays a key role that cannot be replaced by any set of inputs determined prior to the computation. ...

[1]e.g., Lakoff and Núñez's debatable (see [**Md01**]) argument in [**LR00**] that—even though not verifiable in the sense of having an *evidence-based* interpretation—set theory is *the* appropriate language for expressing the 'conceptual metaphors' by which an individual's 'embodied mind brings mathematics into being'.

> . . . a theory of concurrency and interaction requires a new conceptual framework, not just a refinement of what we find natural for sequential [algorithmic] computing. . . .
>
> . . . the assumption that all of computation can be algorithmically specified is still widely accepted."
>
> . . . *Wegner and Goldin:* [**WG03**].

A widespread notion of particular interest, which seems to be recurrently implicit in Wegner and Goldin's assertions too, is that mathematics is a dispensable tool of science, rather than its indispensable mother tongue.

However, the roots of such beliefs may also lie in ambiguities, in the classical definitions of foundational elements, that allow the introduction of non-constructive—hence non-verifiable, non-computational, ambiguous, and essentially Platonic—elements into the standard interpretations of classical mathematics.

For instance, in a 1990 philosophical reflection, Elliott Mendelson's following remarks implicitly imply that classical definitions of various foundational elements can be argued as being either ambiguous, or non-constructive, or both:

> "Here is the main conclusion I wish to draw: it is completely unwarranted to say that CT is unprovable just because it states an equivalence between a vague, imprecise notion (effectively computable function) and a precise mathematical notion (partial-recursive function). . . . The concepts and assumptions that support the notion of partial-recursive function are, in an essential way, no less vague and imprecise than the notion of effectively computable function; the former are just more familiar and are part of a respectable theory with connections to other parts of logic and mathematics. (The notion of effectively computable function could have been incorporated into an axiomatic presentation of classical mathematics, but the acceptance of CT made this unnecessary.) . . . Functions are defined in terms of sets, but the concept of set is no clearer than that of function and a foundation of mathematics can be based on a theory using function as primitive notion instead of set. Tarski's definition of truth is formulated in set-theoretic terms, but the notion of set is no clearer than that of truth. The model-theoretic definition of logical validity is based ultimately on set theory, the foundations of which are no clearer than our intuitive understanding of logical validity. . . . The notion of Turing-computable function is no clearer than, nor more mathematically useful (foundationally speaking) than, the notion of an effectively computable function."
>
> . . . *Mendelson:* [**Me90**].

Consequently, standard interpretations of classical theory may, inadvertently, be weakening a desirable perception of mathematics as the lingua franca of scientific expression by ignoring the possibility that, since mathematics is indisputably accepted as the language that most effectively expresses and communicates semantic truth, the chasm between—at the least—semantic arithmetical truth and syntactic arithmetical provability must, of necessity, be bridgeable explicitly.

Of interest in this context is Martin Davis' argument that an unprovable truth may, indeed, be arrived at 'algorithmically'.

> "Is Mathematical Insight Algorithmic?
>
> Roger Penrose replies "no," and bases much of his case on Gödel's incompleteness theorem: it is *insight* that enables to *see* that the Gödel sentence, undecidable in a given formal system is actually true; how could this *insight* possibly be the result of an algorithm? This seemingly persuasive argument

> is deeply flawed. To see why will require looking at Gödel's theorem at a somewhat more microscopic level than Penrose permits himself. ...
>
> ... Gödel's incompleteness theorem (in a strengthened form based on work of J. B. Rosser as well as the solution of Hilbert's tenth problem) may be stated as follows:
>
>> There is an algorithm which, given any *consistent* set of axioms, will output a polynomial equation $P = 0$ which in fact has no integer solutions, but such that this fact can not be deduced from the given axioms.
>
> Here then is the true but unprovable Gödel sentence on which Penrose relies and in a simple form at that. Note that the sentence is provided by an *algorithm*. If *insight* is involved, it must be in convincing oneself that the given axioms are indeed consistent, since otherwise we will have no reason to believe that that the Gödel sentence is true."
>
> ... *Davis:* [**Da95**].

Now, what Davis is essentially critiquing here—albeit unknowingly—is Penrose's failure to recognise that Gödel's true but unprovable sentence interprets as a quantified arithmetical proposition over $\mathbb{N}$ whose truth is algorithmically verifiable *weakly* (Definition 5.2), but not algorithmically computable *strongly* (Definition 5.2), in $\mathbb{N}$.

However, it can be argued ([**An07b**], [**An07c**]) that Penrose—as well as other philosophers and scientists such as, for instance, Lucas ([**Lu61**]), Wittgenstein ([**Wi78**]) and [**Bu10**]—should not be held to serious account for such lapse, since, as illustrated by Jeff Buechner's fallacious (in view of Theorem 9.10 and Theorem 27.1) argument, it merely reflects their unquestioning faith in standard expositions of classical theory which, too, can be critiqued similarly for failing to make this distinction explicit:

> "In 1984, Putnam proposed an ingenious argument, which he claimed avoided Penrose's error and which restored the Gödel incompleteness theorems as limitative results in psychology. That his argument is invalid is argued in detail in my book *Gödel, Putnam and Functionalism* [20]. As we shall see below, even if human beings could prove the consistency of any formal system strong enough to express the truths of arithmetic, the Gödel ncompleteness theorems could not be used as limitative results in psychology. The reason is straightforward, but it has eluded most thinkers who have weighed in on the role of the Gödel theorems as limitative results in psychology.
>
> What eluded Hilary Putnam, philosophers, mathematicians, cognitive scientists, and neuroscientists is that the Gödel theorems show that no one—whether the Gödel theorems apply to them or not—can finitistically prove the consistency of Peano arithmetic *with mathematical certainty.* They do *not* show that one cannot prove the consistency of Peano Arithmetic with less than mathematical certainty. The proof relation of a formal system confers mathematical certainty upon everything that is proved in it. This importantly qualifies any claim about what can and cannot prove in a formal system. The *only* way finitary beings can achieve mathematical certainty in what they prove is to prove it in a finitary formal system. There are few results in mathematics that are proved with mathematical certainty since few mathematicians prove their results in a finitary formal system (such as first-order logic). No being—not even God—could prove a Gödel sentence with mathematical certainty in a finitary formal system. The only way to prove a Gödel sentence with mathematical certainty is to either use a stronger finitary formal system—in which case there will be a new Gödel

sentence that cannot be proved in it—or to employ an infinitary system in which one constructs infinitary proofs. The latter is within the powers of God, but it is not within the powers of finitary human beings. We cannot construct infinitary proof trees.

The upshot is that no finitary human being can use the Gödel incompleteness theorems to show there are proof-theoretic powers human cognition has that no computational device intended to simulate it can capture."
... Buechner: [**Bu10**]*, p.12.*

We also note that, in a survey of the foundations of mathematics in the 20^{th} century, V. Wictor Marek and Jan Mycielski emphasise the significance of bridging the gap between computability and provability:

"Finally let us formulate three open problems in logic and foundations which seem to us of special importance.

1. *To develop an effective automatic method for constructing proofs of mathematical conjectures, when these conjectures have simple proofs!* Interesting methods of this kind already exist but, thus far, "automated theorem proving procedures" are not dynamic in the sense that they do not use large lists of axioms, definitions, theorems and lemmas which mathematicians could provide to the computer. Also, the existing methods are not yet powerful enough to construct most proofs regarded as simple by mathematicians, and conversely, the proofs constructed by these methods do not appear simple to mathematicians.
2. *Are there natural large cardinal existence axioms LC such that ZFC + LC implies that all OD sets X of infinite sequences of 0s and 1s satisfy the axiom of determinacy AD(X)?* This question is similar to the continuum hypothesis in the sense that it is independent of ZFC plus all large cardinal axioms proposed thus far.
3. *Is it true that* PTIME $\neq$ NPTIME, or at least, that PTIME $\neq$ PSPACE? An affirmative answer to the first of these questions would tell us that the problem of constructing proofs of mathematical conjectures in given axiomatic theories (and many other combinatorial problems) cannot be fully mechanized in a certain sense."
... Marek and Mycielski: [**MM01**]*, p.467.*

We shall therefore attempt to build such a bridge explicitly, since a significant consequence of Theorem 9.7 for constructive mathematics is that it justifies the, not uncommon, belief expressed by by Christian S. Calude, Elena Calude and Solomon Marcus as follows:

"Classically, there are two equivalent ways to look at the mathematical notion of proof: logical, as a finite sequence of sentences strictly obeying some axioms and inference rules, and computational, as a specific type of computation. Indeed, from a proof given as a sequence of sentences one can easily construct a Turing machine producing that sequence as the result of some finite computation and, conversely, given a machine computing a proof we can just print all sentences produced during the computation and arrange them into a sequence."
... Calude, Calude and Marcus: [**CCS01**].

In other words, the authors seem to hold that Turing-computability of a 'proof', in the case of a mathematical proposition, ought to be treated as equivalent to the provability of its representation in the corresponding formal language.

We contrast this with the perspective in a recent article by Sieg and Walsh on the *verifiability* of formalizations of the Cantor-Bernstein Theorem in ZF—via the *proof assistant* AProS which 'allows the direct construction of formal proofs that are humanly intelligible'.

The authors briefly reaffirm conventional wisdom by emphasising the need to distinguish between proof sequences of formal mathematical languages that are computable as 'formal derivations in particular calculi', and their interpretations which are 'the informal arguments given in mathematics'; hinting obliquely that the crucial problem is finding a faithful mathematical representation of the logical inferences in informal arguments that involve 'not surprisingly, the introduction and elimination rules for logical connectives, including quantifiers':

> "The objects of proof theory are proofs, of course. This assertion is however deeply ambiguous. Are proofs to be viewed as formal derivations in particular calculi? Or are they to be viewed as the informal arguments given in mathematics?—The contemporary practice of proof theory suggests the first perspective, whereas the programmatic ambitions of the subject's pioneers suggest the second. We will later mention remarks by Hilbert (in sections 5 and 7) that clearly point in that direction. Now we refer to Gentzen who inspired modern proof theoretic work; his investigations and insights concern *prima facie* only formal proofs. However, the detailed discussion of the proof of the infinity of primes in his [Gentzen, 1936, pp. 506-511] makes clear that he is very deeply concerned with *formalizing* mathematical practice. The crucial problem is finding the atomic inference steps involved in informal arguments. The inference steps Gentzen brings to light are, perhaps not surprisingly, the introduction and elimination rules for logical connectives, including quantifiers."
>
> *. . . Sieg and Walsh:* [**SW17**].

The authors note further that:

> "When extending the effort from logical to mathematical reasoning one is led to the task of devising additional tools for the *natural formalization of proofs*. Such tools should serve to directly reflect standard mathematical practice and preserve two central aspects of that practice, namely, (1) the axiomatic and conceptual organization in support of proofs and (2) the inferential mechanisms for logically structuring them. Thus, the natural formalization in a deductive framework *verifies* theorems relative to that very framework, but it also deepens our understanding and isolates core ideas; the latter lend themselves often, certainly in our case, to a diagrammatic depiction of a proof's conceptual structure. . . . "
>
> *. . . Sieg and Walsh:* [**SW17**].

Without addressing the larger dimensions of the authors' argument—which implicitly sanctifies Gentzen's use of transfinite, set-theoretical, reasoning in formal proofs and is critically based on the thesis that (see also Chapter 18):

> "The language of set theory is, however, the *lingua franca* of contemporary mathematics and ZF its foundation."
>
> *. . . Sieg and Walsh:* [**SW17**].

we conclude from the following (Theorem 10.2) that although set theory may be *the* appropriate language for the symbolic expression of Lakoff and Núñez's 'conceptual metaphors', by which an individual's 'embodied mind brings mathematics into being' (see [**LR00**]), it is the *strong* finitary interpretation of the first-order Peano Arithmetic PA (see Theorem 9.7) that makes PA a stronger contender for the role of the *lingua franca* of adequate expression and effective communication for contemporary mathematics and its foundations, since PA allows us to bridge arithmetic provability and arithmetic computability in the sense of [**CCS01**].

10.1. A Provability Theorem for PA

Thus, we note that (cf. [**An16**], Theorem 7.1, p.41):

THEOREM 10.2. *(Provability Theorem for PA) A PA formula* $[F(x)]$ *is PA-provable if, and only if,* $[F(x)]$ *is algorithmically computable as true in* $\mathbb{N}$ *under* $\boldsymbol{B}$.

PROOF. We have by definition that $[(\forall x)F(x)]$ interprets as true under the interpretation $\boldsymbol{B}$ if, and only if, $[F(x)]$ is algorithmically computable as true in $\mathbb{N}$.

By Lemma 9.2 the closed formulas of PA are algorithmically computable finitarily as true or as false under $\boldsymbol{B}$.

By Theorem 9.7, $\boldsymbol{B}$ defines a finitary model of PA over $\mathbb{N}$ such that:

- If $[(\forall x)F(x)]$ is PA-provable, then $[F(x)]$ is algorithmically computable as true under interpretation in $\mathbb{N}$;
- If $[\neg(\forall x)F(x)]$ is PA-provable, then it is not the case that $[F(x)]$ is algorithmically computable as true under interpretation in $\mathbb{N}$.

Now, we cannot have that both $[(\forall x)F(x)]$ and $[\neg(\forall x)F(x)]$ are PA-unprovable for some PA formula $[F(x)]$, as this would yield the contradiction:

(i) There is a well-defined model—say $\boldsymbol{B'}$—of PA+$[(\forall x)F(x)]$ over $\mathbb{N}$ in which $[F(x)]$ is algorithmically computable as true under interpretation;

(ii) There is a well-defined model—say $\boldsymbol{B''}$—of PA+$[\neg(\forall x)F(x)]$ over $\mathbb{N}$ in which it is not the case that $[F(x)]$ is algorithmically computable as true under interpretation.

The theorem follows. □

We note that there is, however—as Gödel has demonstrated in [**Go31**]—a PA formula $[R(x)]$ that is algorithmically *verifiable* as true under the standard interpretation $\boldsymbol{M}$ of PA in $\mathbb{N}$, but not provable in PA.

It follows that the arithmetical interpretation of the PA formula $[(\forall x)R(x)]$ under $\boldsymbol{M}$—if denoted by $(\forall x)R^*(x)$—is not a logical consequence of '$R^*(0), R^*(1), \ldots, R^*(n), \ldots$' under Tarski's definition of logical consequence[2].

This is often a source of confusion in classical logic (see, for instance, [**Ed03**]), which does not distinguish between the algorithmically *verifiable* truth, and the algorithmically *computable* truth, of an assertion such as:

($\star$) 'Every natural number possesses the property R^*'

when it treats:

'$(\forall x)R^*(x) \equiv R^*(0) \wedge R^*(1) \wedge \ldots \wedge R^*(n) \wedge \ldots$'

as unambiguously symbolising the assertion ($\star$).

[2]Compare with Hilbert's ω-rule detailed in §15.2

10.2. Algorithmic ω-rule: PA is 'algorithmically' complete

It now follows from Theorem 10.2 that PA is 'algorithmically' complete in the sense that[3]:

COROLLARY 10.3. *(Algorithmic ω-Rule) If it is proved that the PA formula $[F(x)]$ interprets as an arithmetical relation $F^*(x)$ that is algorithmically computable as true for any given natural number n, then the PA formula $[(\forall x)F(x)]$ can be admitted as an initial formula (axiom) in PA.* $\square$

[3]The significance of the Algorithmic ω-Rule is detailed in §15.2

Part 3

Some consequences for constructive mathematics of the Provability Theorem for PA

CHAPTER 11

Some *evidence-based* consequences of the Provability Theorem

11.1. PA is ω-*in*consistent

A significant consequence of Theorem 10.2 is that it establishes—contrary to conventional wisdom—that PA is not ω-consistent ([**An16**], Corollary 8.4, p.42; see also Theorem 8.5 and Corollary 11.6).

Since it follows immediately from Theorem 10.2 that any two models of PA are isomorphic, we first note (cf. [**An16**], Corollary 7.2, p.41) that:

COROLLARY 11.1. *The first-order Peano Arithmetic PA is categorical with respect to algorithmic computability.* □

It follows that, contrary to [**Ka91**] and [**Ka11**] (a detailed analysis of *why* PA cannot admit non-standard models is given in §20.19):

COROLLARY 11.2. *There are no non-standard numbers in any model of PA.* □

We further note that:

LEMMA 11.3. *If* $\boldsymbol{M}$ *is the standard model of PA over* $\mathbb{N}$*, then there is a PA formula* $[F]$ *which is algorithmically verifiable as true over* $\mathbb{N}$ *under* $\boldsymbol{M}$ *even though* $[F]$ *is not PA-provable.*

PROOF. Gödel has shown in [**Go31**] how to construct an arithmetical formula with a single variable—say $[R(x)]$[1]—such that $[R(x)]$ is not PA-provable[2], but $[R(n)]$ is instantiationally PA-provable for any specified PA numeral $[n]$[3]. Hence, for any specified numeral $[n]$, Gödel's primitive recursive relation $xB\lceil[R(n)]\rceil$ must hold for some x (where $\lceil[R(n)]\rceil$ denotes the Gödel-number of the formula $[R(n)]$). The lemma follows. □

By the argument in Theorem 10.2 it further follows that:

COROLLARY 11.4. *The formula* $[\neg(\forall x)R(x)]$ *in Lemma 11.3 is PA-provable.* □

---

[1] Gödel refers to the formula $[R(x)]$ only by its Gödel number r ([**Go31**], p.25, eqn.12). Although Gödel's aim in [**Go31**] was to show that $[(\forall x)R(x)]$ is not P-provable, it follows that $[R(x)]$ is also, then, not P-provable.

[2] Which corresponds to Gödel's proof in [**Go31**] that (p.26(2)): $(n)\overline{nB_\kappa(17Gen\ r)}$ holds.

[3] Which corresponds to Gödel's proof in [**Go31**] that (p.26(2)): $(n)Bew_\kappa\left[Sb\left(r\ \begin{matrix}17\\Z(n)\end{matrix}\right)\right]$ holds.

COROLLARY 11.5. *In any model of PA, Gödel's arithmetical formula $[R(x)]$ interprets as an algorithmically verifiable, but not algorithmically computable, arithmetical function $R^*(x)$ which is always true over $\mathbb{N}$.*

PROOF. Gödel has shown that $[R(x)]$ interprets as an algorithmically verifiable arithmetical function $R^*(x)$ which is always true over $\mathbb{N}$. By Corollary 11.4 $[R(x)]$ is not algorithmically computable as always true in $\mathbb{N}$. Hence $R^*(x)$ is not algorithmically computable as always true over $\mathbb{N}$. □

We thus have another proof, independent of Theorem 8.5, that:

COROLLARY 11.6. *PA is not ω-consistent.*

PROOF. Gödel has shown that if PA is consistent, then $[R(n)]$ is PA-provable for any specified PA numeral $[n]$. By Corollary 11.4 and the definition of ω-consistency, if PA is consistent then it is *not* ω-consistent. □

We note that this conclusion is contrary to accepted dogma, since ω-consistency—or an equivalent such as Rosser's Rule C (see §15.6)—is necessary for concluding the existence of 'undecidable' arithmetical propositions. Davis, for instance, remarks that:

> "...there is no equivocation. Either an adequate arithmetical logic is ω-inconsistent (in which case it is possible to prove false statements within it) or it has an unsolvable decision problem and is subject to the limitations of Gödel's incompleteness theorem".
> ... *Davis: (*[**Da82**]*, p.129(iii)).*

11.2. Are there *semantically* undecidable arithmetical propositions?

We note that Corollary 11.4 immediately implies that[4]:

THEOREM 11.7. *There are semantically undecidable propositions of PA under the weak, classically 'standard', interpretation **M** of PA.*

PROOF. By Theorem 5.4, we cannot conclude finitarily from Tarski's definitions whether or not a quantified PA formula $[(\forall x)R]$ is algorithmically *verifiable* as always true under $\boldsymbol{M}$ if $[R]$ is algorithmically *verifiable* but not algorithmically *computable* under the interpretation $\boldsymbol{M}$.

Moreover, from §7.2, Corollary 11.4, and Corollary 11.5, we can *only* conclude that, under $\boldsymbol{M}$, the PA-provability of the formula $[\neg(\forall x)R(x)]$ entails the meta-mathematical assertion:

(i) We cannot *mathematically* conclude from the axioms and rules of inference of PA that:

> For any given natural number n, there is always some deterministic algorithm which will compute $[R(n)]$ and provide evidence that $R^*(n)$ is an algorithmically *verifiable* true arithmetical proposition in $\mathbb{N}$.

However:

---

[4]The significance of Theorem 11.7 for the physical sciences is seen in the suggested resolution that it offers of Schrödinger's putative 'cat' paradox in §29.14.

(ii) Since Gödel has shown *meta-mathematically* that the PA-formula $[R(n)]$ is PA-provable for any given PA-numeral $[n]$, it also follows that:

> For any given natural number n, there is always some deterministic algorithm that will compute $[R(n)]$ and provide evidence that $R^*(n)$ is an algorithmically *verifiable* true arithmetical proposition in $\mathbb{N}$.

The theorem follows. □

We note that according to Timm Lampert (see §21.3)—and reflecting the *evidence-based* perspective of this investigation—the need to differentiate between:

(a) the 'truth' of the formulas of a formal mathematical language L that follows by *mathematical* reasoning from the axioms and rules of inference of L under a well-defined interpretation I; and

(b) the 'truth' of the formulas of L that follows by *meta-mathematical* reasoning from the axioms and rules of inference of L under I,

is implicitly suggested in Wittgenstein's 'notorious' paragraph in [**Wi78**]:

> "The most crucial aspect of any comparison of two different types of unprovability proofs is the question of what serves as the "criterion of unprovability" (I, §15). According to Wittgenstein, such a criterion should be a purely syntactic criteria independent of any meta-mathematical interpretation of formulas. It is algorithmic proofs relying on nothing but syntactic criteria that serve as a measure for assessing meta-mathematical interpretations, not vice-versa."
>
> ... *Lampert:* [**Lam17**].

11.3. The interpretation $\boldsymbol{M}$ of PA is not constructively well-defined

We immediately conclude from Theorem 11.7, independent of Corollary 9.12, that, in the sense of Definition 21.7:

Corollary 11.8. *The weak standard interpretation $\boldsymbol{M}$ of PA is not a constructively well-defined model of PA.* □

We note that the *semantic* undecidability of Gödel's '*formally*' undecidable formula $[\neg(\forall x)R(x)]$ of PA under the *weak*, classically 'standard', interpretation $\boldsymbol{M}$ of PA in Theorem 11.7 reflects the fact that Gödel's PA-formula $[(\forall x)R(x)]$ is algorithmically *verifiable meta-mathematically* as always true over $\mathbb{N}$, but not algorithmically *verifiable mathematically* as always true over $\mathbb{N}$.

11.4. There are no *formally* undecidable arithmetical propositions

Moreover, it further follows immediately from Theorem 10.2 that:

Corollary 11.9. *There are no formally undecidable arithmetical propositions in PA.* □

In other words, the appropriate inference to be drawn from Gödel's 1931 paper ([**Go31**]), then, is no longer that there exist formally undecidable PA formulas[5] such

[5] It would follow that Wittgenstein could justifiably protest, as is implicit in his 'notorious' paragraph ([**Wi78**], Appendix III 8; see also §21.3)—albeit purely on the basis of philosophical

as $[(\forall x)R(x)]$—since $[\neg(\forall x)R(x)]$ is PA-provable by Corollary 11.4—but that we can define PA formulas which, under interpretation, are semantically *undecidable* in the sense that they are algorithmically *verifiable* as true over $\mathbb{N}$, but not algorithmically *computable* as true over $\mathbb{N}$.

11.5. The two interpretations M and B of PA are complementary

Another significant consequence of Theorem 10.2 for the conclusions drawn classically from Gödel's reasoning in [**Go31**] is that:

(a) If we assume the satisfaction and truth of the *compound* formulas of PA are always *non-finitarily* decidable under M, then this assignment corresponds to the classical *weak* standard interpretation M of PA over the domain $\mathbb{N}$ relative to the truth assignments T_M;

whilst:

(b) The satisfaction and truth of the *compound* formulas of PA are always *finitarily* decidable under the assignment B, which corresponds to the *strong* finitary interpretation B of PA over the domain $\mathbb{N}$ relative to the truth assignments T_B; from which we may further *finitarily* conclude on the basis of *evidence-based* reasoning that PA is consistent.

11.6. PA can express only algorithmically computable constants

It also follows from Corollary 11.4, Corollary 11.5, and Theorem 10.2 that:

THEOREM 11.10. *A PA formula can denote only algorithmically computable constants.*

PROOF. If we admit Aristotle's particularisation under the standard interpretation M of PA, then Corollary 11.4 implies that there is an *unspecified* natural number q for which the sentence $R^*(q)$ is algorithmically *verifiable* as false.

However, it follows from Corollary 11.5 that the PA numeral corresponding to such an *unspecified* natural number q is not explicitly definable, by any PA formula, as a first-order term of PA which can be individually denoted within a PA formula. □

Theorem 11.10 establishes that an implicit definition, such as that of a putative natural number q, may—like any definition of 'The current king of France'—be vacuous since, by Corollary 11.2 there can be no non-standard numbers in any constructively well-defined model of PA (thus contradicting [**Ka91**] and [**Ka11**], whose reasoning is refuted in §20.19).

In other words, it follows from Gödel's reasoning that a PA-numeral corresponding to a putative *unspecified* natural number q is not explicitly definable, by any PA formula, as a first-order term of PA which can be individually denoted within a PA formula[6], even though, by Gödel's definition, any putative q satisfying the

---

considerations unrelated to whether or not Gödel's formal reasoning was correct—that Gödel was wrong in concluding that his arithmetical proposition could be formally undecidable but unequivocally true under interpretation!

[6]See also [**Sl15**] for a similar, albeit independent, conclusion, based on considerations that can be viewed as a philosophical interpretation of Theorem 11.10.

definition must lie in the domain of the natural numbers that is defined completely by the semantics of Dedekind's second order Peano Postulates (see [**AR02a**]: p.7, Dedekind's Theorems 132 and 133, and p.3, Definition 3).

An immediate consequence of this is that Rosser's extension of Gödel's argument ([**Ro36**]) cannot appeal to the *eliminable* introduction of an *unspecified* PA-numeral—as an instantiation of an existential formula—into a PA-proof sequence by implicitly appealing (see, for instance, [**Me64**], p.145, Proposition 3.32) to the catalytic stratagem of Rosser's Rule C (see Appendix B [§B]; also [**Ro53**], pp.127-130), for concluding the existence of an 'undecidable' Rosser proposition (which contains an existentially quantified formula) in an arithmetic such as PA.

11.7. Philosophical implications of the Provability Theorem for PA

Philosophically, Theorem 11.10 would admit the possibility that the behaviour of algorithmically *verifiable*, but not algorithmically *computable*, functions may best describe the laws governing the quantum behaviour of some physical processes—such as that of quantum entanglement considered in the EPR argument ([**EPR35**]; see also §29.1)—which the authors Albert Einstein, Boris Podolsky and Nathan Rosen ascribe to the 'putative' existence of laws of nature that may not be expressible in any categorical language (see §29.11)—such as, for instance, PA—and are thus partially hidden from direct human cognition.

Of related interest—but not immediately obvious—is whether the 'logic' of algorithmically *verifiable*, but not algorithmically *computable*, functions mirrors what S. A. Selesnick, J. P. Rawling, and Gualtiero Piccinini describe in a recent 2017 paper as the possible logic of 'quantum' processes that may be partially hidden from direct human cognition:

> "Classical systems, which do not exhibit quantum-like behavior, follow ordinary Boolean logic. The systems we study, which may include neural systems that exhibit quantum-like behavior, have states that we call "confusable". These are states that are similar to one another but are such that their small differences may affect the system's behavior in certain ways not necessarily apparent to external systems. We call systems with confusable states *discriminating* systems; we call other (classical) systems *non-discriminating* systems. Discriminating systems and their quantum-like behavior can be described using a special non-classical logic.
>
> We shall argue that the logic intrinsic to such systems requires a small adjustment to, or deformation of, the usual Boolean logic of non-discriminating systems, where here non-discriminating means "confusable iff identical." For such a non-discriminating system, this logic, namely the collection of all possible propositions concerning the system, is the Boolean lattice of all subsets of the set of states of the system. This Boolean lattice of propositions is replaced in the "discriminating" cases of interest here with a different kind of lattice of subsets. These lattices differ in only one respect from the Boolean case, namely, they are not distributive: the meet does not distribute over the join, nor the join over the meet, an equivalent condition in any lattice. Such lattices are called *ortholattices*, the involution taking the place of complementation in the Boolean case being called in this case the *orthocomplement*. As we shall argue, this single difference, namely the non-distribution of meet over join, is sufficient to explain most if not all of the quantum-like behaviors which seem so anomalous to classical thinkers. Just as ordinary propositional calculus (**PC**) is modeled by Boolean lattices, so there is a logic modeled by ortholattices. It is called orthologic (**OL**) and was

> first studied by R. Goldblatt ... This is the logic that emerges as the correct replacement for **PC** in the models of interest, and we shall exploit various forms of its model theory to reveal quantum-like attributes of these systems. We argue that certain of these models already exhibit, in the total absence of physical trappings, such standard quantum-like classically anomalous behaviors as "quantum parallelism" (as in the fable of Schrödingers cat) "and quantum interference" (*á la* the double slit experiment), though these phenomena are not independent, both stemming from the peculiarities of quantum-like disjunction ... As examples of such models we posit the sets of states of drastically simplified versions of a "network" of the kind mentioned above. Namely, we shall, for the purpose of this paper, except in the ... simplest cases of Boolean or classical networks ..., ignore the details of the network itself, returning to it in the sequel. We are left with the state spaces of clusters of nodes, considered as discriminating systems, whose appropriate logic is **OL**. We shall find that, in analogy with the case of aggregates of non-interacting physical quanta, our logical requirements impose quantum-like behavior on such clusters, though apparently in a different form from actual quantum mechanics ...
>
> We emphasize that our considerations here refer to the kinematics of the possible spaces of states involved: that is to say, the states of affairs before the systems are "observed" or "measured." Thus the correspondent here to the problematic phenomenon known in ordinary quantum theory as the "collapse of the wave-function" does not arise in this paper. It will be addressed in the sequel."
>
> *... S. A. Selesnick, J. P. Rawling, and Gualtiero Piccinini: ([***SRP17***]).*

It is a possibility that may also have significance for the possible mathematical representation of physical phenomena involving fundamental dimensionless constants in terms of functions that are algorithmically *verifiable*, but not algorithmically *computable*[7].

For instance, Marian B. Pour-El and Ning Zhong conclude that computable initial data *can* give rise to non-computable solutions in quantum theory by considering:

> "... the three-dimensional wave equation. It is well-known that the solution $u(x, y, z, t)$ is uniquely determined by two initial conditions: the values of u and $\partial u/\partial t$ at time $t = 0$. Our question is, can computable initial data give rise to non-computable solutions? The answer is "yes," and two quite different types of noncomputability can occur. Theorem 1 below gives an example in which the solution $u(x, y, z, t)$ takes a noncomputable real value at a computable point in space-time. By contrast, Theorem 2 provides an example in which the solution maps each computable sequence of points in space-time into a computable sequence: nevertheless $u(x, y, z, t)$ is not a computable function. ...
>
> The results of this paper are related to comments of Kreisel ... asks whether existing physical theories—e.g., classical mechanics or quantum mechanics—can predict theoretically the existence of a physical constant which is not a recursive real. Previous work of the authors in this area ... was concerned with ordinary differential equations: it was proved that there exists a computable—and hence continuous—function F such that $dy/dx = F(x, y)$ has no computable solutions in any rectangle however small within its domain. In the present paper, by passing to partial differential equations, we obtain similar results with an equation which is more familiar."
>
> *... Pour-El & Zhong: ([***PZ97***]).*

[7]As conjectured in [**An13**]; see also §29.6

11.8. Why Hilbert's ε-calculus is not a conservative extension of of the first-order predicate calculus

Another significant consequence of Theorem 10.2 is that, since Hilbert's ε-calculus admits ε-terms that interpret as *unspecified* natural numbers, the calculus—contrary to conventional wisdom (see, for instance, [**Sl15**])—is not a conservative extension[8] of of the first-order predicate calculus.

COROLLARY 11.11. *Hilbert's ε-calculus is not a conservative extension of the first-order predicate calculus.*

PROOF. If Hilbert's ε-calculus were a conservative extension of the first-order predicate calculus, then it would be consistent and PA would admit Rosser's proof ([**Ro36**]) that the 'Rosser' formula—which is expressed in the language of PA and contains an existential quantifier (see Chapter 16)—is undecidable in the ε-calculus if we define the existential quantifier as in §4.1IV(13)(1)(ii). However, by Corollary 11.9, there are no undecidable PA formulas. The corollary follows. □

[8]As defined in Appendix §A.

CHAPTER 12

The Church-Turing Thesis violates *evidence-based* reasoning

We consider the significance of the Provability Theorem for PA (Theorem 10.2) for the Church-Turing Thesis and Turing's Halting problem.

It is significant that both Gödel (initially) and Alonzo Church (subsequently—possibly under the influence of Gödel's disquietitude) enunciated Church's formulation of 'effective computability' as a Thesis because Gödel was instinctively uncomfortable with accepting it as a definition that *minimally* captures the essence of *intuitive* effective computability (see [**Si97**]).

Gödel's reservations seem vindicated if we accept (as argued, for instance, in [**An06**]) that a number-theoretic function can be effectively computable instantiationally (in the sense of being algorithmically *verifiable*), but not by a uniform method (in the sense of being algorithmically *uncomputable*).

That arithmetical 'truth' too can be effectively decidable instantiationally, but *not* by a uniform method, under an appropriate interpretation of PA is speculated upon by Gödel in his famous 1951 Gibbs lecture, where he remarks[1]:

> "I wish to point out that one may conjecture the truth of a universal proposition (for example, that I shall be able to verify a certain property for any integer given to me) and at the same time conjecture that no general proof for this fact exists. It is easy to imagine situations in which both these conjectures would be very well founded. For the first half of it, this would, for example, be the case if the proposition in question were some equation $F(n) = G(n)$ of two number-theoretical functions which could be verified up to very great numbers N."
> ... *Gödel: (*[**Go51**]*).*

Such a possibility is also implicit in Turing's remarks ([**Tu36**], §9(II), p.139):

> "The computable numbers do not include all (in the ordinary sense) definable numbers. Let P be a sequence whose n-th figure is 1 or 0 according as n is or is not satisfactory. It is an immediate consequence of the theorem of §8 that P is not computable. It is (so far as we know at present) possible that any assigned number of figures of P can be calculated, but not by a uniform process. When sufficiently many figures of P have been calculated, an essentially new method is necessary in order to obtain more figures."
> ... *Turing: (*[**Tu36**]*, §9(II), p.139).*

[1]Rohit Parikh's paper [**Pa71**] on existence and feasibility can also be viewed as an attempt to investigate the consequences of expressing the essence of Gödel's remarks formally.

The need for placing such a distinction[2] on a formal basis has also been expressed explicitly on occasion. Thus, Boolos, Burgess and Jeffrey ([**BBJ03**], p. 37) define a diagonal function, d, any value of which can be decided effectively, although there is no single algorithm that can effectively compute d.

Now, the straightforward way of expressing this phenomenon should be to say that there are constructively well-defined number-theoretic functions that are effectively computable instantiationally, but not algorithmically. However, as the authors quizzically observe, such functions are labeled as uncomputable!

> "According to Turing's Thesis, since d is not Turing-computable, d cannot be effectively computable. Why not? After all, although no Turing machine computes the function d, we were able to compute at least its first few values, For since, as we have noted, $f_1 = f_2 = f_3 =$ the empty function we have $d(1) = d(2) = d(3) = 1$. And it may seem that we can actually compute $d(n)$ for any positive integer n—if we don't run out of time."
> . . . *Boolos/Burgess/Jeffrey: (*[**BBJ03**]*, p.37).*

The reluctance to treat a function such as $d(n)$—or the function $\Omega(n)$ that computes the n^{th} digit in the decimal expression of a Chaitin constant Ω[3]—as computable, on the grounds that the 'time' needed to compute it increases monotonically with n, is curious[4]; the same applies to any total Turing-computable function $f(n)$.

The only difference is that, in the latter case, we know there exists[5] a common 'program' of constant length that will compute $f(n)$ for any given natural number n; in the former, we know we may need distinctly different programs for computing $f(n)$ for different values of n, where the length of the program may, sometime, reference n.

12.1. Why the classical Church-Turing Thesis does not hold in constructive mathematics

If we accept that algorithmically *verifiable* functions may be instantiationally computable but not algorithmically computable then, since algorithmic *verifiability* is defined constructively (see Definition 5.2), the Church-Turing Thesis would not hold if we were to define:

Definition 12.1. An arithmetical function is effectively computable if, and only if, it is algorithmically *verifiable*.

That a paradigm shift may be involved in:

(1) accepting Definition 12.1; and
(2) defining algorithmic verifiability (Definition 5.2) and algorithmic computability (Definition 5.3) constructively,

[2]Parikh's distinction between 'decidability' and 'feasibility' in [**Pa71**] also appears to echo the need for such a distinction.

[3]Chaitin's Halting Probability Ω is given by $0 < \Omega = \sum 2^{-|p|} < 1$, where the summation is over all self-delimiting programs p that halt, and $|p|$ is the size in bits of the halting program p; see [**Ct75**].

[4]The incongruity of this is addressed by Parikh in [**Pa71**].

[5]The issue here seems to be that, when using language to express the abstract objects of our individual, and common, mental 'concept spaces', we use the word 'exists' loosely in three senses, without making explicit distinctions between them (see [**An07c**]).

is suggested by Lázsló Kalmár's reluctance to treat his—essentially similar—argument against the plausibility of Church's Thesis as a proof:

> "...I shall not disprove Church's Thesis. Church's Thesis is not a mathematical theorem which can be proved or disproved in the exact mathematical sense, for it states the identity of two notions only one of which is mathematically defined while the other is used by mathematicians without exact definition. Of course Church's Thesis can be masked under a definition: we call an arithmetical function effectively calculable if and only if it is general recursive, venturing however that once in the future, somebody will define a function which is on one hand, not effectively calculable in the sense defined thus, on the other hand, its value obviously can be effectively calculated for any given arguments."
>
> *... Kalmár:* [**Km59**]*, p.72.*

Making the same point somewhat obliquely, the need for introducing a formally undefined concept of effective computability into the classical Church-Turing thesis is also questioned from an unusual perspective by Saul A. Kripke, who argues that, since any mathematical computation can, quite reasonably under an unarguable 'Hilbert's thesis', be corresponded to a deduction in a first-order theory, the Church-Turing 'thesis' ought to be viewed more appropriately as an immediate corollary of Gödel's completeness theorem:

> "My main point is this: a computation is a special form of mathematical argument. One is given a set of instructions, and the steps in the computation are supposed to follow—follow deductively—from the instructions as given. *So a computation is just another mathematical deduction, albeit one of a very specialized form.* In particular, the conclusion of the argument follows from the instructions as given and perhaps some well-known and not explicitly stated mathematical premises. I will assume that the computation is a deductive argument from a finite number of instructions, in analogy to Turing's emphasis on our finite capacity. It is in this sense, namely that I am regarding computation as a special form of deduction, that I am saying I am advocating a logical orientation to the problem
>
> Now I shall state another thesis, which I shall call "Hilbert's thesis",[21] namely, that the steps of any mathematical argument can be given in a language based on first-order logic (with identity). The present argument can be regarded as either reducing Church's thesis to Hilbert's thesis, or alternatively as simply pointing out a theorem on all computations whose steps can be formalized in a first-order language.
>
> Suppose one has any valid argument whose steps can be stated in a first-order language. It is an immediate consequence of the Gödel completeness theorem for first-order logic with identity that the premises of the argument can be formalized in any conventional formal system of first-order logic. Granted that the proof relation of such a system is recursive (computable), it immediately follows in the special case where one is computing a function (say, in the language of arithmetic) that the function must be recursive (Turing computable).
>
> [...]
>
> So, to restate my central thesis: computation is a special form of deduction. If we restrict ourselves to algorithms whose instructions and steps can be stated in a first-order language (first-order algorithms), and these include all algorithms currently known, the Church-Turing characterization of the class of computable functions can be represented as a special corollary of the Gödel completeness theorem.

> 21 Martin Davis originated the term "Hilbert's thesis"; see Barwise (1974, 41). Davis's formulation of Hilbert's thesis, as stated by Barwise, is that "the informal notion of *provable* used in mathematics is made precise by the formal notion *provable in first-order logic* (Barwise, 41). The version stated here, however, is weaker. Rather than referring to provability, it is simply that any mathematical *statement* can be formulated in a first-order language. Thus it is about statability, rather than provability. For the purpose of the present paper, it could be restricted to steps of a computation.
>
> Very possibly the weaker thesis about statability might have originally been intended. Certainly Hilbert and Ackermann's famous textbook (Hilbert and Ackermann, 1928) still regards the completeness of conventional predicate logic as an open problem, unaware of the significance of the work already done in that direction. Had Gödel not solved the problem in the affirmative a stronger formalism would have been necessary, or conceivably no complete system would have been possible. It is true, however, that Hilbert's program for interpreting proofs with ε-symbols presupposed a predicate calculus of the usual form. There was of course "heuristic" evidence that such a system was adequate, given the experience of Frege, Whitehead and Russell, and others.
>
> Note also that Hilbert and Ackermann do present the "restricted calculus", as they call it, as a fragment of the second-order calculus, and ultimately of the logic of order ω. However, they seem to identifyeven the second-order calculus with set theory, and mentionthe paradoxes. Little depends on these exact historical points."
> *. . . Kripke:* [**Krp13**]*, pp.80-81 & 94.*

12.2. Qualifying the equivalence between Church's and Turing's Theses

Now we note that classical theory[6] holds that:

(a) Every Turing-computable function F is partial recursive[7], and, if F is total[8], then F is recursive ([**Me64**], p.233, Corollary 5.13).

(b) Every partial recursive function is Turing-computable ([**Me64**], p.237, Corollary 5.15).

From this, classical theory concludes that the following, essentially unverifiable (since it treats the notion of 'effective computability' as intuitive, and not definable formally) but refutable, theses (informally referred to as CT) are equivalent ([**Me64**], p.237):

> *Church's Thesis*: A number-theoretic function is effectively computable if, and only if, it is recursive ([**Me64**], p.227).
>
> *Turing's Thesis*: A number-theoretic function is effectively computable if, and only if, it is Turing-computable ([**BBJ03**], p.33).

We note however that, even classically, the above equivalence does not hold strictly, and needs further qualification. The following argument highlights this, where F is any number-theoretic function:

(i) Assume Church's Thesis. Then:

- If F is Turing-computable then, by 12.1(a), it is partial recursive. *If F is total*, then it is both recursive ([**Me64**], p.227) and, by our assumption, effectively computable.

[6] We take Elliott Mendelson [**Me64**], George Boolos et al [**BBJ03**], and Hartley Rogers [**Rg87**], as representative—in the areas that they cover—of standard expositions of classical first order logic and of effective computability (in particular, of standard Peano Arithmetic and of classical Turing-computability).

[7] As defined in §A.

[8] As defined in §A.

- If F is effectively computable then, by our assumption, it is recursive. Hence, by definition, it is partial recursive and, by 12.1(b), Turing-computable.

(ii) Assume Turing's Thesis. Then:

- If F is recursive, it is partial recursive and, by 12.1(b), Turing-computable. Hence, by our assumption, F is effectively computable.
- If F is effectively computable then, by our assumption, it is Turing-computable. Hence, by 12.1(a), it is partial recursive and, *if F is total*, then it is recursive.

The question arises:

Query 12.2. Can we assume that every partial recursive function is effectively decidable as total or not?

12.3. Turing's Halting problem

Turing addressed this issue in his seminal paper on computable numbers ([**Tu36**]), where he considered the Halting problem, which can be expressed as the query:

Query 12.3. *Halting problem for T* ([**Me64**], p.256): Given a Turing machine T, can one effectively decide, given any instantaneous description alpha, whether or not there is a computation of T beginning with *alpha*?

Turing showed that the Halting problem is unsolvable by a Turing machine, in the sense that:

Lemma 12.4. *Whether or not a partial recursive function is total is not always decidable by a Turing machine.* □

In other words, since a function is Turing-computable if, and only if, it is partially Markov-computable ([**Me64**], p.233, Corollary 5.13 & p.237, Corollary 5.15), it is essentially unverifiable algorithmically whether, or not, a Turing machine that computes a given n-ary number-theoretic function will halt classically on every n-ary sequence of natural numbers (for which it is defined) as input, and not go into a non-terminating loop for some natural number input, where:

Definition 12.5. A non-terminating loop is any repetition of the instantaneous tape description of a Turing machine during a computation.

> "An instantaneous tape description describes the condition of the machine and the tape at a given moment. When read from left to right, the tape symbols in the description represent the symbols on the tape at the moment. The internal state q_s in the description is the internal state of the machine at the moment, and the tape symbol occurring immediately to the right of q_s in the tape description represents the symbol being scanned by the machine at the moment."
>
> . . . *Mendelson: (*[**Me64**]*, p.230, footnote 1).*

12.4. *How* every partial recursive function is effectively decidable

However, we now show, as a consequence of the Provability Theorem 10.2, that every partial recursive function is effectively decidable as total or not by a trio (T_1 // T_2 // T_3) of Turing machines operating in parallel, and conclude that:

(a) The parallel trio (T_1 // T_2 // T_3) of Turing machines is not a Turing machine;

(b) The classical Church-Turing Thesis is false.

Now, we note that any Turing machine T can be provided with an auxiliary infinite tape (see [**Rg87**], p.130) to effectively recognise a non-terminating looping situation; it simply records every instantaneous tape description at the execution of each machine instruction on the auxiliary tape, and compares the current instantaneous tape description with the record.

Moreover, T can be meta-programmed to abort the impending non-terminating loop *if* an instantaneous tape description is repeated, and to return a meta-symbol indicating self-termination.

> *Comment*: It is convenient to visualise the tape of such a Turing machine as that of a two-dimensional virtual-teleprinter, which maintains a copy of every instantaneous tape description in a random-access memory during a computation.

However, it now follows from Theorem 10.2 that:

THEOREM 12.6. *It is always possible to determine whether a Turing machine will halt or not when computing any partial recursive function F.*

PROOF. We assume that the partial recursive function F is obtained from the recursive function G by means of the unrestricted μ-operator[9]; in other words, that (see [**Me64**], p.214):

$$F(x_1, \ldots, x_n) = \mu y(G(x_1, \ldots, x_n, y) = 0).$$

If $[H(x_1, \ldots, x_n, y)]$ expresses $\neg(G(x_1, \ldots, x_n, y) = 0)$ in PA we have, by definition, that any interpretation $H^*(x_1, \ldots, x_n, y)$ of $[H(x_1, \ldots, x_n, y)]$ in $\mathbb{N}$ is instantiationally equivalent to $\neg(G(x_1, \ldots, x_n, y) = 0)$ (cf. [**Me64**], p.117).

We now consider the PA-provability and Turing computability of the arithmetical formula $[H(x_1, \ldots, x_n, y)]$ by a Turing machine T that inputs every sequence of numerals $\{[a_1], \ldots, [a_n]\}$ of PA simultaneously into the parallel trio (T_1 // T_2 // T_3) of Turing machines, as below:

(a) Let Q_1 be the meta-assertion that the PA-formula $[H(a_1, \ldots, a_n, y)]$ is not algorithmically *verifiable* as always true under interpretation in $\mathbb{N}$.

It follows that there is some finite k such that $H^*(a_1, \ldots, a_n, k)$ does not hold in $\mathbb{N}$; and so $G(a_1, \ldots, a_n, k)$ holds.

Since $G(a_1, \ldots, a_n, y)$ is recursive, any Turing machine T_1 that computes $G(a_1, \ldots, a_n, y)$ will halt and return the value 0 at $y = k$.

[9] Where 'μy' interprets as 'The least y such that ...'.

(b) Let Q_2 be the meta-assertion that the PA-formula $[H(a_1, \ldots, a_n, y)]$ is algorithmically verifiable as always true, but not algorithmically computable as always true, under interpretation in $\mathbb{N}$.

Hence, for any given $[k]$, the formula $[H(a_1, \ldots, a_n, k)]$ interprets as true in $\mathbb{N}$, but there is no Turing machine that, for any given $[k]$, computes the formula $[H(a_1, \ldots, a_n, k)]$ as 'true' under interpretation in $\mathbb{N}$.

Now it follows from Theorem 10.2 that the PA-formula $[H(a_1, \ldots, a_n, y)]$ is a well-defined, hence computable, formula since every instantiation of it is PA-provable.

However, since $[H(a_1, \ldots, a_n, y)]$ is not algorithmically computable as always true under interpretation in $\mathbb{N}$, any Turing machine T_2 that computes the value of $[y]$ at which $[H(a_1, \ldots, a_n, y)]$ is true cannot return the value 'true' for all values of $[y]$.

Hence T_2 must necessarily initiate a non-terminating loop at some $[y = k']$ and halt, since its auxiliary tape will return the symbol for self-termination at $[y = k']$.

(c) Finally, let Q_3 be the meta-assertion that the PA-formula $[H(a_1, \ldots, a_n, y)]$ is algorithmically computable as always true under interpretation in $\mathbb{N}$.

Hence the Turing machine T_2 will return the value 'true' on any input for $[y]$.

Now it follows from Theorem 10.2 that $[H(a_1, \ldots, a_n, y)]$ is PA-provable.

Let h be the Gödel-number of $[H(a_1, \ldots, a_n, y)]$. We consider, then, Gödel's primitive recursive number-theoretic relation xBy ([**Go31**], p.22, definition 45), which holds if, and only if, x is the Gödel-number of a proof sequence in PA for the PA-formula whose Gödel-number is y. It follows that there is some finite k'' such that any Turing machine T_3, which computes the characteristic function of xBh, will halt and return the value 0 ('true') for $x = k''$.

Since Q_1, Q_2 and Q_3 are mutually exclusive and exhaustive, it follows that, when run simultaneously over the sequence $1, 2, 3, \ldots$ of values for y, one of the parallel trio (T_1 // T_2 // T_3) of Turing machines will always halt for some finite value of y. Moreover:

- If T_1 halts, then a Turing machine will halt when computing the partial recursive function F.
- If either one of T_2 or T_3 halts, then a Turing machine will not halt when computing the partial recursive function F.

The theorem follows. □

We conclude by Lemma 12.4 and Theorem 12.6 that:

COROLLARY 12.7. *The parallel trio of Turing machines (T_1 // T_2 // T_3) is not a Turing machine.* □

12.5. The classical Church-Turing thesis is false

An immediate consequence of Corollary 12.7 is that:

COROLLARY 12.8. *The classical Church-Turing thesis is false.* □

We note that—excepting that it always calculates the function $g(n)$ (defined below) constructively, even in the absence of a uniform procedure, within a fixed postulate system—the reasoning used in Theorem 12.6 is, essentially, the same as Selmer Bringsjord's concise expression of Kalmár's argument ([**Km59**], p.74) in his narrational case against Church's Thesis:

> "First, he draws our attention to a function g that isn't Turing-computable, given that f is[10]:
>
> $g(x) = \mu y(f(x, y) = 0) =$ the least y such that $f(x, y) = 0$ if y exists; and 0 if there is no such y
>
> Kalmár proceeds to point out that for any n in $\mathbb{N}$ for which a natural number y with $f(n, y) = 0$ exists, 'an obvious method for the calculation of the least such y ... can be given,' namely, calculate in succession the values $f(n, 0), f(n, 1), f(n, 2), \ldots$ (which, by hypothesis, is something a computist or TM can do) until we hit a natural number m such that $f(n, m) = 0$, and set $y = m$.
>
> On the other hand, for any natural number n for which we can prove, not in the frame of some fixed postulate system but by means of arbitrary—of course, correct—arguments that no natural number y with $f(n, y) = 0$ exists, we have also a method to calculate the value $g(n)$ in a finite number of steps.
>
> Kalmár goes on to argue as follows. The definition of g itself implies the tertium non datur, and from it and CT we can infer the existence of a natural number p which is such that
>
> (*) there is no natural number y such that $f(p, y) = 0$; and
> (**) this cannot be proved by any correct means.
>
> Kalmár claims that (*) and (**) are very strange, and that therefore CT is at the very least implausible."
>
> ... *Bringsjord:* [**Bri93**].

Kalmár himself argues further to the effect that the proposition stating that, for this p, there is a natural number y such that $f(p, y) = 0$, would then be *absolutely* undecidable in the sense that:

> "... the problem if this proposition holds or not, would be unsolvable, not in Gödel's sense of a proposition neither provable nor disprovable in the frame of a fixed postulate system, nor in Church's sense of a problem with a parameter for which no general recursive method exists to decide, for any given value of the parameter in a finite number of steps, which is the correct answer to the corresponding particular case of the problem, "yes" or "no". As a matter of fact, the problem, if the proposition in question holds or not, does not contain any parameter and, supposing Church's thesis, *the proposition itself can be neither proved nor disproved*, not only in the frame of a fixed postulate system, but *even admitting any correct means.* It cannot be proved for it is false and it cannot be disproved for its negation cannot be proved. According to my knowledge, this consequence of Church's thesis, viz. the existence of a proposition (without a parameter) which is undecidable in this, *really absolute sense*, has not been remarked so far.

[10]Bringsjord notes that the original proof can be found on page 741 of Kleene [**Kl36**].

However, this “absolutely undecidable proposition” has a defect of beauty: we can decide it, for we know, it is false. Hence, *Church’s thesis implies the existence of an absolutely undecidable proposition which can be decided viz., it is false, or*, in another formulation, *the existence of an absolutely unsolvable problem with a known definite solution*, a very strange consequence indeed.”

. . . Kalmár: [**Km59**]*, p. 75.*

Part 4

The significance of *evidence-based* reasoning for some grey areas in Constructive Mathematics

CHAPTER 13

Bauer's five stages of accepting constructive mathematics

"What new and relevant ideas does constructive mathematics have to offer, if any?"

... Bauer: [**Ba16**], *p.1.*

To situate the main conclusions of this investigation within a contemporary perspective, we critically review in selected detail Bauer's attempts (in [**Ba16**]) to familiarise mathematicians in general about the—seemingly paradoxical—counter-intuitive concepts that might inhibit a wider appreciation of the subject.

Bauer's thesis is that learning constructive mathematics requires one to first unlearn certain deeply ingrained intuitions and habits acquired during classical mathematical training. He characterises it as a traumatising event acceptance of which, from a psychological point of view, involves passing through the five stages identified by multi-disciplinary psychologist Elisabeth Kübler-Ross—in her book '*On Death and Dying*'—as: denial, anger, bargaining, depression and, finally, acceptance.

13.1. Denial

Bauer characterises the first stage as the one where mathematicians[1] 'summarily dismiss constructive mathematics as nonsense because they misunderstand' that although 'constructive mathematics is mathematics done without the *law of excluded middle*':

> "*For every proposition P, either P or not P.*"
>
> *... Bauer:* [**Ba16**], *p.1.*

constructivists do not deny excluded middle but are ambivalent about it.

However, he remarks that constructivists:

— deny that a proposition can be both true and false;

— deny that a proposition can be neither true nor false;

[1]Although Bauer's observation may be true of *some* mathematicians, it is more likely that most mathematicians simply offer passive 'inertial' resistance to the adoption of the constraints demanded by constructive mathematics; in the sense that—as David Hilbert's rather more actively articulated reaction (see §13.2) illustrates—the loss they anticipate in giving up what they have inherited—in good faith—under classical mathematics appears incommensurate with the gain that they can envisage by adopting constructive restraints—a phenomena well-known to economists (see, for instance, [**KKT91**], p.197) as *status quo bias.* The thesis of this investigation (see §3) is that such *fear* of a loss—of an illusory self-evident nature of 'endowed truth'—characterises current perspectives of not only classical mathematics, but also of constructive mathematics (including Bauer's in [**Ba16**]).

— deny proof by contradiction;

— admit negations are provable by reaching a contradiction;

— find certain forms of choice acceptable; and

— admit that with a bit of care some instances of excluded middle and choice can be removed, or just turn out to be illusions created by insufficient training in logic.

Bauer notes, for instance, that:

> "Confusingly, mathematicians call 'proof by contradiction' any argument which derives a contradiction from a statement believed to be false, but there are two reasoning principles that have this form. One is indeed proof by contradiction, and it goes as
>
> *Suppose* $\neg P$, *... (argument reaching contradiction) ..., therefore* P.
>
> While the other is how a negation $\neg P$ is proved:
>
> *Suppose* P, *... (argument reaching contradiction) ..., therefore* $\neg P$.
>
> Because $\neg P$ abbreviates $P \Rightarrow \perp$, the rule for proving a negation is an instance of the rule for proving an implication $P \Rightarrow Q$: assume P and derive Q. Admittedly, the two arguments look and feel similar, but notice that in one case the conclusion has a negation removed and in the other added. Unless we already believe in $\neg\neg P \Leftrightarrow P$, we cannot get one from the other by exchanging P and $\neg P$. These really are different reasoning principles."
> *... Bauer:* [**Ba16**], *p.2.*

Bauer emphasises that whereas constructive mathematics admits proof by negation, it denies proof by contradiction since:

> "Proof by contradiction, or reductio ad absurdum in Latin, is the reasoning principle:
>
> *If a proposition* P *is not false, then it is true.*
>
> In symbolic form it states that $\neg\neg P \Rightarrow P$ for all propositions P, and is equivalent to excluded middle."
> *... Bauer:* [**Ba16**], *p.2.*

Bauer further argues that:

> "In constructive mathematics we cannot afford the axiom of choice because it implies excluded middle."
> *... Bauer:* [**Ba16**], *p.3.*

Before proceeding to the next stage, Bauer attempts to clear up one last 'misconception' concerning how the existential quantifier is to be interpreted constructively (however, compare with Definition 3.1 below).

> "Suppose that in a mathematical text we have the assumption that there exists x such that $\phi(x)$. We customarily say 'choose an x satisfying $\phi(x)$' to give ourselves an x satisfying ϕ. This is not an application of the axiom of choice, but rather an elimination of an existential quantifier. Similarly, if we know that a set A is inhabited and we say 'choose $x \in A$', it is not choice but existential quantifier elimination again."
> *... Bauer:* [**Ba16**], *p.4.*

13.2. Anger

Bauer exemplifies the second stage by recalling Hilbert's words:

> "Taking the principle of excluded middle from the mathematician would be the same, say, as proscribing the telescope to the astronomer or to the boxer the use of his fists. To prohibit existence statements and the principle of excluded middle is tantamount to relinquishing the science of mathematics altogether. For compared with the immense expanse of modern mathematics, what would the wretched remnants mean, the few isolated results, incomplete and unrelated, that the intuitionists have obtained without the use of logical ε-axiom?"
>
> *... Hilbert:* [**Hi27**], *p.476.*

He counters Hilbert's tirade (which, we note in §14.1, conflates the principle of excluded middle with Aristotle's particularisation, i.e., the use of the logical ε-axiom) with the argument that:

> "It is much less known in the wider mathematical community that things changed in 1967, a year after Brouwer's death, when Erret Bishop published a book on constructive analysis. The importance of the work was best described by Michael Beeson:
>
> > *The thrust of Bishop's work was that both Hilbert and Brouwer had been wrong about an important point on which they had agreed. Namely both of them thought that if one took constructive mathematics seriously, it would be necessary to 'give up' the most important parts of modern mathematics (such as, for example, measure theory or complex analysis). Bishop showed that this was simply false, and in addition that it is not necessary to introduce unusual assumptions that appear contradictory to the uninitiated. The perceived conflict between power and security was illusory! One only had to proceed with a certain grace, instead of with Hilbert's 'boxer's fists'.*"
> >
> > *... Bauer:* [**Ba16**], *p.5.*
> >
> > *Comment*: An insight to which this investigation—in denying necessity to both the Hilbertian acceptance of Aristotle's particularisation and the Brouwerian denial of the Law of the Excluded Middle—pays homage.

Bauer traces the roots of Hilbertian rejections to the fact that, whereas no sane mathematician would reject the fact that a subset of a finite sets is finite:

> "... constructivists think that a subset of a finite set need not be finite. A cursory literature search reveals other bizarre statements considered in constructive mathematics: '$\mathbb{R}$ has measure zero' , 'there is a bounded increasing sequence without an accumulation point', 'ordinals form a set', 'there is an injection of $\mathbb{N}^{\mathbb{N}}$ into $\mathbb{N}$' , and so on."
>
> *... Bauer:* [**Ba16**], *p.6.*
>
> *Comment*: Compare with Corollary 19.5 that, from an evidence-based arithmetical perspective, $\aleph_0 \longleftrightarrow 2^{\aleph_0}$.

He defends such constructivist conclusions by arguing that:

> "A constructivist might point out that what counts as bizarre is subjective and remind us that once upon a time the discovery of non-Euclidean geometries was shelved in fear of rejection, that Weierstraß's continuous but nowhere differentiable function was and remains a curiosity, and that the Banach-Tarski theorem about conjuring two balls from one is even today called a 'paradox'."
>
> *... Bauer:* [**Ba16**], *p.6.*

13.3. Bargaining

Bauer characterises the third stage as the one that requires a classical mathematician to compromise on the intuitive notion of 'truth':

> "Classical mathematical training plants excluded middle so deeply into young students' minds that most mathematicians cannot even detect its presence in a proof. In order to gain some sort of understanding of the constructivist position, we should therefore provide a method for suspending belief in excluded middle.
>
> If a geometer tried to disbelieve Euclid's fifth postulate, they would find helpful a model of non-Euclidean geometry—an artificial world of geometry whose altered meanings of the words 'line' and 'point' caused the parallel postulate to fail.
>
> Our situation is comparable, only more fundamental because we need to twist the meaning of 'truth' itself. We cannot afford a full mathematical account of constructive worlds, but we still can distill their essence, as long as we remember that important technicalities have been omitted."
>
> *. . . Bauer:* [**Ba16**]*, p.6.*

He then claims that:

> "It is well worth pointing out that constructive mathematics is a generalization of classical mathematics, as was emphasized by Fred Richman, for a proof which avoids excluded middle and choice is still a classical proof. However, trying to learn constructive thinking in the classical world is like trying to learn noncommutative algebra by studying abelian groups."
>
> *. . . Bauer:* [**Ba16**]*, p.6.*

Bauer expands on the need of constructive mathematics to 'twist' the meaning of 'truth' as necessitated by the differing modes of truth-assignments required by the gamut of differing constructive worlds which—as Bauer ruefully notes in the fourth stage (dramatically namely 'Depression')—a constructive mathematics that claims to generalise classical mathematics is compelled to accommodate.

13.3.1. Realizability. He then addresses two such assignments, the first of which appeals to the computable properties of realisability.

> "In our first honestly constructive world only that is true which can be computed. Let us imagine, as programmers do, that mathematical objects are represented on a computer as data, and that functions are programs operating on data. Furthermore, a logical statement is only considered valid when there is a program witnessing its truth. We call such programs realizers, and we say that statements are realized by them. The *Brouwer-Heyting-Kolmogorov* rules explain when a program realizes a statement:
>
> (1) falsehood $\bot$ is not realized by anything;
> (2) truth $\top$ is realized by a chosen constant, say $\star$;
> (3) $P \vee Q$ is realized by a pair (p, q) such that p is a realizer of P and q of Q;
> (4) $P \wedge Q$ is realized either by $(0, p)$, where p realizes P, or by $(1, q)$, where q realizes Q;
> (5) $P \Rightarrow Q$ is realized by a program which maps realizers of P to realizers of Q;
> (6) $\forall x \in A.P(x)$ is realized by a program which maps (a representation of) any $a \in A$ to a realizer of $P(a)$;

(7) $\exists x \in A.P(x)$ is realized by a pair (p, q) such that p represents some $a \in A$ and q realizes $P(a)$;
(8) $a = b$ is realized by a p which represents both a and b.

The rules work for any reasonable notion of 'program'. Turing machines would do, but so would quantum computers and programs actually written by programmers in practice."

... Bauer: [**Ba16**], *pp.6-7.*

As examples of the use of realizers, Bauer first offers an example of the computational interpretation of universal quantification:

"*For every natural number there is a prime larger than it.*

This is a 'for all' statement, so its realizer is a program p which takes as input a natural number n and outputs a realizer for 'there is a prime larger than n', which is a pair (m, q) where m is again a number and q realizes 'm is prime and $m > n$'. If we forget about q, we see that p is essentially a program that computes arbitrarily large primes. Because such a program exists, there are arbitrarily large primes in the computable world."

... Bauer: [**Ba16**], *p.7.*

He then proffers as a more interesting example:

"(1) $\forall x \in R.x = 0 \vee x \neq 0$.

If we define real numbers as the Cauchy completion of rational numbers, then a real number $x \in R$ is represented by a program p which takes as input $k \in N$ and outputs a rational number r_k such that $|x - r_k| \leq 2^{-k}$. Thus a realizer for (1) is a program q which accepts a representation p for any $x \in \mathbb{R}$ and outputs either $(0, s)$ where s realizes $x = 0$, or $(1, t)$ where t realizes $x \neq 0$. Intuitively speaking, such a q should not exist, for however good an approximation r_k of x the program q calculates, it may never be sure whether $x = 0$. To make a water-tight argument, we shall use q to construct the Halting oracle, which does not exist. (The usual proof of nonexistence of the Halting oracle is yet another example of a constructive proof of negation.) Given a Turing machine T and an input n, define the sequence $r_0, r_1, r_2, \ldots$ of rational numbers by

- $r_k = 2^{-j}$ if $T(n)$ halts at step j and $j \leq k$,
- $r_k = 2^{-k}$ otherwise.

This is a Cauchy sequence because $| r_k - r_m | \leq 2^{-min(k,m)}$ for all $k, m \in N$, and it is computable because the value of r_k may be calculated by a simulation of at most k steps of execution of $T(n)$. The limit $x = \lim_k r_k$ satisfies

- $x = 2^{-j} > 0$, if $T(n)$ halts at step j,
- $x = 0$, if $T(n)$ never halts.

The program p which outputs r_k on input k represents x because $|x - r_k| \leq 2^{-k}$ for all $k \in N$. We may now decide whether $T(n)$ halts by running $q(p)$: if it outputs $(0, s)$, then $T(n)$ does not halt, and if it outputs $(1, t)$, then $T(n)$ halts."

... Bauer: [**Ba16**], *pp.7-8.*

Bauer notes that although the above argument needs:

"the following (valid) instance of excluded middle: for every $k \in \mathbb{N}$, either $r_k = 2^{-k}$ or $r_k = 2^{-j}$ for some $j < k$"

... Bauer: [**Ba16**], *p.8.*

the statement (1) is an instance of excluded middle which is not realized.

He concludes with an anti-mechanist thesis that echoes—albeit for debatable reasons—a concluding thesis of this investigation (Theorem 27.1):

> "The strategy to place constructivism inside a box is working! If one takes the limited view that everything must be computed by machines, then excluded middle fails because machines cannot compute everything. Our excluded middle is not affected because we are not machines."
> *. . . Bauer:* [**Ba16**]*, p.8.*

Bauer uses the computable world to further explain why the following instance of 'subsets of finite sets are finite' is not realized:

> "(2) *All countable subsets of* $0, 1$ *are finite.*
>
> In computable mathematics a finite set is represented by a finite list of its elements, and a countable set by a program which enumerates its elements, possibly with repetitions. The subsets $\{\}$, $\{0\}$, $\{1\}$ and $\{0, 1\}$ are all countable and finite, so (2) looks pretty true. Remember though that in the computable world 'for all' means not 'it holds for every instance' but rather 'there is a program computing witnesses from instances'. A realizer for (2) is a program q which takes as input a program p enumerating the elements of a subset of $\{0, 1\}$ and outputs a finite list of all the elements so enumerated."
> *. . . Bauer:* [**Ba16**]*, p.8.*

Bauer argues that:

> "To see intuitively where the trouble lies, suppose p starts enumerating zeroes:
>
> $0, 0, 0, 0, 0, 0, \ldots$
>
> The output list should contain 0, but should it contain 1? However long a prefix of the enumeration we investigate, if it is all zeroes, then we cannot be sure whether 1 will appear later. For an actual proof we use the same trick as before: with q in hand we could construct the Halting oracle. Given any Turing machine T and input n, consider the program p which works as follows:
>
> - $p(k) = 1$ if $T(n)$ halts in fewer than k steps,
> - $p(k) = 0$ otherwise.
>
> The subset $S \subseteq \{0, 1\}$ enumerated by p is constructed so that
>
> - $1 \in S$ if $T(n)$ halts,
> - $1 \notin S$ if $T(n)$ does not halt.
>
> Now scan the finite list computed by $q(p)$: if it contains 1, then $T(n)$ holds, otherwise it does not."
> *. . . Bauer:* [**Ba16**]*, p.8.*

13.3.2. Sheaves. In Bauer's second example of a constructive model, the truth-assignments appeal to the properties of sheaves, where he notes that:

> "Truth varies as well, so that a statement may be true on one open set and false on another. Restrictions and the gluing property of sheaves transfer to truth:
>
> (1) if a statement is true on an open set $U \subseteq X$, then it is also true on a smaller open set $V \subseteq U$;
>
> (2) if a statement is true on each member U_i of an open cover, then it is also true on the union $\bigcup_{i \in I} U_i$.

In the topos the truth values are the open subsets of X. The truth value of a statement is the largest open set on which it holds, and the logic is dictated by the topology of X:

- falsehood and truth are $\emptyset$ and X, the least and greatest open sets, respectively;
- conjunction $U \wedge V$ is $U \cap V$, the largest open set contained in U and V;
- disjunction $U \vee V$ is $U \cup V$, the least open set containing U and V;
- negation $\neg U$ is the topological exterior $\mathrm{ext}(U)$, the largest open set disjoint from U;
- implication $U \Rightarrow V$ is $\mathrm{ext}(U\ V\ )$, the largest open set whose intersection with U is contained in V.

Excluded middle amounts to saying that $U \cup \mathrm{ext}(U) = X$ for all open $U \subseteq X$, a condition equivalent to open and closed sets coinciding. Only a very special kind of space X satisfies this condition, for as soon as it is a T_0-space (points are uniquely determined by their neighborhoods), it has to be discrete."

... Bauer: [**Ba16**], *pp.9-10.*

13.4. Depression

Bauer characterises the fourth stage as the one where a classical mathematician might gloomily wonder whether not understanding constructivism is like not having a sense of humor!

Reason: Bauer wryly concedes that there are:

> " ... many toposes, each a model of constructive mathematics. They were invented by the great Alexander Grothendieck for the purposes of studying algebraic geometry, but have since proved generally useful in mathematics. The Dubuc topos contains the 17th-century nilpotent infinitesimals, but without the 17th-century confusion and paradoxes. Joyal's theory of combinatorial species is just a topos in disguise, and so are various kinds of graphs. Simplicial sets, the home of homotopy theorists, form a presheaf topos. The realizability toposes are computer scientists' Gardens of Eden in which everything is computable by design. Even such mundane topics as the syntax of programming languages get their own toposes.
>
> Does anyone care about all these models of constructive mathematics? Well, if excluded middle is the only price for achieving rigor in infinitesimal calculus, our friends physicists just might be willing to pay it. After all they still use Newton's infinitesimals, despite our having lectured them about ϵ's and δ's since the time of Cauchy and Weierstraß. And how often does a physicist start a calculation by saying 'suppose not'? The situation with computer scientists is worse, as some of them actually help spread constructive mathematics with slogans such as 'propositions are types'. The recently discovered homotopy-theoretic interpretation of Martin-Löf type theory, a most extreme form of constructivism, has made some homotopy theorists and category theorists into allies of constructive mathematics. They even profess a new foundation of mathematics in which logic and sets are just two levels of an infinite hierarchy of homotopy types."
>
> *... Bauer:* [**Ba16**], *p.11.*

He notes, further, that turning to set theorists for advice offers no panacea since:

> "The axioms of Zermelo and Fraenkel stand as firm as ever, they assure us, and are the de facto foundation of today's mathematics. We are told that even the builders of toposes and modelers of homotopy types ultimately rely

on set theory, and we need not renounce excluded middle to compute with infinitesimals. The relief however does not last long. Set theorists go on to explain that Grothendieck actually used set theory extended with universes, each of which is an entire model of classical mathematics. Ever since Cohen's work on the independence of Cantor's hypothesis, set theorists have been exploring not one, but many worlds of classical mathematics. Would you like to have infinitesimals, or make all sets of reals measurable, or do you fancy $2^{\aleph_0} = \aleph_{42}$ A world of classical mathematics is readily built to order for you."

... *Bauer:* [**Ba16**], *pp.11-12.*

Bauer ruefully confesses that:

> "We initially set out to understand the difference between the classical and the constructive world of mathematics, only to have discovered that there are not two but many worlds, some of which simply cannot be discounted as logicians' contrivances. Excluded middle as the dividing line between the worlds is immaterial in comparison with having Cantor's paradise shattered into an unbearable plurality of mathematical universes."
>
> ... *Bauer:* [**Ba16**], *p.12.*

13.5. Acceptance

Nevertheless, Bauer characterises the concluding fifth stage as the one where a working mathematician eventually discovers that:

> "Some aspects of constructive mathematics are just logical hygiene: avoid indirect proofs in favor of explicit constructions, detect and eliminate needless uses of the axiom of choice, know the difference between a proof of negation and a proof by contradiction. Of course, constructivism goes deeper than that. The stringent working conditions of constructive worlds require an economy of thought which is disheartening at first but eventually pays off with vistas of new mathematical landscapes that are proscribed by orthodox mathematics."
>
> ... *Bauer:* [**Ba16**], *p.12.*

CHAPTER 14

The significant feature of Bauer's perspective

The most significant feature that emerges from Bauer's perspective of constructive mathematics (BPCM) is that:

(a) Whereas the goal of classical mathematics—post Peano, Dedekind and Hilbert—has been to uniquely characterise each informally defined mathematical structure (e.g., the Peano Postulates and its associated classical predicate logic) by a corresponding formal first-order language and a set of finitary axioms/axiom schemas and rules of inference (e.g., the first-order Peano Arithmetic PA and its associated first-order logic FOL) that assign unique provability values to each well-formed proposition of the language,

(b) The goal of constructive mathematics—post Brouwer and Tarski—has been to assign unique *evidence-based* truth values to each well-formed proposition of the language under a constructively well-defined interpretation (in the sense of Definitions 21.6, 21.5 and 21.7) over the domain of the structure.

From a functional perspective, (a) can be viewed in engineering terms as analogous to formalising the specifications of a proposed structure from a prototype. A more precise definition in terms of 'explication' is due to Rudolf Carnap:

> "By the procedure of *explication* we mean the transformation of an inexact, prescientific concept, the *explicandum*, into a new exact concept, the *explicatum*. Although the explicandum cannot be given in exact terms, it should be made as clear as possible by informal explanations and examples. ... A concept must fulfill the following requirements in order to be an adequate explicatum for a given explicandum: (1) similarity to the explicandum, (2) exactness, (3) fruitfulness, (4) simplicity."
> ... *Carnap:* [**Ca62a**]*, p.3 & p.5.*

Similarly, (b) can be viewed in engineering terms as analogous to confirming that the formal specifications (*explicatum*) of a proposed structure do succeed in uniquely identifying the prototype (*explicandum*).

In other words—as is implicit in Bishop's remarks quoted above (in §13.2)—the goals of the two activities ought to be viewed as necessarily complementing (see also Appendix 44), rather than being independent of or competing with, each other as to which is more foundational.

This investigation seeks to justify this view by identifying, and removing, the root of the misunderstanding that seems to inhibit recognition of the complementary roles of classical and constructive mathematics; a misunderstanding which, we argue, reflects unsustainable beliefs whose illusory, 'self-evidentiary', appeal could reasonably be viewed as owing more—perhaps as Bauer insightfully suggests—to psychological factors than to mathematical ones.

For instance, we illustrate in §22 the unsettling consequences of such 'self-evidentiary' appeal in our analysis of Goodstein's *curious* argumentation; where we show that, if we treat the subsystem ACA_0 of second-order arithmetic as a conservative extension of PA that is *equiconsistent* with PA, then we are led to the bizarre conclusion (Theorem 9.10) that, since PA is consistent:

> Goodstein's sequence $G_o(m_o)$ over the finite ordinals in ACA_0 *terminates* with respect to the ordinal inequality '$>_o$' even if Goodstein's sequence $G(m)$ over the natural numbers in ACA_0 *does not terminate* with respect to the natural number inequality '$>$' in any putative model of ACA_0!

14.1. Denial of an unrestricted applicability of the law of *excluded middle* is a *belief*

What is refreshing about Bauer's perspective of constructive mathematics (BPCM) is the—albeit tacit—acknowledgment that constructive mathematics holds denial or acceptance of the law of excluded middle (LEM) as an optional belief that is open to persuasion:

> "Unless we already believe in $\neg\neg P \Leftrightarrow P$, we cannot get one from the other by exchanging P and $\neg P$."
> *... Bauer:* [**Ba16**], *p.2.*

> "Classical mathematical training plants excluded middle so deeply into young students' minds that most mathematicians cannot even detect its presence in a proof. In order to gain some sort of understanding of the constructivist position, we should therefore provide a method for suspending belief in excluded middle."
> *... Bauer:* [**Ba16**], *p.6.*

We argue in this investigation that this is actually a misunderstanding embedded deeply not in classical mathematical training, but in constructive mathematics such as BPCM.

As we show, it is constructive mathematics that mistakenly equates denial of the ε-axioms in Hilbert's ε-calculus ([**Hi27**]) with denial of the law of the excluded middle in constructively well-defined (in the sense of Definitions 21.6, 21.5 and 21.7) interpretations of formal theories whose logical axioms and rules of inference are those of the standard first-order logic FOL which—as defined in introductory logical texts (e.g., [**Me64**])—forms an essential part of classical mathematical training.

The root of this misunderstanding lies in the fact that Brouwer's original objection (in [**Br08**]) was to the definition of existential quantification in terms such as those of Hilbert's ε-operator in the latter's ε-calculus, in which LEM is a theorem (see §3.1).

Denying LEM is thus *sufficient* for Brouwer's purpose of denying validity to any interpretation of Hilbert's definition of existential quantification over any putative structure in which the calculus is satisfied.

However it is not *necessary* since, by showing finitarily that the first-order Peano Arithmetic is consistent (Theorem 9.10)—whence FOL too is *finitarily* consistent—we show that the converse does not hold.

In other words, we show that denying validity to any interpretation of Hilbert's definition of existential quantification over a structure in which the calculus FOL is satisfied does not entail that LEM is not satisfied over the structure.

Moreover, as observed by Gödel in [**Go33**], such a denial of tertium non datur compelled Arend Heyting to admit an intuitionistic notion of "absurdity" into his formalisation of intuitionistic arithmetic, which entailed that "all of the classical axioms become provable propositions for intuitionism as well":

> "If one lets correspond to the basic notions of Heyting's propositional calculus the classical notions given by the same symbols and to "absurdity" ($\neg$), ordinary negation ($\sim$), then the intuitionistic propositional calculus $\boldsymbol{A}$ appears as a proper subsystem of the usual propositional calculus $\boldsymbol{H}$. But, using a different correspondence (translation) of the concepts, the reverse occurs: the classical propositional calculus is a sub-system of the intuitionistic one.. For, one has: Every formula constructed in terms of conjunction ($\wedge$) and negation ($\neg$) alone which is valid in $\boldsymbol{A}$ is also provable in $\boldsymbol{H}$. For each such formula must be of the form: $\neg A_1 \wedge \neg A_2 \wedge \ldots \wedge \neg A_n$, and if it is valid in $\boldsymbol{A}$, so must be each individual $\neg A_i$; but then by Gilvenko $\neg A_i$ is also provable in $\boldsymbol{H}$ and hence also the conjunction of the $\neg A_i$. From this, it follows that: if one translates the classical notions $\sim p,\ p \rightarrow q,\ p \vee q,\ p.q$ by the following intuitionistic notions: $\neg p,\ \neg(p \wedge \neg q),\ \neg(\neg p \wedge \neg q),\ p \wedge q$ then each classically valid formula is also valid in $\boldsymbol{H}$.
>
> The aim of the present investigation is to prove that something analogous holds for all of arithmetic and number theory, as given e.g. by the axioms of Herbrand. Here also one can give an interpretation of the classical notions in terms of intuitionistic notions, so that all of the classical axioms become provable propositions for intuitionism as well.
>
> [. . .]
>
> Theorem I, whose proof has now been completed, shows that intuitionistic arithmetic and number theory are only apparently narrower than the classical versions, and in fact contain them (using a somewhat deviant interpretation). The reason for this lies in the fact that the intuitionistic prohibition against negating universal propositions to form purely existential propositions is made ineffective by permitting the predicate of absurdity to be applied to universal propositions, which leads formally to exactly the same propositions as are asserted in classical mathematics. Intuitionism would seem to result in genuine restrictions only for analysis and set theory, and these restrictions are the result, not of the denial of tertium non datur, but rather of the prohibition of impredicative concepts. The above considerations, of course, yield a consistency proof for classical arithmetic and number theory. However, this proof is certainly not "finitary" in the sense given by Herbrand, following Hilbert."
>
> *. . . Gödel:* [**Go33**]*, pp.75 & 80.*

Thus, from an *evidence-based* perspective, on one hand Gödel's demonstration of an equivalence between classical arithmetic and Heyting's Arithmetic emphasises the thesis of this investigation that denial of LEM (tertium non datur) is unnecessary for ensuring finitism; especially since such denial apparently denies formal finitary argumentation to Intuitionism for much of that which it sought to protect.

On the other hand, current expositions of classical mathematics too *can* be held culpable insofar that whilst *dispensing* with Hilbert's explicit—hence accountable—formal *definition* of existential quantification in terms of his ε-operator—and therefore dispensing with the ε-epsilon calculus itself—it informally *introduces* the Hilbertian ε-operator interpretation of existential quantification as an implicitly self-evident—hence unaccountable—*postulation* which, generally introduced insidiously in the earliest pages of any introductory text on classical logic, does indeed embed itself so deeply—and unobtrusively—'into young students' minds that most mathematicians cannot even detect its presence in a proof'[1]!

This is the *postulation* of Aristotle's particularisation (Definition 3.1), which is essentially the assertion that the formula $[\exists x]$ of a formal theory may be unrestrictedly assumed—under any well-defined interpretation of the theory over a putative structure—as implying some *unspecified* instantiation of the existentially quantified predicate in the domain of the structure.

14.2. The significance of Aristotle's particularisation for constructivity

We recall that Aristotle's particularisation is the postulation that, from an informal assertion such as:

> 'It is not the case that, for any specified x, $P(x)$ does not hold',

usually denoted symbolically by '$\neg(\forall x)\neg P(x)$', we may always validly infer in the classical logic of predicates (compare with [**HA28**], pp.58-59) that:

> 'There exists an *unspecified* x such that $P(x)$ holds',

usually denoted symbolically by '$(\exists x)P(x)$'.

We shall show (in §15.1) that Aristotle's particularisation implies the first-order logic FOL is ω-consistent; whence we may always interpret the formal expression '$[(\exists x)F(x)]$' of a formal language under an interpretation as:

> 'There exists an object s in the domain of the interpretation such that $F^*(s)$.

We note that Aristotle's particularisation is a *non-finitary*, but fundamental, tenet of classical logic that—as noted in §14.1—is yet unrestrictedly adopted as *intuitively obvious* by standard literature.

We also recall (§4.2) that, as Brouwer had noted in his seminal 1908 paper on the unreliability of logical principles ([**Br08**]), the commonly accepted interpretation of this formula is ambiguous if such interpretation is intended over an infinite domain.

Brouwer essentially argued that:

(i) Even supposing the formula '$[P(x)]$' of a formal Arithmetical language interprets as an arithmetical relation denoted by '$P^*(x)$'; and
(ii) the formula '$[\neg(\forall x)\neg P(x)]$' interprets as the arithmetical proposition denoted by '$\neg(\forall x)\neg P^*(x)$';

---

[1]See for instance: Hilbert: [**Hi25**], p.382; Hilbert/Ackermann [**HA28**], p.48; Skolem: [**Sk28**], p.515; Gödel: [**Go31**], p.32; Carnap: [**Ca37**], p.20; Kleene: [**Kl52**], p.169; Rosser: [**Ro53**], p.90; Bernays/Fraenkel: [**BF58**], p.46; Beth: [**Be59**], pp.178 & 218; Suppes: [**Su60**], p.3; Luschei: [**Lus62**], p.114; Wang: [**Wa63**], p.314-315; Quine: [**Qu63**], pp.12-13; Kneebone: [**Kn63**], p.60; Cohen: [**Co66**], p.4; Mendelson: [**Me64**], p.52(ii); Novikov: [**Nv64**], p.92; Lightstone: [**Li64**], p.33; Shoenfield: [**Sh67**], p.13; Davis: [**Da82**], p.xxv; Rogers: [**Rg87**], p.xvii; Epstein/Carnielli: [**EC89**], p.174; Murthy: [**Mu91**]; Smullyan: [**Sm92**], p.18, Ex.3; Awodey/Reck: [**AR02b**], p.94, Appendix, Rule 5(i); Boolos/Burgess/Jeffrey: [**BBJ03**], p.102; Crossley: [**Cr05**], p.6.

(iii) the formula '$[(\exists x)P(x)]$'—which is formally defined as '$[\neg(\forall x)\neg P^*(x)]$'—need not interpret as the arithmetical proposition denoted by the usual abbreviation '$(\exists x)P^*(x)$'; and

(iv) such postulation is invalid as a general logical principle in the absence of a means for constructing some putative object a for which the proposition $P^*(a)$ holds in the domain of the interpretation.

The significance of Brouwer's objection for formal first-order theories of the kind that interested Hilbert (i.e., those whose logic was defined by §4.1) is that, in the event that there is no way of constructing some putative object a for which the proposition $P^*(a)$ is claimed to hold in the domain of the interpretation of a first-order theory S, then the S-term which would putatively correspond to a under the interpretation may also not be recursively definable from the primitive terms of the theory—thus contradicting the first-order constraint on S.

Moreover we shall show that such a postulation would imply that S is ω-consistent (see §15.7) or, equivalently, that Rosser's Rule C is valid in S (see §15.6); an implication that not only—as Gödel and Rosser have shown—has far-reaching consequences for any formal system that admits such postulation but—significantly and hitherto unsuspectedly—does not hold for the first-order Peano Arithmetic PA (see Corollary 11.6 and Theorem 11.10).

In this investigation we therefore adopt the convention that the assumption that '$(\exists x)P^*(x)$' is the intended interpretation of the formula '$[(\exists x)P(x)]$'—which is essentially the assumption that Aristotle's particularisation holds over the domain of the interpretation—must always be explicit.

CHAPTER 15

Hilbert's Programme

15.1. The significance of Gödel's ω-consistency for constructive mathematics

We note that, in order to avoid intuitionistic objections to his reasoning in his seminal 1931 paper on formally undecidable arithmetical propositions, Gödel did not assume that the *weak* standard interpretation $\boldsymbol{M}$ of PA (as defined in §A, Appendix A, and analysed in Chapter 7) is constructively well-defined (in the sense of Definitions 21.6, 21.5 and 21.7).

Instead, Gödel introduced the syntactic property of ω-consistency as an explicit assumption in his formal reasoning ([**Go31**], p.23 and p.28).

> ω-**consistency**: A formal system S is ω-consistent if, and only if, there is no S-formula $[F(x)]$ for which, first, $[\neg(\forall x)F(x)]$ is S-provable and, second, $[F(a)]$ is S-provable for any specified S-term $[a]$.

Gödel explained that his reasons for introducing ω-consistency as an explicit assumption in his formal reasoning was to avoid appealing to the semantic concept of classical arithmetical truth—a concept which we shall show (Corollary 15.11) is implicitly based on an intuitionistically objectionable logic that assumes Aristotle's particularisation holds over $\mathbb{N}$.

> "The method of proof which has just been explained can obviously be applied to every formal system which, first, possesses sufficient means of expression when interpreted according to its meaning to define the concepts (especially the concept "provable formula") occurring in the above argument; and, secondly, in which every provable formula is true. In the precise execution of the above proof, which now follows, we shall have the task (among others) of replacing the second of the assumptions just mentioned by a purely formal and much weaker assumption."
> *. . . Gödel:* [**Go31**], *p.9.*

We now show (Corollary 15.9) that Gödel's assumption is 'weaker' in the sense that:

- If Tarski's inductive definitions of the satisfaction and truth of existentially quantified PA formulas under the standard interpretation $\boldsymbol{M}$ (as defined in §A, Appendix A) *assume*[1] that Aristotle's particularisation is valid over $\mathbb{N}$,
- Then PA is consistent if, and only if, it is ω-consistent.

---

[1] *Assume* in the sense that: "A sequence s satisfies $(Ex_i)A$ if and only if there is a sequence s′ which differs from s in at most the i$^{\mathtt{th}}$ place such that s′ satisfies A." *. . . Mendelson:* [**Me64**], *p.52, V(ii).*

15.2. The significance of Hilbert's ω-Rule for constructive mathematics

To place Gödel's assumption of ω-consistency within the perspective of this investigation, we consider an:

> ***Algorithmic*** ω***-Rule***: *If it is proved that the PA formula* $[F(x)]$ *interprets as an arithmetical relation* $F^*(x)$ *that is algorithmically computable as true for any given natural number* n*, then the PA formula* $[(\forall x)F(x)]$ *can be admitted as an initial formula (axiom) in PA.*

The significance of the Algorithmic ω-Rule is that, as part of his program for giving mathematical reasoning a finitary foundation, Hilbert proposed an ω-Rule ([**Hi30**], pp.485-494) as a means of extending a Peano Arithmetic to a possible completion (i.e. to logically showing that, given any arithmetical proposition, either the proposition, or its negation, is formally provable from the axioms and rules of inference of the extended Arithmetic).

> ***Hilbert's*** ω***-Rule***: *If it is proved that the PA formula* $[F(x)]$ *interprets as an arithmetical relation* $F^*(x)$ *that is algorithmically verifiable as true for any given natural number* n*, then the PA formula* $[(\forall x)F(x)]$ *can be admitted as an initial formula (axiom) in PA.*

The question of whether or not Hilbert's ω-Rule can be considered as finitary is addressed in detail by Schirn and Niebergall:

> "Restricted versions of the ω-rule have been suggested both as a means of explicating certain forms of finitary arguments or proofs and as a way of correctly extending a theory already accepted. In this section, we want to deal with the question as to whether weak versions of the ω-rule can be regarded as finitary. For if they can, they may prove useful for the construction of metamathematical theories that clash neither with Hilbert's programme nor with Gödel's Incompleteness Theorems. In pursuing our aim, we align ourselves with Hilbert's programme. By contrast, in his 1931 essay Hilbert himself introduces a restricted ω-rule as a means of extending PA, though he does so in a way which admits different interpretations.
>
> > *Rule* ω^*: When it is shown that the formula $A(\mathbf{Z})$ is a correct numerical formula for each particular numeral $\mathbf{Z}$, then the formula $\forall x A(x)$ can be taken as a premise.
>
> Hilbert qualifies this rule expressly as finitary and goes on to remind us that $\forall x A(x)$ has a much wider scope than $A(\tilde{\mathfrak{n}})$, where $\tilde{\mathfrak{n}}$ is an arbitrary given numeral."
>
> *. . . Schirn and Niebergall:* [**SN01**]*, p.137.*

Schirn and Niebergall conclude—echoing the thesis of this investigation—that Hilbert's assumption of Aristotle's particularisation as a valid, and essential, form of reasoning—as evidenced in his definitions of the universal and existential quantifiers in terms of his ε-operator (see §4.1)—committed him to an essentially non-finitary perspective, reflected also in his ω-rule, both of which we show (in §15.7 and §15.5 respectively) are stronger than Gödel's assumption of ω-consistency in his 1931 paper [**Go31**] on 'formally undecidable' arithmetical propositions:

> "We venture to surmise that Hilbert qua metalogician relies on existence assumptions of precisely this kind without being haunted by any finitist qualms. And we do think that those assumptions of infinity that are made

> by accepting one application of rule ω^* are not more far-reaching than those made by accepting transfinite induction upto ε_0.
>
> It should be evident that the ω-rule or even one application of it cannot be accepted from Hilbert's original finitist point of view. Yet both modern metalogic and Hilbert's metamathematics of the 1920s rest on certain assumptions of infinity that clash anyway with his classical finitism (cf. Niebergall and Schirn 1998, section 4). Intuitively speaking, one may tend to believe that the metalogical assumptions of infinity just appealed to, or Hilbert's assumption in his work on proof theory in the 1920's that there are infinitely many stroke-symbols, are slightly weaker than those that we make when we apply an ω-rule. However this may be, we do not rule out that Hilbert wants to commit himself only to the possible existence of infinitely many stroke-figures or, alternatively, to the existence of infinitely many possible stroke-figures. Unless a satisfactory theory of the potential infinite is to hand, it is probably wise to postpone closer scrutiny of the question whether, from the point of view of strength, applications of a given ω-rule and the assumptions of infinity, both made by Hilbert in the 1920s and common in contemporary metalogic, differ essentially from each other."
>
> *... Schirn and Niebergall:* [**SN01**]*, p.141.*

Now, Gödel's 1931 paper can, not unreasonably, be viewed as the outcome of a presumed attempt to formally validate Hilbert's ω-rule finitarily, since:

LEMMA 15.1. *If we meta-assume Hilbert's ω-rule for PA, then a consistent PA is ω-consistent.*

PROOF. If the PA formula $[F(x)]$ interprets as an arithmetical relation $F^*(x)$ that is algorithmically *verifiable* as true for any given natural number n, and the PA formula $[(\forall x)F(x)]$ can be admitted as an initial formula (*axiom*) in PA, then $[\neg(\forall x)F(x)]$ cannot be PA-provable if PA is consistent. The lemma follows. $\square$

We note, however, that we cannot similarly conclude from the the Algorithmic ω-Rule that a consistent PA is ω-consistent.

Moreover, by Gödel's Theorem VI in [**Go31**], it follows from Lemma 15.1 that one consequence of assuming Hilbert's ω-Rule is that there must, then, be an undecidable arithmetical proposition; a further consequence of which would be that any first-order arithmetic such as PA must be essentially incomplete.

15.3. Is Hilbert's ω-Rule equivalent to Gentzen's Infinite Induction?

Schirn and Niebergall also address the question of whether Hilbert's ω-rule is weaker than Gentzen's cut-elimination, and consider the argument that:

> "Since we can construe the infinitely many premises of one application and, hence, of finitely many applications of the ω-rule as ordered with order type ω, the proof theorist who intends to employ the ω-rule has to presuppose only (the existence of) ω. By contrast, Gentzen's consistency proof for pure number theory in his 1936 article presupposes (the existence of) ε_0. Moreover, if a proof theorist endorsing the basic tenets of Hilbert's finitism were asked how he brings it about to prove infinitely many premises, he might respond as follows:
>
>> To accept one application of rule ω^* is not more problematic than to make the assumption that one can conclude from the PA-provability of '$\forall x(0 \leq x)$' to the PA-provability of '$0 \leq n$' for every n. Both cases require that *modus ponens* be applied

infinitely many times, where the sequence of the prooflines has order-type ω."

... Schirn and Niebergall: [**SN01**], *p.140.*

Schirn and Niebergall stress that, as highlighted in §4.3 of this investigation, the issue confronting Hilbert then—as also finitists of all hues since—was that of unambiguously defining a deterministic procedure for interpreting quantification finitarily both over the numerals, and the numbers that they seek to formally represent:

"It is important to bear in mind that finitist mathematics may be extended by adding well-formed formulae or by adjoining further 'principles'. It is the first that is at issue in Hilbert's proposed finitist interpretation of quantified statements about numerals (Hilbert and Bernays 1934, 32ff.). So, let us begin by taking a closer look at this.

(1) A general statement about numerals '$\forall \tilde{\mathfrak{n}}\ \tilde{\mathfrak{A}}(\tilde{\mathfrak{n}})$' can be interpreted finitistically only as a *hypothetical* statement, i.e. as a statement about every *given* numeral. A general statement about numerals expresses a law that has to be verified for each individual case.[25]
(2) An existential statement about numerals '$\exists \tilde{\mathfrak{n}}\ \tilde{\mathfrak{A}}(\tilde{\mathfrak{n}})$' must be construed, from the finitist point of view, as a 'partial proposition', i.e. 'as an incomplete communication of a more exactly determinate statement, which consists either in the direct specification of a numeral with the property $\tilde{\mathfrak{A}}$ or in the specification of a procedure for gaining such a numeral' (Hilbert and Bernays 1934, 32). The specification of the procedure requires that for the sequence of acts to be carried out a determinate limit be presented.
(3) In like manner we have to interpret finistically statements in which a general statement is combined with an existential statement such as 'For every numeral $\tilde{\mathfrak{r}}$ with the property $\tilde{\mathfrak{A}}(\tilde{\mathfrak{r}})$ there exists a numeral $\tilde{\mathfrak{l}}$ for which $\tilde{\mathfrak{B}}(\tilde{\mathfrak{r}}, \tilde{\mathfrak{l}})$ holds', for example. In the spirit of the finitist attitude, this statement must be regarded as the incomplete communication of a procedure with the help of which we can find for each given numeral $\tilde{\mathfrak{r}}$ with the property $\tilde{\mathfrak{A}}(\tilde{\mathfrak{r}})$ a numeral $\tilde{\mathfrak{l}}$ which stands to $\tilde{\mathfrak{r}}$ in the relation $\tilde{\mathfrak{B}}(\tilde{\mathfrak{r}}, \tilde{\mathfrak{l}})$.
(4) Hilbert points out that negation is unproblematic when applied to what he calls 'elementary propositions', i.e. to statements which can be decided by direct intuitive observation. In the case of universally and existentially quantified statements about numerals, however, it is not immediately clear what ought to be regarded as their negation in a finitist sense. The assertion that a numeral $\tilde{\mathfrak{n}}$ with the property $\tilde{\mathfrak{A}}(\tilde{\mathfrak{n}})$ does not exist has to be conceived of as the assertion that it is *impossible* that a numeral $\tilde{\mathfrak{n}}$ has the property $\tilde{\mathfrak{A}}(\tilde{\mathfrak{n}})$. Strengthened negation of an existential statement, thus constructed, is not (as in the case of negation of an elementary statement) the contradictory of '$\exists \tilde{\mathfrak{n}} \tilde{\mathfrak{A}}(\tilde{\mathfrak{n}})$'. From the finitist standpoint, we therefore cannot make use of the alternative according to which there either exists a numeral $\tilde{\mathfrak{n}}$ to which $\tilde{\mathfrak{A}}(\tilde{\mathfrak{n}})$ applies or the application of $\tilde{\mathfrak{A}}(\tilde{\mathfrak{n}})$ to a numeral $\tilde{\mathfrak{n}}$ is excluded.Hilbert admits that, from the finitist perspective, the law of the excluded middle is invalid in so far as for quantified sentences we do not succeed in finding a negation of finitist content which satisfies the law.

Fn. 25 The proposed interpretation of universal quantification is reminiscent of Gentzen's and W. W. Tait's account (See Tait 1981) in that it likewise embodies a version of the ω-rule which rests on the identification of numerals with numbers. Tait's additional idea is that the law in question is to be construed as something given by a finitist function."

... Schirn and Niebergall: [**SN01**], *p.143.*

Schirn and Niebergall note that, although Hilbert endeavoured to distinguish between quantified propositions over numerals and quantified propositions over the numbers that they seek to represent (corresponding to what we have termed as *weak* and *strong* interpretations of quantification in §4.3), he could not express the distinction formally; possibly because—as illustrated by Definitions 5.2 and 5.2—a transparent and unambiguous description of the deterministic infinite procedures needed to *evidence* the distinction formally, i.e. Hilbert's 'reduction procedure' (quoted in §15.4) became available only *after* the realisation that Turing's 1936 paper [**Tu36**]) admits *evidence-based* reasoning—in the sense that one can view the *values* of a simple functional language as specifying *evidence* for propositions in a constructive logic ([**Mu91**], §1 Introduction):

> "Now, when we compare (1)-(4) with Hilbert's remarks on what can be formulated finitistically in say, 'Über das Unendliche' (1926), we notice two things. Explication (4) is very much akin to the points made in that paper about the negation of quantified statements. The matter stands differently with (1)-(3). On plausible grounds, one should assume that a finitistically interpreted sentence is capable of being formulated finitistically in the first place. If that is correct, then (1) to (3) ought to be understood in such a way that universally quantified sentences, even sentences whose formalizations are genuine Π^0_2-sentences (cf. (3)), can be formulated in the language of finitist mathematics. Plainly, if around 1934 Hilbert really wished to maintain that quantified sentences of types (1)-(3) have a proper place in the language of finitist metamathematics, he would have departed significantly from his conception of metamthematics in the 1920s. It is quite true that both in 'Über das Unendliche' and in *Grundlagen der Mathematik* (1934) Hilbert spares himself the trouble of developing the language of finitist metamathematics in a systematic way. There is one crucial difference, though. In his celebrated essay, the distinction between real and ideal statements, although chiefly designed to streamline the formalism, provides at least a clue for assessing the scope and the limits of the language of finitist mathematics. By contrast, the reader of Hilbert and Bernays 1934 who is expecting to encounter this helpful distinction again here will be disappointed. In this book, there is not even a trace of it framed in familiar terms.
>
> Admittedly, all this does not exclude that an alternative way of construing the phrase 'finitistically interpretable' can be contrived. Consider sentences of type (1). In 'Über das Endliche' '$\forall x(x + 1 = 1 + x)$' is not a sentence of L_M, and the same applies to an expression like (*) 'For every given $\tilde{\mathfrak{a}}$ '$\tilde{\mathfrak{a}} + 1 = 1 + \tilde{\mathfrak{a}}$' is true'. By contrast, if a numeral $\tilde{\mathfrak{a}}$ is given, the expression '$\tilde{\mathfrak{a}} + 1 = 1 + \tilde{\mathfrak{a}}$' is a sentence of the language of finitist metamathematics. In *Grundlagen der Mathematik* (1934), the question of which language (*) may belong to is passed over in silence. We are only told that a finitist interpretation of (*) requires that it be construed as a hypothetical judgement about every given numeral (cf. (1)) (we aassume that (*) should be considered a general statement about numerals). A similar formulation is employed in 'Über das Endliche' (91 [378]), with the minor difference that here Hilbert talks about interpretation *simpliciter*.[28] And it is almost precisely at this point that he introduces his conception of real and ideal statements, stressing that the latter are, from the finitist point of view, devoid of meaning. This shows: the fact that in 'Über das Endliche' certain sentences of type (1), like (*), are amenable to (a finitist) interpretation is compatible with the fact that the language of finitist metamathematics does not comprise sentences of this type. The finitist interpretation of (*) proceeds in such a way that for every given numeral $\tilde{\mathfrak{a}}$ (*) is replaced with '$\tilde{\mathfrak{a}} + 1 = 1 + \tilde{\mathfrak{a}}$', and then each of the sentences '$\tilde{\mathfrak{a}} + 1 = 1 + \tilde{\mathfrak{a}}$' is interpreted finitistically. Seen from this angle, we should not take it for granted that in

Grundlagen der Mathematik (1934) finitist interpretability implies finitist formulability. What we do take for granted is that if this implication holds for sentences of one of these types, then it must also hold for the sentences of the remaining types.

Fn. 28 It is reasonable to assume that here he likewise has a finitist interpretation in mind. Notice that non-finitary sentences, i.e. ideal sentences, are not interpreted at all."

. . . Schirn and Niebergall: [**SN01**]*, p.143.*

15.4. Hilbert's *weak* proof of consistency for PA

Schirn and Niebergall note further that, in order to argue that every numerical formula derivable from the axioms of a weakened arithmetic H was 'true', Hilbert and Bernays introduced the concept of 'verifiabilty', whose well-definedness, however, appealed to the existence of appropriate 'reduction procedures' in cases where quantification and/or its negation was interpreted over only all 'numeral' instantiations of the formulas of H:

> "In order to find out whether in *Grundlagen der Mathematik* (1934) quantified sentences of types (1)-(4) are indeed regarded to belong to the well-formed sentences of the language of finitist metamathematics, it is useful to take a closer look both at the number-theoretic formalisms presented there and at the corresponding consistency proofs. In §6 (Hilbert and Bernays 1934, 220ff.), Hilbert carries out a consistency proof for a certain weak arithmetical axiom system (cf. 1934, 219) which we call H. The 'proof' is entirely informal, and it is not clear whether Hilbert shows metamathematically 'There is no proof in H for *falsum*' or only for every concretely given proof figure a that a is no proof for *falsum* in H. The very beginning of the proof speaks in favour of the second option, that is, we conjecture that Hilbert conducts what is in effect an informal version of what in our paper 'Hilbert's finitism and the notion of infinity' (1998) we call an *approximative* consistency proof:[29] 'We now imagine that we are given such a proof figure with the end formula $0 \neq 0$. On this (proof figure) two processes can be effected one after another which we call *dissolution* of the proof figure in "*proof-threads*" and *elimination of the free variables*' (Hilbert and Bernays 1934, 220; cf. 298).
>
> Hilbert and Bernays show, in the first place, that every numerical formula that can be derived from the axioms of H without the use of bound variables is true.[30] In a second step, they demonstrate that every numerical formula provable in H is true even if we drop the restriction concerning the bound variables. They generalize the notion of a true formula in such a way that all formulae of a given proof figure are taken into account, not only the numerical ones (cf. Hilbert and Bernays 1934, 232ff.). This is accomplished by introducing the term 'verifiable'. Confining themselves provisionally to formulae without universal quantifiers, Hilbert and Bernays explain the term as follows: (i) a numerical formula is verifiable, if it is true; (ii) a formula containing one or more free individual variables, but no other variables, is verifiable, if it can be shown that it is true for every replacement of the variables with numerals; and (iii) a formula with bound variables, but without formula variables and without universal quantifiers is verifiable, if the application of a certain reduction procedure leads to a verifiable formula in the sense of (i) or (ii).[31] In a further step, Hilbert and Bernays show that the end formula of the given proof (in H) is verifiable (cf. Hilbert and Bernays 1934, 244ff.). H is therefore consistent.
>
> As to (ii), it is plain that verifiability is defined through an unbounded quantification over numerals, i.e. for all substitution instances. The phrase 'can be shown' remains unexplained and is possibly meant to impart a 'constructive' or finitist air to unbounded universal quantification over numerals.

These belong, in the terminology of Hilbert (1926), to the class of ideal statements and are as such unacceptable for the finitist of the 1920s. We further note that carrying out consistency proofs along the lines of (i)-(iii) requires that the verifiability predicate can be formulated in the language of finitist metamathematics. Hence, this language must contain sentences of type (1)."

Fn. 29 In Niebergall and Schirn 1998, §6 we define this notion as follows (for axiomatizable theories S and T with representation τ): S proves the approximative consistency of T:$\Leftrightarrow \forall n\ S \vdash \neg Proof_{\tau}(n, \perp)$. We assume here that the formalized proof predicate is the standard one. In our opinion, the notion of an approximative consistency proof captures the core of the conception of finitary metamathematical consistency proofs which Hilbert developed in his papers on proof theory in the 1920s.

Fn. 30 Numerical formulae are characterized as quantifier-free sentences; see Hilbert and Bernays 1934, 228. Hilbert emphasizes that this is only a stricter version of the assertion that it is impossible to derive $0 \neq 0$ from the axioms of H without admitting bound variables (Hilbert and Bernays 1934, 230)."

...Schirn and Niebergall: [**SN01**]*, pp.144-145.*

Now, if we treat Hilbert and Bernays' intent whilst introducing their concept of 'verifiability' (as detailed above) as corresponding to the concept of 'algorithmic verifiability' introduced in Chapter 5 (Definition 5.2) then—despite Schirn and Niebergall's reservations in [**SN01**]—it can be argued that Hilbert's reasoning does yield a *weak* consistency proof for PA which is essentially that of Theorem 7.7 (in contrast to the *strong* finitary proof of consistency for PA in Theorem 9.10).

Moreover, from such a perspective Hilbert and Bernays' reasoning would at least be as constructive as Gentzen's proof ([**Me64**], p.258) of consistency for a first-order number theory—such as the formal system S of Peano Arithmetic defined by Mendelson (in [**Me64**], pp.102-103)—if we admit Gentzen's Rule of Infinite Induction ([**Me64**], p.259) in a formal system S_∞ in which all theorems of S are provable ([**Me64**], p.263, Lemma A-3):

$$\text{Infinite Induction: } \frac{\mathcal{A}(\overline{n}) \vee \mathcal{D} \quad \texttt{for all natural numbers n}}{((x)\mathcal{A}(x)) \vee \mathcal{D}}$$

Further, if we were to interpret Infinite Induction as essentially stating that:

Proposition 15.2. If the S_∞-formula $[\mathcal{A}(\overline{n})]$ interprets as true for any given natural number n, then we may conclude that $[(\forall x)\mathcal{A}(x)]$ is provable in S_∞.

then it would follow that:

Thesis 15.3. Hilbert's ω-Rule is equivalent to Gentzen's Infinite Induction. □

15.5. Hilbert's ω-Rule is stronger than ω-consistency

Now we note that, in his 1931 paper [**Go31**], Gödel constructed an arithmetical formula $[R(x)]$ in his formal arithmetic P and showed that, if P is assumed ω-consistent, then both $[(\forall x)R(x)]$ and $[\neg(\forall x)R(x)]$ are unprovable in P ([**Go31**], p.25(1), p.26(2)), even though $[R(n)]$ interprets as true for any given numeral $[n]$.

It immediately follows that:

Lemma 15.4. *Hilbert's ω-Rule is stronger than ω-consistency.* □

Lemma 15.4 justifies why Gödel's argument in [**Go31**]—from which he concludes the existence of an undecidable arithmetical proposition—is based on the weaker (i.e., weaker than assuming Hilbert's ω-rule) premise that a consistent PA can be ω-consistent.

The question arises whether an even weaker Algorithmic ω-Rule—as defined above (which, prima facie, does not imply that a consistent PA is necessarily ω-consistent)—can yield a finitary completion for PA as sought by Hilbert, albeit for an ω-*in*consistent PA.

It is a question that we answer in the affirmative, since we show that PA is not only 'algorithmically' complete in the sense of the Algorithmic ω-Rule (§10.1), but categorical with respect to algorithmic computability (Corollary 11.1).

15.6. Rosser's Rule C is equivalent to Gödel's ω-consistency

Clearly such categoricity conflicts with the conventional wisdom that J. Barkley Rosser's proof of undecidability ([**Ro36**]) successfully avoids the assumption of ω-consistency.

However, we note that a formal system P is ω-consistent if, and only if:

(i) Either, we cannot have that a P-formula $[(\exists x)F(x)]$ is P-provable and also that $[\neg F(a)]$ is P-provable for any given, constructively well-defined, term $[a]$ of P;

(ii) Or, from the P-provability of $[(\exists x)F(x)]$ we can always conclude the existence of an *unspecified* P-term $[a]$ such that $[F(a)]$ is provable, without establishing that $[a]$ is a constructively well-defined P-term.

We note that by admitting introduction of an *unspecified* P-term into the formal reasoning, (ii) implicitly assumes—without proof (see §16.5), and without formally admitting an axiom of choice into P equivalent to Hilbert's ε-based choice axiom (see §4.1)—that such an $[a]$ can, indeed, be recursively constructed—at least in principle—from the primitive terms of P by the first-order construction of terms permitted within P from its primitive terms (since a closed PA term can denote only algorithmically computable constants by Theorem 11.10).

We further note that (i) is Gödel's definition of ω-consistency, which he explicitly assumed when deriving his 'formally undecidable' arithmetical formula (which involves a universal quantifier).

We also note that (ii) is Rosser's Rule C (see §B, Appendix B; also [**Me64**], Sec §7, pp.73-75), which he tacitly assumes as a valid deduction rule of FOL when deriving his 'formally undecidable' arithmetical formula (which involves an existential quantifier) in [**Ro36**], where he explicitly assumes only that P is simply consistent.

However, Rosser's belief that simple consistency suffices for establishing his 'formally undecidable' arithmetical formula (which involves an existential quantifier) in P is illusory (see §16) since, if P is simply consistent, the introduction of an *unspecified* P-term into the formal reasoning under Rule C entails Aristotle's particularisation in any interpretation of P, which in turn entails that P is ω-consistent (Corollary 15.8).

Although the *implicit* assumption of ω-consistency—entailed by Rosser's Rule C—is not immediately obvious in Rosser's original proof, it is implicit (see §16.5) in steps (i)-(iii) on p.146 of Mendelson's proof of Proposition 3.32 (Gödel-Rosser Theorem) in [**Me64**].

15.7. Aristotle's particularisation is stronger than ω-consistency

In this investigation we argue that these issues are related, and that placing them in an appropriate perspective requires any constructive perspective of mathematics to question not only the persisting belief in classical mathematics that Aristotle's particularisation remains valid even when applied over an infinite domain such as $\mathbb{N}$, but also the basis of Brouwer's denial of the Law of the Excluded Middle in constructive mathematics, following his challenge of the belief in [**Br08**].

For instance, we note that:

LEMMA 15.5. *If PA is consistent but not ω-consistent, then there is some PA formula $[F(x)]$ such that, under any interpretation—say $\mathcal{I}_{PA(N)}$—of PA over $\mathbb{N}$:*

(i) *the PA formula $[\neg(\forall x)F(x)]$ interprets as an algorithmically verifiable true arithmetical proposition under $\mathcal{I}_{PA(N)}$;*

(ii) *for any given numeral $[n]$, the PA formula $[F(n)]$ interprets as an algorithmically verifiable true arithmetical proposition under $\mathcal{I}_{PA(N)}$.*

PROOF. The lemma follows from the definition of ω-consistency and from Tarski's standard definitions of the satisfaction, and truth, of the formulas of a formal system such as PA under an interpretation. □

Further:

LEMMA 15.6. *If PA is consistent and the interpretation $\mathcal{I}_{PA(N)}$ admits Aristotle's particularisation over $\mathbb{N}$[2]:*

(i) *and the PA formula $[\neg(\forall x)F(x)]$ interprets as an algorithmically verifiable true arithmetical proposition under $\mathcal{I}_{PA(N)}$,*

(ii) *then there is some unspecified natural number m such that the interpreted arithmetical proposition $F^*(m)$ is algorithmically verifiable as false in $\mathbb{N}$.*

PROOF. The lemma follows from the definition of Aristotle's particularisation and Tarski's standard definitions of the satisfaction, and truth, of the formulas of a formal system such as PA under an interpretation. □

It follows immediately from Lemma 15.6 that:

COROLLARY 15.7. *If PA is consistent and Aristotle's particularisation holds over $\mathbb{N}$, there can be no PA formula $[F(x)]$ such that, under any interpretation $\mathcal{I}_{PA(N)}$ of PA over $\mathbb{N}$:*

(i) *the PA formula $[\neg(\forall x)F(x)]$ interprets as an algorithmically verifiable true arithmetical proposition under $\mathcal{I}_{PA(N)}$;*

(ii) *for any given numeral $[n]$, the PA formula $[F(n)]$ interprets as an algorithmically verifiable true arithmetical proposition under $\mathcal{I}_{PA(N)}$.* □

[2] i.e., any interpretation that defines the existential quantifier as in [**Me64**], pp.51-52 V(ii).

In other words[3]:

Corollary 15.8. *If PA is consistent and Aristotle's particularisation holds over $\mathbb{N}$, then PA is ω-consistent.* □

It follows that:

Corollary 15.9. *If Aristotle's particularisation holds over $\mathbb{N}$, then PA is consistent if, and only if, it is ω-consistent.*

Proof. We note first that, by Corollary 15.8, if PA is consistent and Aristotle's particularisation holds over $\mathbb{N}$, then PA is ω-consistent.

We note next that if PA is ω-consistent then, since $[n = n]$ is PA-provable for any given PA numeral $[n]$, we cannot have that $[\neg(\forall x)(x = x)]$ is PA-provable. Since an inconsistent PA proves $[\neg(\forall x)(x = x)]$, an ω-consistent PA cannot be inconsistent. □

It also follows that (cf. Corollary 9.12):

Corollary 15.10. *If PA is consistent but not ω-consistent, then Aristotle's particularisation does not hold in any interpretation of PA over $\mathbb{N}$.* □

It further follows immediately by Theorem 8.5 that:

Corollary 15.11. *Aristotle's particularisation does not hold in any model of PA.* □

15.8. Markov's principle does not hold in PA

We note that an immediate consequence of Corollary 15.11 is that Markov's principle does not, as has been argued by some advocates of intuitionistic logic, hold in PA:

> "Mathematicians of the Russian school accept the following principle: if [n] is a recursive binary sequence (i.e., for each i, $\mathrm{n_i} = 0$ or $\mathrm{n_i} = 1$), and if we know that not for all i does $\mathrm{n_i} = 0$, then we may say that there is an i such that $\mathrm{n_i} = 1$. Formally, in terms of a binary number-theoretic function, f:
>
> $\neg\forall \mathrm{x}(\mathrm{f(x)} = 0) \rightarrow \exists \mathrm{n}(\mathrm{f(n)} = 1)$.
>
> Advocates of intuitionistic logic often find this unpalatable. Existential statements should be harder to prove. But in fact this is the principle that allows one to prove in constructive recursive analysis that every real valued function is continuous at each point in which it is defined. This was first proved by Tseĭtin. Markov himself had proved weaker versions, which are classically but not constructively equivalent."
>
> *... Posy:* [**Pos13**]*, p.112.*

Corollary 15.12. *Markov's principle: $\neg(\forall x)(f(x) = 0) \rightarrow (\exists n)(f(n) = 1)$ does not hold in PA.*

Proof. For example, we have by Lemma 11.3 that Gödel's formula $[R(n)]$ is PA-provable for any given numeral $[n]$, whilst by Corollary 11.4 the PA formula $[\neg(\forall x)R(x)]$ is also PA-provable. □

[3] We note that Corollary 15.8 negates Martin Davis' speculation in [**Da82**], p.129, that such a proof of ω-consistency may be "... open to the objection of *circularity*".

15.9. Hilbert's purported 'sellout' of finitism

We digress here slightly to emphasise a philosophical observation of topical significance that:

(a) the making of a formal distinction (as in Theorem 7.7) between what may be considered 'constructive (*weak*)', vis à vis what may be considered 'finitary (*strong*)', reasoning has, unfortunately, seemed of diminishing concern, and interest, in academia; and that

(b) this can, not unreasonably, be attributed to an unreasonably persisting influence of Hilbert's thinking, after 1929, on current perspectives towards foundational issues.

Our observation is supported, in particular, by what Schirn and Niebergall—in their analysis of Hilbert's finitism ([**SN01**])—term as 'The sellout of finitism' by Hilbert and Bernays, where they note that:

> "In §5.2 of Hilbert and Bernays (1939), entitled 'The formalized metamathematics of the number-theoretic formalism' (cf. 302ff.), the authors introduce a notational variant of PA which they call Z_μ. Its purported drawback for metamathematical purposes rests on the fact 'that in the formalization of finitist reasoning in the system (Z_μ) the characteristic of the finitist argumentation is, for the most part, lost' (1939, 361). Nonetheless, Z_μ is regarded as setting a provisional upper limit for a finitistically acceptable metatheory (Hilbert and Bernays 1939, 353ff., 361ff.).
>
> At the beginning of the section 'Eliminability of the "tertium non datur" for the investigation of the consistency of the system (Z_μ)', Hilbert and Bernays observe that the 'proof-theoretic methods hitherto applied (by them) , even though they partially go beyond the domain of recursive number theory, apparently do not transcend the domain of those concept formations and modes of inference that can still be presented within the formalism Z_μ' (Hilbert and Bernays 1939, 361).[50] On the face of it, this passage suggests that Hilbert and Bernays are here operating with a twofold notion of extending proof-theory or metamathematics: the extension involves both the language of metamathematics and the metamathematical theory itself. Unfortunately, they do not distinguish clearly between these two methods of extending metamathematics; their respective remarks give rise to ambiguity.
>
> Hilbert and Bernays sketch, in the first place, an extension L^+_{PRA} of L_{PRA} which is supposed to contain only 'finitary' statements. Taking L_{PRA} as the starting point, L^+_{PRA} is arrived at in two stages: first, symbols for certain computable number-theoretic functions are adjoined to L_{PRA} (call the set of formulae thereby defined L'_{PRA}). Second, L'_{PRA} is converted into L^+_{PRA} by way of adding to L'_{PRA} only those statements that can be 'interpreted in a strict sense' by a statement of L'_{PRA} (cf. Hilbert and Bernays 1939, 362). Hilbert and Bernays do not explain the phrase 'interpreted in a strict sense', but their ensuing exposition suggests that it is at least formulae of the type '$\forall x \exists y\ \psi(x, y)$' with quantifier-free formula ψ that aare capable of being 'interpreted in a strict sense' in L'_{PRA}. The interpretation can be given by choosing for such a '$\forall x \exists y\ \psi(x, y)$' the quantifier-free formula '$\psi(x, \mathrm{f}(x))$' in L'_{PRA}, where f is a function-sign for a recursive function which has already been introduced in L'_{PRA}. That these two formulae are equivalent to one another in some sense of 'equivalent' is suggested by the phrase 'strict interpretation', but the authors do not argue for this 'equivalence'.[51]

Fn. 50 The authors also argue that the proof-theoretical methods have been extended from PRA to PA without infringing the 'methodic fundamental idea of finitist proof theory' (1939, 362).

Fn. 51 Obviously, the conception of the finitistically admissible presented in this example is akin to the position Hilbert and Bernays advocate in 1934, but deviates from Hilbert's finitism in the 1920s. The truly original, austere notion of a finitary statement embodies less than what can be expressed in L^{+}_{PRA}."

... *Schirn and Niebergall:* [**SN01**], *p.154.*

15.10. Gödel's Zilsel lecture

What is noteworthy from an *evidence-based* perspective about the above account is that the search for finitary means of reasoning in the first volume of *Grundlagen der Mathematik* (1934)—which even then conflicted with Hilbert's enthusiastic espousal of Cantor's set theory, thereby leading to what came to be known as 'Hilbert's Program'—was apparently abandoned around the period of the second volume of *Grundlagen der Mathematik* (1939); justified in part, perhaps, by developments following Gödel's 1931 incompleteness theorems which seemed to suggest—as Gödel reportedly remarked in his 1938 Zilsel lecture—that "intuitionistic methods went beyond finitist ones" (as Gödel had analysed formally in [**Go33**]).

In a detailed account of these developments, and their impact on Hilbert's Program, Wilfried Sieg refers to a lecture Gödel delivered in Vienna on 29 January 1938:

> "... to a seminar organized by Edgar Zilsel. The lecture presents an overview of possibilities for continuing Hilbert's program in a revised form. It is an altogether remarkable document: biographically, it provides, together with (1933b) and (1941), significant information on the development of Gödel's foundational views; substantively, it presents a hierarchy of constructive theories that are suitable for giving (relative) consistency proofs of parts of classical mathematics (see §§24 of the present note); and, mathematically, it analyzes Gentzen's (1936) proof of the consistency of classical arithmetic in a most striking way (see §7). A surprising general conclusion from the three documents just mentioned is that Gödel in those years was intellectually much closer to the ideas and goals pursued in the Hilbert school than has been generally assumed (or than can be inferred from his own published accounts). ...
>
> The Zilsel lecture gives, as we remarked, an overview of possibilities for a revised Hilbert program. The central element of that program was to prove the consistency of formalized mathematical theories by finitist means. Gödel's 1931 incompleteness theorems have been taken to imply that for theories as strong as first-order arithmetic this is impossible, and indeed, so far as Gödel ventures to interpret Hilbert's finitism, that is Gödel's view in the present text as well as earlier in (1933b) (though not in (1931d)) and later in (1941), (1958) and (1972). The crucial questions then are what extensions of finitist methods will yield consistency proofs, and what epistemological value such proofs will have.
>
> Two developments after (Gödel 1931d) are especially relevant to these questions. The first was the consistency proof for classical first-order arithmetic relative to intuitionistic arithmetic obtained by Gödel (1933d). The proof made clear that intuitionistic methods went beyond finitist ones (cf. footnote 10 below). Some of the issues involved had been discussed in Gödel's lecture (1933b), but also in print, for example in (Bernays 1935b) and (Gentzen 1936). Most important is Bernays's emphasis on the "abstract element" in intuitionistic considerations. The second development was Gentzen's consistency proof for first-order arithmetic using as the additional

principle—justified from an intuitionistic standpoint—transfinite induction up to ε_0. Already in (1933b, p. 31) Gödel had speculated about a revised version of Hilbert's program using constructive means that extend the limited finitist ones without being as wide and problematic as the intuitionistic ones:

> "But there remains the hope that in future one may find other and more satisfactory methods of construction beyond the limits of the system A [[capturing finitist methods]], which may enable us to found classical arithmetic and analysis upon them. This question promises to be a fruitful field for further investigations."

The Cambridge lecture does not suggest any intermediate methods of construction; by contrast, Gödel presents in the Zilsel lecture two "more satisfactory methods" that provide bases to which not only classical arithmetic but also parts of analysis might be reducible: quantifier-free theories for higher-type functionals and transfinite induction along constructive ordinals. Before looking at these possibilities, we sketch the pertinent features of the Cambridge talk, because they give a very clear view not only of the philosophical and mathematical issues Gödel addresses, but also of the continuity of his development."

... Sieg: [**Si12**]*, Chapter II.4, pp.193-195.*

The above account raises the following point of interest from the *evidence-based* perspective of [**An16**].

For any integer $n \geq 0$, and integers $x_i \geq 0$, we denote the ordinal $W < \omega^{\omega}$ by $(x_0, x_1, x_2, x_3, x_4, \ldots, x_n)$, where:

$$W = \omega^n.x_n + \ldots + \omega^4.x_4 + \omega^3.x_3 + \omega^2.x_2 + \omega.x_1 + x_0$$

Define:

$$S_k = \{(x_0, x_1, x_2, x_3, x_4, \ldots, x_n)\} \ni (x_0+x_1+x_2+x_3+x_4+\ldots+x_n) = k$$

Then S_k is a finite set of n-tuples for any $k \geq 0$. Hence $\{S_k\}$ is denumerable.

Now we note that $\omega^i \in S_1$ for all $n \geq i \geq 1$, and it is reasonable to assume that some finite initial segment of any denumerable ordering of the ordinals below ω^{ω}, which does not appeal (non-constructively) to an axiom of choice, must include an ordinal $\omega^i.x_j$ for some $x_j > 0$ corresponding to each $n \geq i \geq 1$.

Query 15.13. Can the above argument be extended to ordinals below ϵ_0 by defining higher order ordinals similarly in terms of the ordered n-tuples $(W, W_1, W_2, \ldots, W_n)$, where $W_i = \omega_i^n.x_{i,n} + \ldots + \omega_i^4.x_{i,4} + \omega_i^3.x_{i,3} + \omega_i^2.x_{i,2} + \omega_i.x_{i,1}$, and so on recursively?

Since transfinite induction can reasonably be considered constructive only if the induction is definable in terms of an *evidence-based* procedure over a denumerable ordering of the ordinals, it is difficult to see in what sense Gentzen's proof—unlike the *weak* proof of consistency in Theorem 7.7—can be considered constructive.

Sieg notes that the issue of constructivity was addressed by Gödel earlier in his 1933 'Cambridge' lecture as follows:

> "Understanding by mathematics "the totality of the methods of proof actually used by mathematicians", Gödel sees the problem of providing a foundation for these methods as falling into two distinct parts (p. 1):
>
>> At first these methods of proof have to be reduced to a minimum number of axioms and primitive rules of inference, which have to be stated as precisely as possible, and then secondly a justification in some sense or other has to be sought for these axioms, i.e., a theoretical foundation of the fact that they lead to results agreeing with each other and with empirical facts.

> The first part of the problem is solved satisfactorily through type theory and axiomatic set theory, but with respect to the second part Gödel considers the situation to be extremely unsatisfactory. "Our formalism", he contends, "works perfectly well and is perfectly unobjectionable as long as we consider it as a mere game with symbols, but as soon as we come to attach a meaning to our symbols serious difficulties arise" (p. 15). Two aspects of classical mathematical theories (the non-constructive notion of existence and impredicative definitions) are seen as problematic because of a necessary Platonist presupposition "which cannot satisfy any critical mind and which does not even produce the conviction that they are consistent" (p. 19). This analysis conforms with that given in the Hilbert school, for example in (Hilbert and Bernays 1934), (Bernays 1935b) and (Gentzen 1936). Gödel expresses the belief, again as the members of the Hilbert school did, that the inconsistency of the axioms is most unlikely and that it might be possible "to prove their freedom from contradiction by unobjectionable methods"."
> *... Sieg:* [**Si12**]*, Chapter II.4, pp.195-196.*

We note that Gödel is implicitly underscoring a thesis of this investigation that:

(α) Whereas the goal of classical mathematics, post Peano, Dedekind and Hilbert, has been:

- to uniquely characterise each informally defined mathematical structure (e.g., the Peano Postulates and its associated classical predicate logic)
- by a corresponding formal first-order language, and a set of finitary axioms/axiom schemas and rules of inference (e.g., the first-order Peano Arithmetic PA and its associated first-order logic FOL)
- which assign unique provability values to each well-formed proposition of the language;

(β) The goal of constructive mathematics, post Brouwer and Tarski, has been:

- to assign unique, *evidence-based*, truth values to each well-formed proposition of the language
- under a constructively well-defined interpretation over the domain of the structure (when viewed as a 'conceptual metaphor' in the terminology of [**LR00**]).

(γ) The goals of the two activities ought to, thus, be viewed as necessarily complementing, rather than being independent of or in conflict with, each other as to which is more 'foundational'.

Further, the *strong* (intuitionistically unobjectionable) finitary proof of consistency for PA in Theorem 9.10 justifies the optimism Gödel shared in 1933 with Hilbert and Bernays over a positive outcome for Hilbert's Program.

Theorem 9.10, moreover, underscores an implicit thesis of this investigation that:

> The deterministic infinite procedures (corresponding to Hilbert's 'reduction procedure' quoted in §15.4) needed to *formalise* the distinction between 'constructive' and 'finitary' reasoning (as illustrated for quantification in §4.1; and generally by Definitions 5.2 and 5.3) involve a paradigm shift in recognising that:

– Turing's 1936 paper [**Tu36**]) admits *evidence-based* reasoning for assigning the values of 'satisfaction' and 'truth' to the formulas of a first-order language such as PA,

– in the sense that one can view the *values* of a simple functional language as specifying *evidence* for propositions in a constructive logic ([**Mu91**], §1 Introduction),

– which yields two constructively well-defined, hitherto unsuspected, *complementary* interpretations of PA (as defined in Chapter 7 and Chapter 9)

– under Tarski's inductive definitions of the satisfiability and truth of the PA-formulas under an interpretation.

We note further that, according to Sieg, Gödel's focus in 1933 was already on identifying the minimum requirements that *any* method claiming to prove consistency of a system must satisfy in order to be considered constructive:

> "Clearly, the methods whose justification is being sought cannot be used in consistency proofs, and one is led to the consideration of parts of mathematics that are free of such methods. Intuitionistic mathematics is a candidate, but Gödel emphasizes (p. 22) that
>
> > "the domain of this intuitionistic mathematics is by no means so uniquely determined as it may seem at first sight. For it is certainly true that there are different notions of constructivity and, accordingly, different layers of intuitionistic or constructive mathematics. As we ascend in the series of these layers, we are drawing nearer to ordinary non-constructive mathematics, and at the same time the methods of proof and construction which we admit are becoming less satisfactory and less convincing."
>
> The strictest constructivity requirements are expressed by Gödel (pp. 2325) in a system A that is based "exclusively on the method of complete induction in its definitions as well as in its proofs". That implies that the system A satisfies three general characteristics: (A1) Universal quantification is restricted to "infinite totalities for which we can give a finite procedure for generating all their elements"; (A2) Existential statements (and negations of universal ones) are used only as abbreviations, indicating that a particular (counter-)example has been found without—for brevity's sake—explicitly indicating it; (A3) Only decidable notions and calculable functions can be introduced. As the method of complete induction possesses for Gödel a particularly high degree of evidence, "it would be the most desirable thing if the freedom from contradiction of ordinary non-constructive mathematics could be proved by methods allowable in this system A" (p. 25)."
>
> *... Sieg:* [**Si12**]*, Chapter II.4, p.196.*

If we apply Gödel's stipulations (A1), (A2) and (A3) to the *weak* standard interpretation of PA defined in Chapter 7), and the *strong* finitary interpretation of PA defined in Chapter 9, we note that:

(1) The *weak* interpretation of universal quantification under the *weak* standard interpretation $\boldsymbol{M}$ of PA (see §4.4), as well as the *strong* interpretation of universal quantification under the *strong* finitary interpretation $\boldsymbol{B}$ of PA (see §4.5), are both defined constructively in terms of finitely determinate algorithms over the respective domains of quantification;

(2) Existential quantification in each case is used only as an abbreviation for the negation of universal quantification such that:

(a) The formula $[(\exists x)F(x)]$ is an abbreviation of $[\neg(\forall x)\neg F(x)]$, and is *defined* as *verifiably* true in $\boldsymbol{M}$ relative to its truth assignment T_M if, and only if, it is not the case that, for any *specified* natural number n, we may conclude on the basis of *evidence-based* reasoning that the proposition $\neg F^*(n)$ holds in $\boldsymbol{M}$; where the proposition $F^*(n)$ is *postulated* as holding in $\boldsymbol{M}$ for some *unspecified* natural number n if, and only if, it is not the case that, for any *specified* natural number n, we may conclude on the basis of *evidence-based* reasoning that the proposition $\neg F^*(n)$ holds in $\boldsymbol{M}$;

   (i) However, we note that we cannot (see §6.1) assume that the satisfaction and truth of quantified formulas of PA are always finitarily decidable—in the sense of being algorithmically *computable*—under the *weak* standard interpretation $\boldsymbol{M}$ of PA over $\mathbb{N}$ (as defined in §A, Appendix A), since we cannot prove finitarily from *only* Tarski's definitions and the assignment T_M of algorithmically *verifiable* truth values to the atomic formulas of PA under $\boldsymbol{M}$ whether, or not, a given quantified PA formula $[(\forall x_i)R]$ is algorithmically *verifiable* as true under $\boldsymbol{M}$;

   (ii) Moreover, it is not unreasonable to conclude—in the light of Gödel's stipulation (A2) in the previous quote—that the failure to successfully carry out Hilbert's Program may be attributed to an unawareness of the *evidence-based* distinction between algorithmically *computable* truth and algorithmically *verifiable* truth (see Chapter 5).

(b) The formula $[(\exists x)F(x)]$ is an abbreviation of $[\neg(\forall x)\neg F(x)]$, and is *defined* as true in $\boldsymbol{B}$ relative to its truth assignment T_B if, and only if, we may conclude on the basis of *evidence-based* reasoning that it is not the case, for any *specified* natural number n, that the proposition $\neg F^*(n)$ holds in $\boldsymbol{B}$.

   We note that $\boldsymbol{B}$ is a *strong* finitary interpretation of PA since—when interpreted suitably—all theorems of first-order PA interpret as *finitarily* true in $\boldsymbol{B}$ relative to T_B (see §9.1, Theorem 9.7).

(c) Only decidable notions are used to establish that the PA axiom schema of induction interprets as *verifiably* true under the *weak* standard interpretation $\boldsymbol{M}$ of PA (Lemma 7.3); and as *computably* true under the *strong* finitary interpretation $\boldsymbol{B}$ of PA (Lemma 9.4).

To an extent, the above explains in hindsight why, according to Sieg, Gödel's focus shifted from seeking the consistency sought originally by Hilbert's Program to assessing the relative consistency of various systems and proofs:

> "Gödel infers that Hilbert's original program is unattainable from two claims: first, all attempts for finitist consistency proofs actually undertaken in the Hilbert school operate within system A; second, all possible finitist arguments can be carried out in analysis and even classical arithmetic. The latter claim implies jointly with the second incompleteness theorem that finitist consistency proofs cannot be given for arithmetic, let alone analysis. Gödel puts this conclusion here quite strongly: ". . . . unfortunately the hope of succeeding along these lines [[using only the methods of system A]] has

vanished entirely in view of some recently discovered facts" (p. 25). But he points to interesting partial results and states the most far-reaching one, due to (Herbrand 1931) in a beautiful and informative way (p. 26):

> If we take a theory which is constructive in the sense that each existence assertion made in the axioms is covered by a construction, and if we add to this theory the non-constructive notion of existence and all the logical rules concerning it, e.g., the law of excluded middle, we shall never get into any contradiction.

Gödel conjectures that Herbrand's method might be generalized to treat Russell's "ramified type theory", i.e., we assume, the theory obtained from system A by adding ramified type theory instead of classical first-order logic.[9]

There are, however, more extended constructive methods than those formalized in system A; this follows from the observation that system A is too weak to prove the consistency of classical arithmetic together with the fact that the consistency of classical arithmetic can be established relative to intuitionistic arithmetic.[10] The relative consistency proof is made possible by the intuitionistic notion of absurdity, for which "exactly the same propositions hold as do for negation in ordinary mathematics—at least, this is true within the domain of arithmetic" (p. 29). This foundation for classical arithmetic is, however, "of doubtful value": the principles for absurdity and similar notions (as formulated by Heyting) employ operations over all possible proofs, and the totality of all intuitionistic proofs cannot be generated by a finite procedure; thus, these principles violate the constructivity requirement (A1).

Despite his critical attitude towards Hilbert and Brouwer, Godel dismisses neither in (1933b) when trying to make sense out of Hilberts program in a more general setting, namely, as a challenge to find consistency proofs for systems of "transfinite mathematics" relative to "constructive" theories. And he expresses his belief that epistemologically significant reductions may be obtained.

[Fn. 9] In Konzept, p. 0.1, Godel mentions Herbrands results again and also the conjecture con- cerning ramified type theory. The obstacle for an extension of Herbrands proof is the principle of induction for transfinite statements, i.e., formulae containing quantifiers. Interestingly, as discovered in (Parsons 1970), and independently by Mints (1971) and Takeuti (1975, p. 175), the induction axiom schema for purely existential statements leads to a conservative extension of A, or rather its arithmetic version, primitive recursive arithmetic. How Herbrands central considerations can be extended (by techniques developed in the tradition of Gentzen) to obtain this result is shown in (Sieg 1991).

[Fn. 10] In his introductory note to (1933d), Troelstra (1986, p. 284) mentions relevant work also of Kolmogorov, Gentzen and Bernays. Indeed, as reported in (Gentzen 1936, p. 532), Gentzen and Bernays discovered essentially the same relative consistency proof independently of Godel. According to Bernays (1967, p. 502), the above considerations made the Hilbert school distinguish intuitionistic from finitist methods. Hilbert and Bernays (1934, p. 43) make the distinction without referring to the result discussed here."

... Sieg: [**Si12**]*, Chapter II.4, pp.196-197.*

We also note that—according to Carl J. Posy's implicitly empathetic account of Hilbert's Program—prior to publication of the second volume of the *Grundlagen der Mathematik* in 1929, Hilbert was yet 'confident in our ability to produce provably adequate formal systems':

"**Hilbert's Program: Constructivism of the Right**

It might seem strange to call Hilbert a constructivist. After all, he himself introduced non-constructive methods into algebra, he was unfriendly towards the Kroneckerian restrictions, and—in opposition to Brouwer—he was a staunch supporter of classical logic. Indeed, Hilbert did not practice or condone "constructive mathematics" in the sense that I have been using the term. Nevertheless, he was a constructivist: he saw infinity as a problem for mathematics (or, more precisely, as the source of mathematics' problems), and as a solution he aimed to found mathematics on a base of intuition, just as do all the constructivists we have considered.

Hilbert in fact was driven by an opposing pair of pulls, and his program for the foundation of mathematics was the result of those pulls.

On the one hand, Hilbert held that there is no infinity in physical reality, and none in mathematical reality either. Only intuitable objects truly exist, and only an intuitively grounded process (he spoke of "finitary thought") can keep us within the realm of the intuitable. This is his constructivism. Mathematical paradox arises, he said, when we exceed those bounds. And indeed, he held that infinite mathematical objects do go beyond the bounds of mathematical intuition. For him finite arithmetic gave the basic objects, and he held that arithmetic reasoning together was the paradigm of finitary thought. Together this comprised the "real" part of mathematics. All the rest—set theory, analysis, and the like—he called the "ideal" part, which had no independent "real content".

On the other hand, Hilbert also believed that this ideal mathematics was sacrosant. No part of it was to be jettisoned or even truncated. This is why I dub it "constructivism of the right". "No one will expel us," he famously declared, "from the paradise into which Cantor has led us (Hilbert 1926).

Hilbert's program, which was first announced in 1904 and was further developed in the 1920s, was designed to reconcile these dual pulls.[35] outline of the program for a branch of mathematics whose consistency is in question is generally familiar: axiomatize that branch of mathematics; formalize the axiomatization in an appropriate formal language; show that the resulting formal system is adequate to the given branch of mathematics (i.e., sound and complete); and then prove the formal system to be consistent.

The important assumptions here are that formal systems are finitely graspable things and that the study of formal systems is a securely finitary study. Thus, he is proposing to use the finitary, trustworthy part of mathematics to establish the consistency of the ideal part.

Today, of course, we know that the program as thus formulated cannot succeed. Gödel's theorems tell us that. But in the late 1920s, Hilbert still had ample encouraging evidence. Russell and Whitehead's *Principia Mathematica* stood as a monument to formalization. He and his students successfully had axiomatized and formalized several branches of mathematics. Moreover, he firmly believed that within each branch of mathematics we can prove or refute any relevant statement. He believed that is, optimistically, in the solvability of all mathematical problems. And so he was confident in our ability to produce provably adequate formal systems. And—assuming in advance the success of his program—he was comfortable in developing the abstract, unanchored realms of ideal mathematics.

Fn. 35 It was announced in Hilbert's lecture "Über die Grundlagen der Logik und der Arithmetik" (published as Hilbert 1905). He developed the Program more fully in the 1920s. Hilbert and Bernays' book *Grundlagen der Mathematik* (1934) contains the most mature statement of the program."

. . . Posy: [**Pos13**], *pp.119-120.*

In other words, around 1929 Hilbert's focus, and that of mainstream classical meta-mathematics thereafter, apparently shifted from seeking finitary means of reasoning—in order to justify that a formal system (viewed in the sense of Carnap's *explicandum* as considered in §23.1) does indeed represent that which (corresponding to Carnap's *explicatum* as considered in §23.1) it seeks to express formally—to where it has resided ever since: determining the relative proof-theoretic strengths of formal systems, irrespective of whether or not they have any *evidence-based* interpretation that would assure the soundness—and hence the consistency—of the concerned systems.

Schirn and Niebergall deplore at length this weakening of Hilbert's finitary resolve, which they implicitly seem to also ascribe to efforts by Hilbert and Bernays to contain the perceived negative implications of Gödel's 1931 paper [**Go31**] on finitism, whilst at the same time unquestioningly accepting the validity of Gödel's (as we show in §11.4, unjustified) conclusions therein; even though such acceptance entailed accepting (illusory, as we show in Corollary 11.2) non-standard integers, such as Cantor's transfinite ordinals 'ω' and 'ε_0' as legitimate objects in 'constructive' reasoning.

> "We observe that in Hilbert and Bernays 1939 the authors pass easily from the determination of what is finitistically formulable to a characterization of what is finitistically provable. We are told that for the formalization of certain general results of proof theory it is *desirable* to obtain as mathematical theorems conditionals containing a universally quantified sentence as antecedent (Hilbert and Bernays 1939, 358, 362). Such sentences are for example (formalizations of) assertions concerning the unprovability or verifiability of formulae or the computability of functions. To illustrate the idea, Hilbert and Bernays sketch a formalization of the informal consistency proof for H in *Grundlagen der Mathematik* (1934), to which we have already referred in §2. The formalization is carried out in PA, and it is shown by means of a complexity analysis that a fragment of PA, though extending PRA, would actually suffice for the consistency proof.. Proof-theoretic means extending PRA, including a form of complete induction which cannot be formalized by the induction schema of recursive number theory (Hilbert and Bernays 1939, 358), are said to be useful or desirable for conducting certain formal consistency proofs.
>
> However this may be, the crucial question for Hilbert and Bernays is whether the so-called finitary methods may go beyond the scope of the modes of inference formalizable in Z_μ. The question is said to lack a precise formulation, on the grounds that 'finitary' has not been introduced as a sharply defined termed, but only as a label for a 'methodic guideline'. It serves merely to recognize certain forms of concept formation and of inference definitely as finitary and certain others definitely as non-finitary. It is not appropriate, though, for drawing an exact dividing line between modes of inference which meet the requirements of the finitist method and modes of inference which do not.[52]
>
> It is in this connection that Hilbert and Bernays mention a typical borderline-case; it concerns the question whether conditionals with a universally quantified sentence as antecedent can be formulated finitistically. They claim to have removed this indeterminacy by distinguishing between sentences and inference rules (Hilbert and Bernays 1939, 358f., 361). Hilbert and Bernays admit, though, that in some cases this distinction may strike us as *forced*, and all this is said to require that the bounds of the finitist framework hitherto established be somewhat loosened, that is, that we go beyond what can be formulated in L^+_{PRA} and proved in recursive number theory.
>
> Two comments on these and similar remarks and ideas in Hilbert and Bernays (1939) are in order here. First, what the authors may make clear with them is at best that, compared with Hilbert's finitism of the 1920s, the *language* of finitist metamathematics must be extended; for instance, unbounded quantifications should now be finitistically formulable. Yet Hilbert and Bernays do not even address the issue why in that case all theorems of PA should be sound from a finitist point of view. Moreover, remarks to the

extent that it is useful or desirable that the language of metamathematics has a certain expressive power and that the metamathematical theory itself includes a certain repertoire of proof-theoretic means convey nothing about the assumed finitary character of both the metamathematical language and the metamathematical theory under consideration.

Second, Hilbert's and Bernay's remarks presented above suggest that the old *foundational* view dominating the pre-Gödelian period of Hilbertian proof theory has been replaced with a view like this: we are accustomed to certain informal metamathematical considerations, and experience teaches us that they can be formalized in PA. Hence, we are entitled to use them in metamathematical reasoning. Whether Hilbert and Bernays do not care any longer much about questions of finitist justifiability, or whether they leave their readers with a principle of the following kind: *what is not definitely infinitistic may be regarded as finitist*, remains unclear. Deplorably, this is not the only place where Hilbert and Bernayshedge instead of putting their cards on the table. Surely Hilbert, as the founder of the finitist point of view, should feel called upon to give a clear-cut explication of 'finitist' allowing a fair assessment of his programme. So, it could seem that the appeal to the alleged indefinability of 'finitist' is meant to serve as a safeguard against possible objections. This may come out a little clearer in Hilbert's and Bernays's treatment of transfinite induction to which we now turn.

Possibly guided by some principle of the kind just mentioned and the desire to be able to formalize metatheoretical considerations to as high a degree as possible, Hilbert and Bernays arrive at PA (or Z_μ, respectively) as a provisional boundary within which a finitist metatheory may be developed (1939, 354, 361). The crucial question for Hilbert and Bernays is now whether the so-called finitary methods may go beyond the scope of the modes of inference formalizable in Z_μ. (Remember that, owing to the vagueness of the word 'finitary', they do not consider this question to be formulated in precise terms.) For, as they point out (1939, 353f.), a (formal) metamathematical consistency proof for PA cannot be carried out in PA itself. Nevertheless, Hilbert and Bernays do not rest content with the idea that there can be no finitary proof for PA. Accordingly, they insist that 'in any case, it is possible [...] to surpass the modes of inference formalizable in (Z_μ) without using the typically non-finitary inferences. And in this way we succeed in giving a very simple consistency proof for the system (Z)' (1939, 362). Hilbert and Bernays refer in this connection to an arithmetical version of transfinite induction.[53] The line of thought which leads them eventually to considering transfinite induction, in particular up to ε_0, as a possibly 'legitimate' method of proof theory deserves close attention."

[...]

"At the very end of the last chapter of *Grundlagen der Mathematik* (1939), Hilbert and Bernays make a concluding (but convoluted) remark on Gentzen's (1936) consistency proof, which suggests that it was no longer their serious concern to argue for the finitist nature of the proof-theoretic means applied in consistency proofs for mathematical theories they consider important. We are told that it is a consequence of Gödel's Theorem that

> the more comprehensive the formalism to be considered is, the higher are the order types, i.e. forms of the generalized induction principle, that must be used. [...] The methodic requirements for the contentual proof of that higher induction principle supply the standard for [determining] which kind of methodic assumptions must be taken as a basis for the contentual attitude, if the consistency proof for the formalism in question is to be successful, (Hilbert and Bernays 1939, 387)

Fn. 52 We think that in Hilbert's classical papers the expression 'finitary' is much less vague than in *Grundlagen der Mathematik* (1939). In spite of its vagueness both

> during the pre-Gödelian and post-Gödelian period of Hilbertian proof theory, it is reasonable to say that it had undergone a thorough shift of meaning by 1939.
>
> Fn. 53 Therefore the remark just quoted seems to suggest that PA+TI[ε_0] could be treated as a finitistically admissible theory."
>
> ... *Schirn and Niebergall:* [**SN01**], *pp.154-157.*

However, since:

(i) Schirn and Niebergall observe that, regarding the consistency of PA, 'Hilbert and Bernays do not rest content with the idea that there can be no finitary proof for PA'; and

(ii) Hilbert's and Bernays' 'informal' proof of the consistency of arithmetic in the *Grundlagen der Mathematik*—as analysed in [**SN01**] (see §15.4)—can be viewed as essentially outlining a proof of Theorem 7.7;

a more appropriate perspective may be that Hilbert's weakened finitism in 1939 reflected, as we noted earlier, the circumstance that the deterministic infinite procedures (corresponding to Hilbert's 'reduction procedure' quoted in §15.4) needed to *formalise* the distinction between 'constructive' and 'finitary' reasoning (as illustrated for quantification in §4.1; and generally by Definitions 5.2 and 5.3) have become available only *after* the realisation that Turing's 1936 paper [**Tu36**]) admits *evidence-based* reasoning—in the sense that one can view the *values* of a simple functional language as specifying *evidence* for propositions in a constructive logic ([**Mu91**], §1 Introduction).

CHAPTER 16

Analysing Gödel's and Rosser's proofs of 'undecidability'

We note that, in his seminal 1931 paper, Gödel constructively defined a Peano Arithmetic P, and a P-formula $[R(x)]$ (in his argument, Gödel refers to this formula only by its 'Gödel' number 'r'; [**Go31**], p.25, Eqn.(12)), such that ([**Go31**], Theorem VI, p.24, p.25(1) & p.26(2)):

LEMMA 16.1. *If P is ω-consistent, then neither $[(\forall x)R(x)]$ nor $[\neg(\forall x)R(x)]$ are P-provable.* $\square$

Of course, since every ω-consistent system is necessarily simply consistent, Gödel's conclusion is significant only if there is an ω-consistent language that seeks to formally express all our true propositions about the natural numbers.

The issue, of whether there is an ω-consistent system of Arithmetic at all, appears to have been treated as inconsequential[1] following J. Barkley Rosser's 1936 paper ([**Ro36**]), in which he claimed that Gödel's reasoning can be 'extended' to arrive at Gödel's intended result (i.e., construction of a formally undecidable arithmetical proposition in P) by assuming only that P is simply consistent (i.e., without assuming that P is ω-consistent).

However, we now analyse various expositions of Rosser's argument (vis à vis Gödel's reasoning), and show that they either implicitly appeal to Aristotle's particularisation, or tacitly to the weaker assumption (see §15.7) that P is ω-consistent.

16.1. Rosser and formally undecidable arithmetical propositions

Although both Gödel's proof and Rosser's argument are complex, and not easy to unravel, the former has been extensively analysed, and its various steps formally validated[2], in a number of expositions of Gödel's number-theoretic reasoning (e.g., [**Me64**], p.143; [**EC89**], p.210-211).

[1]See, for instance, [**Be59**], p.595; [**Wa63**], p.19 (Theorem 3) & p.25; [**Me64**], p.144; [**Sh67**], p.132 (Incompleteness Theorem); [**EC89**], p.215; [**BBJ03**], p.224 (Gödel's first incompleteness theorem).

[2]Possibly because Gödel's remarkably self-contained 1931 paper—it neither contained, nor needed, any formal citations—remains unsurpassed in mathematical literature for thoroughness, clarity, transparency and soundness of exposition.

In sharp contrast, Rosser's widely cited argument does not appear to have received the same critical scrutiny, and its number-theoretic expositions generally remain either implicit or sketchy[3].

16.2. Wang's outline of Rosser's argument

Wang, for instance, states that ([**Wa63**], p.337) from the formal provability of:

(i) $\neg(x)(B(x,\overline{q}) \supset (Ey)(y \leq x \;\&\; B(y, n(\overline{q}))))$

in his formal system of first-order Peano Arithmetic Z, we may infer the formal provability of[4]:

(ii) $(Ex)(B(x,\overline{q}) \;\&\; \neg(Ey)(y \leq x \;\&\; B(y, n(\overline{q}))))$

However, the inference (ii) from (i) appears to assume that the following deduction is valid for some *unspecified* $\overline{j}$:

$\neg(x)(B(x,\overline{q}) \supset (Ey)(y \leq x \;\&\; B(y, n(\overline{q}))))$

$\bullet$ $(Ex)\neg(B(x,\overline{q}) \supset (Ey)(y \leq x \;\&\; B(y, n(\overline{q}))))$

$\star$ $\neg(B(\overline{j},\overline{q}) \supset (Ey)(y \leq \overline{j} \;\&\; B(y, n(\overline{q}))))$

$B(\overline{j},\overline{q}) \;\&\; \neg(Ey)(y \leq \overline{j} \;\&\; B(y, n(\overline{q})))$

$(Ex)(B(x,\overline{q}) \;\&\; \neg(Ey)(y \leq x \;\&\; B(y, n(\overline{q}))))$

Thus, Wang's conclusion appears to implicitly *assume* both Aristotle's particularisation ($\bullet$) and Rosser's Rule C ($\star$); entailing, ipso facto, that Z is ω-consistent (see §15.6).

16.3. Beth's outline of Rosser's argument

Similarly, in his outline of a formalisation of Rosser's argument, Beth implicitly concludes ([**Be59**], p.594 (ij)) that from the formal provability of:

vspace+1ex

(i) $\neg(q)[G_1(m^0, q, m^0) \rightarrow (s)\{B(s,q) \rightarrow (Et)[t \leq s \;\&\; (Er)\{H(q,r) \;\&\; B(t,r)\}]\}]$

in his formal system of first-order Peano Arithmetic P, we may infer the formal provability of[5]:

---

[3]See, for instance, [**Be59**], pp.593-595 (which focuses on Rosser's argument, and treats Gödel's proof of his Theorem VI ([**Go31**], p.24) as a, secondary, weaker result); [**Wa63**], p.337; [**Sh67**], p.232 (curiously, this introductory text contains *no* reference to Gödel or to his 1931 paper!); [**Rg87**], p.98; [**EC89**], p.215 and p.217, Ex.2; [**Sm92**], p.81; [**BBJ03**], p.226 (this introductory text, too, focuses on Rosser's argument, and treats Gödel's argument as more of a historical curiosity!).

[4]We note that although Wang does not explicitly define the interpretation of the formal Z-formula '$(Ex)F(x)$' as 'There is some x such that $F(x)$', this interpretation appears implicit in his discussion and definition of '$(Ev)A(v)$' in terms of Hilbert's ε-function ([**Wa63**], p.315(2.31); see also p.10 & pp.443-445) as a property of the underlying logic of Wang's Peano Arithmetic Z, and is obvious in the above argument. In other words Wang implicitly implies that the interpretation of existential quantification cannot be specific to any particular interpretation of a formal mathematical language, but must necessarily be determined by the predicate calculus that is to be applied uniformly to all the mathematical languages in question.

[5]We note that, in this case, Beth explicitly defines the interpretation of the formal P-formula '(Ex)' as 'There is a value of x such that' ([**Be59**], p.178). Thus Beth, too, implies that the interpretation of existential quantification in formalised axiomatics cannot be specific to any

(ii) $(Eq)[G_1(m^0, q, m^0) \;\&\; (s)\{B(s, q) \;\&\; (t)[t \leq s \rightarrow (r)\{H(q, r) \rightarrow \overline{B(t, r)}\}]\}]$

However, again, the inference (ii) from (i) appears to assume that the following deduction is valid for some *unspecified* $\overline{j}$:

$$\neg(q)[G_1(m^0, q, m^0) \rightarrow (s)\{B(s, q) \rightarrow (Et)[t \leq s \;\&\; (Er)\{H(q, r) \;\&\; B(t, r)\}]\}]$$

$$\bullet \;(Eq)\neg[G_1(m^0, q, m^0) \rightarrow (s)\{B(s, q) \rightarrow (Et)[t \leq s \;\&\; (Er)\{H(q, r) \;\&\; B(t, r)\}]\}]$$

$$\star \;\neg[G_1(m^0, \overline{j}, m^0) \rightarrow (s)\{B(s, \overline{j}) \rightarrow (Et)[t \leq s \;\&\; (Er)\{H(\overline{j}, r) \;\&\; B(t, r)\}]\}]$$

$$G_1(m^0, \overline{j}, m^0) \;\&\; (s)\{B(s, \overline{j}) \;\&\; (t)[t \leq s \rightarrow (r)\{H(\overline{j}, r) \rightarrow \overline{B(t, r)}\}]\}$$

$$(Eq)[G_1(m^0, q, m^0) \;\&\; (s)\{B(s, q) \;\&\; (t)[t \leq s \rightarrow (r)\{H(q, r) \rightarrow \overline{B(t, r)}\}]\}]$$

Thus, Beth's conclusion, too, appears to implicitly *assume* both Aristotle's particularisation ($\bullet$) and Rosser's Rule C ($\star$); entailing, ipso facto, that Z is ω-consistent (see §15.6).

16.4. Rosser's original argument *implicitly* presumes ω-consistency

Now, Rosser's claim in his 'extension' ([**Ro36**]) of Gödel's argument ([**Go31**]) is that, whereas Gödel's argument assumes that his Peano Arithmetic, P, is ω-consistent, Rosser's assumes only simple consistency.

However, Rosser's original argument (also a sketch) appears to *implicitly* presume that the system of Peano Arithmetic in question is ω-consistent.

For instance, Rosser defines a P-formula $R(x, y)$ and concludes ([**Ro36**], p.234) that:

(i) If, for any given natural number n, the formula $[\neg R(n, a)]$ in Gödel's Peano Arithmetic P whose Gödel-number is:

$$Neg(Sb(r \;\begin{matrix} u \\ Z(n) \end{matrix} \;\begin{matrix} v \\ Z(a) \end{matrix}\;))$$

is P_κ-provable[6] under the given premises;

(ii) Then, if P is simply consistent, the P-formula $[(\forall u)\neg R(u, a)]$ whose Gödel-number is:

$$uGen(Neg(Sb(r \;\begin{matrix} v \\ Z(a) \end{matrix}\;)))$$

is P_κ-provable;

(iii) Since:

> "...the formal analogue of $(z)[z = 0 \;\vee\; z = 1 \;\vee\; \ldots \vee\; z = x \;\vee\; (Ew)[z = x + w]]$ is provable in P and hence in P_κ, and so $Bew_\kappa(uGen(Neg(Sb(r \;\begin{matrix} v \\ Z(a) \end{matrix}\;))))$".

particular interpretation of a formal mathematical language, but must necessarily be determined by the predicate calculus that is to be applied uniformly to all the mathematical languages in question.

[6] *Notation* (due to Gödel): By 'P_κ-provable' we mean provable from the axioms of P and an arbitrary class, κ, of P-formulas—including the case where κ is empty—by the rules of deduction of P.

However, we note that Rosser's argument in (iii) above would need to assume Rosser's Rule C (as we highlight in §16.5) in any proof sequence in P that involves an existentially quantified P-formula such as '$(Ew)[z = x + w]$', and which yields his conclusion (ii).

By §15.6, this would imply, however, that P is ω-consistent.

16.5. Mendelson's proof highlights where Rosser's argument presumes ω-consistency

We analyse next Mendelson's meticulously detailed expression ([**Me64**], p.145, Proposition 3.32) of Rosser's argument, and highlight where it tacitly presumes that P is ω-consistent.

Now, Gödel defines a formal Peano Arithmetic P, and a primitive recursive relation, $q(x, y)$, that holds if, and only if, x is the Gödel-number of a well-formed P-formula, say $[H(w)]$—which has a single free variable, $[w]$—and y is the Gödel-number of a P-proof of $[H(x)]$.

So, for any natural numbers h, j:

(a) $q(h, j)$ holds if, and only if, j is the Gödel-number of a P-proof of $[H(h)]$.

Rosser's argument defines an additional primitive recursive relation, $s(x, y)$, which holds if, and only if, x is the Gödel-number of $[H(w)]$, and y is the Gödel-number of a P-proof of $[\neg H(x)]$.

Hence, for any natural numbers h, j:

(b) $s(h, j)$ holds if, and only if, j is the Gödel-number of a P-proof of $[\neg H(h)]$.

Further, it follows from Gödel's Theorems V ([**Go31**], p.22) and VII ([**Go31**], p.29) that the primitive recursive relations $q(x, y)$ and $s(x, y)$ are instantiationally equivalent to some arithmetical relations, $Q(x, y)$ and $S(x, y)$, such that, for any natural numbers h, j:

(c) If $q(h, j)$ holds, then $[Q(h, j)]$ is P-provable;

(d) If $\neg q(h, j)$ holds, then $[\neg Q(h, j)]$ is P-provable;

(e) If $s(h, j)$ holds, then $[S(h, j)]$ is P-provable;

(f) If $\neg s(h, j)$ holds, then $[\neg S(h, j)]$ is P-provable;

Now, whilst Gödel defines $[H(w)]$ as:

$$[(\forall y)\neg Q(w, y)],$$

Rosser's argument defines $[H(w)]$ as:

$$[(\forall y)(Q(w, y) \rightarrow (\exists z)(z \leq y \wedge S(w, z)))],$$

Further, whereas Gödel considers the P-provability of the Gödelian proposition,:

$$[(\forall y)\neg Q(h, y)],$$

Rosser's argument considers the P-provability of the proposition:

$$[(\forall y)(Q(h, y) \rightarrow (\exists z)(z \leq y \wedge S(h, z)))].$$

We note that, by definition:

(i) $q(h, j)$ holds if, and only if, j is the Gödel-number of a P-proof of:

$[(\forall y)(Q(h, y) \rightarrow (\exists z)(z \leq y \wedge S(h, z)))]$;

(ii) $s(h, j)$ holds if, and only if, j is the Gödel-number of a P-proof of:

$[\neg((\forall y)(Q(h, y) \rightarrow (\exists z)(z \leq y \wedge S(h, z))))]$.

16.6. Where Mendelson's proof tacitly assumes ω-consistency

(a) We assume, first, that r is the Gödel-number of some proof sequence in P for the Rosser proposition $[(\forall y)(Q(h, y) \rightarrow (\exists z)(z \leq y \wedge S(h, z)))]$.

Hence $q(h, r)$ is true, and $[Q(h, r)]$ is P-provable.

However, we then have that $[Q(h, r) \rightarrow (\exists z)(z \leq r \wedge S(h, z))]$ is P-provable.

Further, by Modus Ponens, we have that $[(\exists z)(z \leq r \wedge S(h, z)))]$ is P-provable.

Now, if P is simply consistent, then $[\neg((\forall y)(Q(h, y) \rightarrow (\exists z)(z \leq y \wedge S(h, z))))]$ is not P-provable.

Hence, $s(h, n)$ does not hold for any natural number n, and so $\neg s(h, n)$ holds for every natural number n.

It follows that $[\neg S(h, n)]$ is P-provable for every P-numeral $[n]$.

Hence, $[\neg((\exists z)(z \leq r \wedge S(h, z)))]$ is also P-provable—a contradiction.

Hence, $[(\forall y)(Q(h, y) \rightarrow (\exists z)(z \leq y \wedge S(h, z)))]$ is not P-provable if P is simply consistent.

(b) We assume next that r is the Gödel-number of some proof-sequence in P for the proposition $[\neg((\forall y)(Q(h, y) \rightarrow (\exists z)(z \leq y \wedge S(h, z))))]$.

Hence $s(h, r)$ holds, and $[S(h, r)]$ is P-provable.

However, if P is simply consistent, $[(\forall y)(Q(h, y) \rightarrow (\exists z)(z \leq y \wedge S(h, z)))]$ is not P-provable.

Hence, $\neg q(h, n)$ holds for every natural number n, and $[\neg Q(h, n)]$ is P-provable for all P-numerals $[n]$.

(i) The foregoing implies $[y \leq r \rightarrow \neg Q(h, y)]$ is P-provable, and we consider the following deduction ([**Me64**], p.146):

(1) $[r \leq k]$ . . . *Hypothesis*
(2) $[S(h, r)]$ . . . *By 3(b)*
(3) $[r \leq k \wedge S(h, r)]$ . . . *From (1), (2)*
(4) $[(\exists z)(z \leq k \wedge S(h, z))]$ . . . *From (3)*

(ii) From (1)-(4), by the Deduction Theorem, we have that $[r \leq k \rightarrow (\exists z)(z \leq k \wedge S(h, z))]$ is provable in P for any P-numeral $[k]$;

(iii) Now, $[k \leq r \vee r \leq k]$ is P-provable for any P-numeral $[k]$;

(iv) Also, $[(k \leq r \rightarrow \neg Q(h, k)) \wedge (r \leq k \rightarrow (\exists z)(z \leq k \wedge S(h, z)))]$ is P-provable for any P-numeral $[k]$.

(v) Hence $[(\neg(k \leq r) \vee \neg Q(h,k)) \wedge (\neg(r \leq k) \vee (\exists z)(z \leq k \wedge S(h,z)))]$ is P-provable for any P-numeral $[k]$.

(vi) Hence $[\neg Q(h,k) \vee (\exists z)(z \leq k \wedge S(h,z))]$ is P-provable for any P-numeral $[k]$.

(vii) Hence $[(Q(h,k) \rightarrow (\exists z)(z \leq k \wedge S(h,z))]$ is P-provable for any P-numeral $[k]$.

(viii) Now, (vii) contradicts our assumption that $[\neg((\forall y)(Q(h,y) \rightarrow (\exists z)(z \leq y \wedge S(h,z))))]$ is P-provable.

(ix) Hence $[\neg((\forall y)(Q(h,y) \rightarrow (\exists z)(z \leq y \wedge S(h,z))))]$ is not P-provable if P is simply consistent.

However, the claimed contradiction in (viii) *only* follows if we assume that P is ω-consistent, and *not* if we assume only that P is simply consistent.

In other words, Mendelson's step (viii) implicitly appeals to Rosser's Rule C (see §B, Appendix B), and assumes that the formula $[\neg(\forall y)(Q(h,y)]$ entails the formula $[\neg(Q(h,k)]$ for some *unspecified* term $[k]$ of P—which is equivalent to the assumption that Aristotle's particularisation holds in any model of P (see §15.6).

CHAPTER 17

Why Gödel's formula does not assert its own unprovability

17.1. Wittgenstein's reservations on the 'meaning' of quantified formulas under Aristotle's particularisation

We note that the ambiguity in the 'meaning' of formal mathematical expressions containing unrestricted existential (and, implicitly, universal) closure under an interpretation was emphasised by Ludwig Wittgenstein as follows:

> "Do I understand the proposition "There is . . ." when I have no possibility of finding where it exists? And in so far as what I can do with the proposition is the criterion of understanding it, thus far it is not clear in advance whether and to what extent I understand it."
> . . . *Wittgenstein:* [**Wi78**].

The significance of Wittgenstein's remark is seen in Gödel's proof of Theorem XI in his seminal 1931 paper ([**Go31**]), where Gödel defined a formula, say $[W]$, in a Peano Arithmetic, P, and *assumed* that $[W]$ translates—under an interpretation of P which admits Aristotle's particularisation—as an arithmetical proposition, say W^*, that is true if, and only if, a specified formula of P is unprovable in P.

Gödel then argued that his formula $[W]$ is not P-provable if P is ω-consistent, from which he concluded that the consistency of the Peano Arithmetic P is not provable within the Arithmetic.

17.2. Gödel's argument for his Theorem XI

Specifically, Gödel, first, showed how 46 meta-propositions about P can be defined by means of primitive recursive functions and relations.

These included:

(#23) A primitive recursive relation, $Form(x)$, which is true if, and only if, x is the Gödel-number of a formula of P;

(#43) A primitive recursive relation, $Fl(x, y, z)$, which is true if, and only if, x is the Gödel-number of a P-formula that is an immediate consequence in P of the two P-formulas whose Gödel-numbers are y and z;

(#44) A primitive relation, $Bw(x)$, which is true if, and only if, x is the Gödel-number of a finite sequence of P-formulas each of which is either an axiom of P, or an immediate consequence in P of two preceding formulas;

(#45) A primitive recursive relation, xBy, which is true if, and only if, x is the Gödel-number of a proof sequence of P whose last formula has the Gödel-number y.

Gödel assured the constructive nature of the first 45 definitions by specifying:

> "Everywhere in the following definitions where one of the expressions (x), (Ex), ϵx occurs it is followed by a bound for x. This bound serves only to assure the recursive nature of the defined concept."
> *... Gödel:* [**Go31**], *p.17, footnote 34.*

Gödel then defined an unbounded meta-mathematical proposition that is not primitive recursive:

(#46) The proposition, $Bew(x)$, is true if, and only if, $(\exists y)yBx$ is true.

Thus $Bew(x)$ is true if, and only if, x is the Gödel-number of a provable formula of P.

17.3. The significance of Gödel's Theorem VII

Now, by Gödel's Theorem VII, any recursive relation, say $Q(x)$, can be represented in P by some, corresponding, arithmetical formula, say $[R(x)]$, such that, for any natural number n:

If $Q(n)$ is true, then $[R(n)]$ is P-provable

If $Q(n)$ is false, then $[\neg R(n)]$ is P-provable.

However, Gödel's reasoning in the first half of his Theorem VI established that the above representation does not extend to the closure of a recursive relation, in the sense that we cannot always assume:

If $(\forall x)Q(x)$ is true (i.e, $Q(n)$ is true for any given natural number), then $[(\forall x)R(x)]$ is P-provable.

In other words, we cannot assume that, even though the recursive relation $Q(x)$ is instantiationally equivalent to any well-defined interpretation of the P-formula $[R(x)]$, the number-theoretic proposition $(\forall x)Q(x)$ must, necessarily, be logically equivalent to the corresponding interpretation of the P-formula $[(\forall x)R(x)]$.

Reason: In recursive arithmetic, the expression '$(\exists x)F^*(x)$' is an abbreviation for the assertion:

(*) There is some (algorithmically computable) natural number n such that $F^*(n)$ holds.

In Peano Arithmetic, however, the formula '$[(\exists x)F(x)]$' is simply an abbreviation for '$[\neg(\forall x)\neg F(x)]$', which, under an evidence-based finitary interpretation of PA (see §9) asserts that:

(**) The relation $\neg F^*(x)$ is not algorithmically computable as always true in $\mathbb{N}$.

Moreover, Gödel's Theorem VI established (see also §11.4) that we cannot conclude (*) from (**) without risking inconsistency, since $\neg F^*(x)$ may be algorithmically verifiable, but not algorithmically computable, as always true in $\mathbb{N}$.

Consequently, although a primitive recursive relation may be instantiationally equivalent to a well-defined interpretation of a P-formula, we cannot assume that the existential closure of the relation must have the same meaning as the interpretation of the existential closure of the corresponding P-formula (cf. §21.12).

However this, precisely, is the implicit presumption made by Gödel in the proof of Theorem XI, from which he concluded that the consistency of P is not P-provable.

17.4. Gödel's implicit presumption in his Theorem XI

Specifically, Gödel first defined the notion of 'P is consistent' classically as follows:

> P is consistent if, and only if, $Wid(\mathrm{P})$ is true

where $Wid(\mathrm{P})$ is defined symbolically as:

> $(\exists x)(Form(x) \wedge \neg Bew(x))$,

which is merely an abbreviation for:

> There is a natural number n which is the Gödel-number of a formula of P, and this formula is not P-provable.

Thus, $Wid(\mathrm{P})$ is true if, and only if, P is consistent (since an inconsistent P would prove every P-formula).

However, Gödel, then, presumed that:

(i) If the recursive relation, $Q(x, y)$ ([**Go31**], p24, eqn.(8.1)), is represented by the P-formula $[R(x, y)]$, and p is the Gödel-number of the P-formula $[R(x, y)]$, then the proposition:

"$[(\forall x)R(x, p)]$ is true under a well-defined interpretation $\boldsymbol{I}$ of P"

is logically equivalent to (i.e., has the same meaning as)

"$(\forall x)Q(x, p)$ is true";

(ii) The existentially quantified meta-statement $Wid(\mathrm{P})$ can be *unambiguously* represented by some formula $[W]$ of P such that:

"$[W]$ is true under a well-defined interpretation $\boldsymbol{I}$ of P",

and

"$Wid(\mathrm{P})$ is true",

are logically equivalent (i.e., have the same meaning).

Gödel then argued that:

(a) Since the P-formula $[(\forall x)R(x, p)]$ is not provable in P, it asserts its own unprovability ([**Go31**], p37, footnote 67);

and the latter to conclude that:

(b) Since the P-formula $[W]$ is not provable in P, the consistency of P is unprovable in P ([**Go31**], p.36, Theorem XI).

17.5. Gödel's formula does not assert its own unprovability

However, there is an inherent ambiguity in the classical interpretation of quantification (see §21), insofar that although 17.4(a), for instance, does follow (by Theorem 10.2) if:

(i) "$[(\forall x)R(x,p)]$ is true under an interpretation $\boldsymbol{I}$ of P over $\mathbb{N}$"

translates as (see Definitions 5.2 and 5.3):

(ii) "$R^*(x,p)$ is algorithmically computable as always true in $\mathbb{N}$ under $\boldsymbol{I}$",

it does not if (i) translates as:

(iii) "$R^*(x,p)$ is algorithmically *verifiable* as always true in $\mathbb{N}$, but it is not algorithmically *computable* as always true in $\mathbb{N}$, under $\boldsymbol{I}$"

where the P-formula $[(\forall x)R(x,p)]$ interprets as the arithmetical relation $R^*(x,p)$ in $\mathbb{N}$ under $\boldsymbol{I}$.

In other words:

THEOREM 17.1. *The P-formula $[(\forall x)R(x,p)]$ does not assert its own unprovability in P.*

PROOF. We have for Gödel's primitive recursive relation $Q(x,y)$ that:

(a) $Q(x,p)$ is true if, and only if, the P-formula $[R(x,p)]$ is not provable in P.[1]

Further, Gödel's Theorem VI establishes that, if P is consistent, then (see Definition 5.2):

(b) The arithmetical interpretation $R^*(x,p)$ of the P-formula $[R(x,p)]$ is algorithmically verifiable as always true over the structure $\mathbb{N}$ of the natural numbers.[2]

Now, in order to conclude that the P-formula $[(\forall x)R(x,p)]$ asserts its own unprovability in P, Gödel's argument must further imply the stronger meta-statement (see Definition 5.3):

(c) The arithmetical interpretation $R^*(x,p)$ of the P-formula $[R(x,p)]$ is algorithmically computable as always true over the structure $\mathbb{N}$ of the natural numbers,

from which we may then conclude that:

(d) The primitive recursive relation $Q(x,p)$ is algorithmically computable as always true if, and only if, the arithmetical interpretation $R^*(x,p)$ of the P-formula $[R(x,p)]$ is algorithmically computable as always true over the structure $\mathbb{N}$ of the natural numbers.

---

[1] *Comment*: In Gödel's terminology, '$Q(x,p) \equiv \overline{xB_\kappa[Sb(p\ {}^{19}_{Z(p)})]}$' ([**Go31**], p.24, eqn.(8.1)).

[2] *Comment*: An immediate consequence, in Gödel's terminology, of '$(n)Bew_\kappa[Sb(r\ {}^{17}_{Z(n)})]$' ([**Go31**], p.26, #2).

However, this is not possible since (c) and (d) would then yield the contradiction:

(e) By Theorem 10.2, $(\forall x)Q(x,p)$ is true (i.e., $Q(x,p)$ is algorithmically computable as always true) if, and only if, the P-formula $[(\forall x)R(x,p)]$ is provable in P;

whereas:

(f) By definition ([**Go31**], p.24, eqn.8.1), if $(\forall x)Q(x,p)$ is true, then the P-formula whose Gödel-number is p, i.e., the formula $[(\forall x)R(x,y)]$, is not provable in P when the numeral $[p]$ is substituted for the variable $[y]$ (in other words, the formula $[(\forall x)R(x,p)]$ is not provable in P).

The theorem follows. □

17.6. Gödel's argument does not support his claim in Theorem XI

Assuming that the same objection would apply to 17.4(b) had Gödel defined W explicitly[3]—as he had defined $R(x,p)$—we conclude that, at best, Gödel's reasoning can only be taken to establish that the consistency of P is not provable in P by a P-formula that interprets as an algorithmically computable truth in $\mathbb{N}$.

In other words—contrary to conventional wisdom (e.g., [**Vo10**]; [**EC89**], Theorem 5, p.211; [**Sm92**], p.109; [**Da82**], p.129; [**Sh67**], pp.212-213; [**Me64**], p.148)—Gödel's particular argument, based on his definition of $Wid(\mathrm{P})$, does not support the broader claim of his Theorem XI.

17.7. A curious interpretation of Gödel's claim

> "A simple example would be a proof of $1 = 0$ from the axioms of (first-order) Peano Arithmetic: PA + not-Con(PA) is consistent (assuming PA is), so it has a model that thinks there's a proof of $1 = 0$ from PA; but viewed set-theoretically, that model is benighted, the thing it takes for a proof of $1 = 0$ has nonstandard length, isn't really a proof."
> *. . . Maddy:* [**Ma18**], *p.12.*

A curious interpretation of Gödel's claim is highlighted by Penelope Maddy's argument in [**Ma18**] that, if we assume the P-formula $[W]$ can, indeed, be interpreted as '$Wid(\mathrm{P})$ is true' under some well-defined interpretation $\boldsymbol{I}$ of P, then it would follow from:

(i) the unprovability of the formula $[W]$ in P, and

(ii) the unprovability of the formula $[\neg W]$ in P (since P is assumed ω-consistent),

that the theory P+ $[\neg W]$ would not only be consistent, but have a well-defined interpretation of P under which the P-formula $[\neg W]$ would 'truthfully' assert that:

'$Wid(\mathrm{P})$ is false; whence P is inconsistent and $1 = 0$'!

---

[3]That this may have been Gödel's original intent is suggested by his concluding remarks in [**Go31**] (p.38):

> "We have limited ourselves in this paper essentially to the system P and have only indicated the applications to other systems. The results will be expressed and proved in full generality in a sequel to appear shortly. Also in that paper, the proof of Theorem XI, which has only been sketched here, will be presented in detail."

CHAPTER 18

Must BPCM admit non-constructive set-theoretical structures?

Another significant feature of BPCM is the, tacitly reluctant, admission (in §13.4) that Cohen's proof of the independence of the Axiom of Choice *compels* constructive mathematics to accommodate (through appropriate interpretation) the gamut of putative set-theoretical structures—which Hilbert alluded to as Cantor's 'paradise' ([**Hi27**], p.376)—that satisfy the first-order Zermelo-Fraenkel Set Theory ZF.

> **Axiom of Choice** (a standard interpretation): Given any set S of mutually disjoint non-empty sets, there is a set C containing a single member from each element of S.

Such a perspective appears to tacitly admit the widely-held belief that all *significant* mathematical 'truths' — such as, for example, the theorems of a first-order Peano Arithmetic (PA) — can be suitably interpreted as theorems of a set-theory such as ZFC (i.e., ZF plus an axiom of choice) without *any* loss of generality (see, for example, [**Me64**], pp.192-193).

For instance, in a 1991 lecture on *The Future of Set Theory*, Saharon Shelah presents an overview of classical Set Theory that is based on an implicit thesis that mathematical truth is intuitive and essentially non-verifiable, and on the explicit belief that:

> "... ZFC exhausts our intuition except for things like consistency statements, so a proof means a proof in ZFC ... all of us are actually proving theorems in ZFC."
>
> ... *Shelah:* [**She91**].

A similar thesis is, curiously, reflected as 'fact' in John R. Steel's *Mathematics Needs New Axioms*:

> "It is a familiar but remarkable fact that all mathematical languages can be translated into the language of set theory, and all theorems of 'ordinary' mathematics can be proved in ZFC."
>
> ... *Steel:* [**FFMS**], *p.423.*

The belief that the set theory ZF is a lingua franca of *verifiable* mathematics—despite the essential non-verifiability of the axiom of infinity in any *evidence-based* interpretation of the theory[1]—is reflected in recent arguments by Sieg and Walsh on the *verifiability* of formalizations of the Cantor-Bernstein Theorem in ZF, via the *proof assistant* AProS which 'allows the direct construction of formal proofs'—containing quantifiers—'that are humanly intelligible':

[1]An intriguing, but debatable, unconscionable origin of such belief is tacit in Lakoff and Núñez's arguments in [**LR00**], where they view set theory as the language of the conceptual metaphors by which, they claim, the embodied brain brings mathematics into being.

> "The objects of proof theory are proofs, of course. This assertion is however deeply ambiguous. Are proofs to be viewed as formal derivations in particular calculi? Or are they to be viewed as the informal arguments given in mathematics?—The contemporary practice of proof theory suggests the first perspective, whereas the programmatic ambitions of the subject's pioneers suggest the second. We will later mention remarks by Hilbert (in sections 5 and 7) that clearly point in that direction. Now we refer to Gentzen who inspired modern proof theoretic work; his investigations and insights concern *prima facie* only formal proofs. However, the detailed discussion of the proof of the infinity of primes in his [Gentzen, 1936, pp. 506-511] makes clear that he is very deeply concerned with *formalizing* mathematical practice. The crucial problem is finding the atomic inference steps involved in informal arguments. The inference steps Gentzen brings to light are, perhaps not surprisingly, the introduction and elimination rules for logical connectives, including quantifiers.
>
> Gentzen specifies in [Gentzen, 1936, p. 513] the concept of a *deduction* and adds in parentheses *formal image of a proof*; i.e., deductions are viewed as formal images of mathematical proofs and are obtained by formalizing the latter. The process of formalization is explained as follows: "The words of ordinary language are replaced by particular *signs*, the logical inference steps [are replaced by] rules that form new formally presented statements from already proved ones." Only in this way, he claims, is it possible to obtain a "rigorous treatment of proofs". However, and that is strongly emphasized, "The objects of proof theory shall be the *proofs* carried out in mathematics proper." [Gentzen, 1936, p. 499] For us, the formalization of proofs is the quasi-empirical starting point for uncovering *proof methods in mathematics*; formal rigor is not to be considered a foe of simplicity or understanding.
>
> When extending the effort from logical to mathematical reasoning one is led to the task of devising additional tools for the *natural formalization of proofs*. Such tools should serve to directly reflect standard mathematical practice and preserve two central aspects of that practice, namely, (1) the axiomatic and conceptual organization in support of proofs and (2) the inferential mechanisms for logically structuring them. Thus, the natural formalization in a deductive framework *verifies* theorems relative to that very framework, but it also deepens our understanding and isolates core ideas; the latter lend themselves often, certainly in our case, to a diagrammatic depiction of a proof's conceptual structure. . . .
>
> We chose as the deductive framework Zermelo-Fraenkel set theory ZF. One can clearly choose different ones, for example, Higher Order Logic, Martin Löf's Type Theory or Feferman's Explicit Mathematics. The language of set theory is, however, the *lingua franca* of contemporary mathematics and ZF its foundation. So it seems both important and expedient to use ZF for the project of formalizing proofs naturally."
>
> . . . *Sieg and Walsh:* [**SW17**].

The reason such a belief—clearly ambiguous in the absence of explicit, *evidence-based*, definitions of *weak* and *strong* quantification that must necessarily precede any formal definition of mathematical truth (see §4.3 and §5.1)—does not seem unreasonable is that it reflects conventional wisdom which—for over a generation—has been explicitly echoed in standard texts and literature with increasing certitude:

- "It is not at all obvious at first glance that every mathematical discipline can be reduced to a formalized theory of the standard type. The crucial point here consists in carrying out such a reduction for the general theory of sets, since as we know from the work of Frege and his followers, and in particular from Whitehead and Russell's *Principia Mathematica*, the whole of mathematics can be formalized within set theory." . . . "
  . . . *Tarski: (*[**Ta39**]*, p.164)*

- "... NBG apparently can serve as a foundation for all present-day mathematics (i.e., it is clear to every mathematician that every mathematical theorem can be translated and proved within NBG, or within extensions of NBG obtained by adding various extra axioms such as the Axiom of Choice) ... "

  ... *Mendelson: (*[**Me64**]*, p.193)*

- "Today set theory plays a role similar to that played by Euclidean geometry for over over 15 centuries (up to the time of the construction of mathematical analysis by Newton and Leibniz). Namely, it is a universal axiomatic theory for modern mathematics. ...

  We conjecture that set theory will remain the most useful and inspiring universal theory on which all of mathematics can be based."

  ... *Marek and Mycielski: (*[**MM01**]*, p.459 & p.467 respectively)*

- "Such is the case, for instance, with the formal systems considered in works on set theory, such as the one known as ZFC, which are adequate for formalizing essentially all accepted mathematical proofs."

  ... *Boolos, Burgess, and Jeffrey: (*[**BBJ03**]*, p.225)*

- "The system of set theory introduced by Zermelo in [Zermelo, 1908] was intended to show, 'how the entire theory created by Cantor and Dedekind can be reduced to a few definitions and seven principles, or axioms, which appear to be mutually independent.' In the last section we described an expanded frame for our formalization project: a definitional extension of ZF together with a flexible rule-based inferential mechanism. The latter includes not only I- and E- rules for the logical connectives, but also for defined notions. This mechanism is absolutely critical, if one wants to reflect mathematical practice and exploit the conceptual, hierarchical organization of parts of mathematics that are represented in set theory. ... We consider the basic frame for our project we just described as level 0 of the hierarchy. This conservative extension of ZF can be further expanded to level 1, where relations and functions are introduced as set theoretic objects. That is in full harmony with Zermelo's view of set theory as 'that branch of mathematics whose task is to investigate mathematically the fundamental notions 'number', 'order', and 'function', taking them in their pristine, simple form, and to develop thereby the logical foundations of all of arithmetic and analysis; thus it constitutes an indispensable component of the science of mathematics.' [Zermelo, 1908, p. 261]

  A little more than ten years later, Hilbert discussed in 1920 Zermelo's axiom system and claims that it is the 'most comprehensive mathematical system'. He supports that claim by a penetrating observation:

  > The theory which results from the development of the consequences of this axiom system [Zermelo's] encompasses all mathematical theories (like number theory, analysis, geometry), in the sense that the relations which obtain between the objects of these mathematical disciplines are represented in a perfectly corresponding way by relations which obtain within a subdomain of Zermelo's set theory. [Hilbert, 2013, p. 292]"

  ... *Sieg and Walsh:* [**SW17**].

It is a belief that, curiously, is tacitly shared by computer scientists, whose discipline epitomises constructive mathematical practices:

> "Mathematics can be axiomatized using for example the Zermelo Frankel system, which has a finite description."
>
> ... *Arora and Barak: (*[**Ar09**]*, pp.2.24(60), Ex.6, Ch.2.)*

If one accepts such a belief, then the goal of constructive mathematics vis à vis ZF should, reasonably, be to assign *evidence-based* truth values to the constructively interpretable ZF propositions in some putative set-theoretical structure in Cantor's 'paradise'.

However, as we concluded from Theorem 10.2 in Chapter 10, even if we accept that a set theory such as ZF may be *the* appropriate language for the symbolic expression of Lakoff and Núñez's 'conceptual metaphors', by which an individual's 'embodied mind brings mathematics into being' (see [**LR00**]), it is the *strong* finitary interpretation of the first-order Peano Arithmetic PA (see Theorem 9.7) that makes PA a *lingua franca* of adequate expression and effective communication for contemporary mathematics and its foundations, since it allows us to bridge arithmetic provability and arithmetic computability constructively in the sense of, say, [**CCS01**].

Moreover, the case for perforce induction into the language of constructive mathematics of a gamut of such—admittedly non-constructive—structures under any putative interpretation of ZF collapses if we note that Cohen's proof appeals explicitly to the intuitionistically objectionable Aristotle's particularisation.

18.1. Cohen's proof appeals to Aristotle's particularisation

The significance of the assumption of Aristotle's particularisation is highlighted in a 1927 address in which Hilbert reviewed, as part of his 'proof theory', his axiomatisation $\mathcal{L}_\varepsilon$ of classical predicate logic as a formal first-order ε-predicate calculus ([**Hi27**], pp.465-466).

A specific aim of the axiomatisation appears to have been the introduction of a primitive choice-function symbol, 'ε', for formalising the existence of the *unspecified* object in Aristotle's particularisation ([**Ca62**], p.156):

> "... $\varepsilon(A)$ stands for an object of which the proposition $A(a)$ certainly holds if it holds of any object at all ..."[2]
>
> ... *Hilbert:* ([**Hi25**], *p.382)*

Hilbert showed, moreover, how the universal and existential quantifiers—classically denoted by '$\forall$' and '$\exists$'—are formally definable using the choice-function 'ε' (see §4.1)—and noted that:

> "... The fundamental idea of my proof theory is none other than than to describe the activity of our understanding, to make a protocol of the rules according to which our thinking actually proceeds."
>
> ... *Hilbert:* ([**Hi27**], *p.475)*

More precisely, he showed that (cf. [**Hi25**], pp.382-383; [**Hi27**], p.466(1)):

LEMMA 18.1. *$\mathcal{L}_\varepsilon$ adequately expresses—and yields, under a suitable interpretation—classical predicate logic if the ε-function is interpreted so as to yield the unspecified object in Aristotlean particularisation.* □

What came to be known later as Hilbert's Program[3]—which was built upon Hilbert's 'proof theory'—can be viewed as, essentially, the subsequent attempt to show that the formalisation was also necessary for communicating propositions of

---

[2] *Comment*: We note that Hilbert here *postulates without qualification* that $\varepsilon(A)$ can be treated as a 'term' if $\mathcal{L}_\varepsilon$ is first-order. The need for qualification arises since, by Theorem 11.10, $\varepsilon(A)$ can be considered a term of any first-order $\mathcal{L}_\varepsilon$ if, and only if, $A(a)$ 'holds' for some term a of $\mathcal{L}_\varepsilon$ that is recursively definable in terms of the primitive terms of $\mathcal{L}_\varepsilon$.

[3] See, for instance, the Stanford Encyclopedia of Philosophy: Hilbert's Program.

classical predicate logic effectively and unambiguously under any interpretation of the formalisation.

This goal is implicit in Hilbert's remarks:

> "Mathematics in a certain sense develops into a tribunal of arbitration, a supreme court that will decide questions of principle—and on such a concrete basis that universal agreement must be attainable and all assertions can be verified."
>
> . . . *Hilbert: ([**Hi25**], p.384)*

> ". . . a theory by its very nature is such that we do not need to fall back upon intuition or meaning in the midst of some argument."
>
> . . . *Hilbert: ([**Hi27**], p.475)*

18.2. Aristotle's particularisation is 'stronger' than the Axiom of Choice

The difficulty in attaining this goal constructively along the lines desired by Hilbert—in the sense of the above quotes—becomes evident from Rudolf Carnap's analysis in a 1962 paper on the use of Hilbert's ε-operator in scientific theories ([**Ca62**], pp.157-158; see also Wang's remarks [**Wa63**], pp.320-321):

> "What now is the connection between the ε-operator and the axiom of choice? Is the acceptance of the former tantamount to that of the latter? In more formal terms, is the axiom of choice derivable from the other axioms of set theory if the underlying logic contains the ε-operator with its axioms? In some sense, this is the case, but the assertion needs some qualifications. . . . The decisive point for this question of derivability is the specific form of the axiom schema of subsets (*Aussonderungsaxiom*). In the customary language L it may be formulated as follows, where "Su" stands for "u is set":
>
> (4) '$Su \supset (\exists y)\ [Sy \cdot (v)(v \in y \equiv v \in u\ \cdot\ \phi)]$' where ϕ is any sentential formula of language L containing 'v' as the only free variable.
>
> If L_ε is taken as the axiomatic language, there is the choice of two versions of the axiom schema, differing in the kinds of formulas admitted as ϕ. The first version is the same as (4): only the formulas of L_ε without 'ε' are admitted; in other words, formulas of L (as a sub-language of L_ε). The second version, which we shall call (4_ε), is formed from (4) by replacing 'L' with 'L_ε'. (4_ε) is stronger than (4). But to accept this version seems natural, once the ε-operator has been accepted as a primitive logical constant.
>
> Consider now the principle of choice:
>
> (5) If x is a set such that:
>
> (a) any element of x is non-empty,
>
> (b) any two distinct elements of x are disjoint,
>
> then there is a set y (called a selection set of x) such that
>
> (c) $y \subset \bigcup x$,
>
> (d) for any element z of x, $y \cap z$ has exactly one element.
>
> It can now be seen easily that, if the axiom schema of subsets is taken in the stronger form (4_ε), then (5) is derivable. The derivation is as follows. Let x be any set satisfying the conditions (a) and (b) in (5). According to the axiom of the union set, $\bigcup x$ is a set. Therefore, by (4_ε), there is a set y containing exactly those elements v of $\bigcup x$ for which
>
> $(\exists z)\ [z \in x\ \cdot\ v = \varepsilon_u(u \in z)]$,

(This last formula is taken as ϕ in (4_{ε}).) Thus y is a subset of $\bigcup x$ containing just the representative of the elements of x. Hence y satisfies the conditions (c) and (d) in (5). Thus (5) is derived."

*... Carnap: ([**Ca62**], pp.157-158)*

Now, it follows from Carnap's analysis that, if we define a formal language ZF_{ε} by replacing (see §4.1):

$$[(\forall x)F(x)] \text{ with } [F(\varepsilon_x(\neg F(x)))]$$

$$[(\exists x)F(x)] \text{ with } [F(\varepsilon_x(F(x)))]$$

in the Zermelo-Fraenkel set theory ZF, then:

LEMMA 18.2. *The Axiom of Choice is true in any putative interpretation of the Zermelo-Fraenkel set theory ZF_{ε} that admits Aristotle's particularisation.* □

Thus, the postulation of an *unspecified* object in Aristotlean particularisation is a stronger postulation than the Axiom of Choice!

18.3. Cohen and The Axiom of Choice

The significance of this is seen in the accepted interpretation of Cohen's argument in his 1963-64 papers ([**Co63**] & [**Co64**]); the argument is accepted as definitively establishing that the Axiom of Choice is essentially independent of a set theory such as ZF.

However Cohen's argument—in common with the arguments of many important theorems in standard texts on the foundations of mathematics and logic—appeals to the *unspecified* object in Aristotle's particularisation when interpreting the existential axioms of ZF (or statements about ZF ordinals).

This is seen in his proof ([**Co66**], p.19) and application of the—seemingly paradoxical (see Skolem's remarks [**Sk22**], p295; also [**Co66**], p.19)—Löwenheim-Skolem Theorem ([**Lo15**], p.245, Theorem 6; [**Sk22**], p.293).

(Downwards) Löwenheim-Skolem Theorem: If a first-order proposition is satisfied in any domain at all, then it is already satisfied in a denumerably infinite domain.

Cohen appeals to this theorem for legitimising putative models of a formal theory—such as a standard model 'M' of ZF ([**Co66**], p.19 & p.82), and its forced derivative 'N' ([**Co66**], p.121)—in his argument ([**Co66**], p.83 & p.112-118).

The significance of Hilbert's formalisation of Aristotle's particularisation by means of the ε-function is now seen in Cohen's following remarks, where he explicitly appeals in the above argument to a semantic—rather than formal—definition of the *unspecified* object in Aristotle's particularisation:

"When we try to construct a model for a collection of sentences, each time we encounter a statement of the form $(\exists x)B(x)$ we must invent a symbol $\overline{x}$ and adjoin the statement $B(\overline{x})$. ... when faced with $(\exists x)B(x)$, we should choose to have it false, unless we have already invented a symbol $\overline{x}$ for which we have strong reason to insist that $B(\overline{x})$ be true."

*... Cohen: ([**Co66**], p.112; see also p.4)*

Cohen, then, shows that:

Lemma 18.3. *The Axiom of Choice is false in N.* □

18.4. Any interpretation of ZF which appeals to Aristotle's particularisation is not constructively well-defined

Since Hilbert's ε-function formalises precisely Cohen's concept of '$\overline{x}$'—more properly, '$\overline{x}_B$'—as $[\varepsilon_x B(x)]$, it immediately follows that:

Theorem 18.4. *Any model of ZF which admits Aristotle's particularisation is a model of ZF*$_\varepsilon$ *if the expression* $[\varepsilon_x B(x)]$ *is interpreted to yield Cohen's symbol* '$\overline{x}_B$' *whenever* $[B(\varepsilon_x(B(x)))]$ *interprets as true.* □

Hence Cohen's argument is also applicable to ZF$_\varepsilon$. However, since the Axiom of Choice is true in any interpretation of ZF$_\varepsilon$ which appeals to classical predicate logic, Cohen's argument ([**Co63**] & [**Co64**]; [**Co66**])—when applied to ZF$_\varepsilon$—actually shows that (see also Corollary 11.11):

Corollary 18.5. *ZF has no constructively well-defined model that appeals to Aristotle's particularisation.* □

We cannot, therefore, conclude that the Axiom of Choice is essentially independent of the axioms of ZF, since none of the putative models 'forced' by Cohen (in his argument for such independence) are constructively well-defined by any interpretation of ZF.

18.5. Cohen and the Gödelian argument

We note that, at the conclusion of his lectures on 'Set Theory and the Continuum Hypothesis', delivered at Harvard University in the spring term of 1965, Cohen also remarked:

> "We close with the observation that the problem of CH is not one which can be avoided by not going up in type to sets of real numbers. A similar undecidable problem can be stated using only the real numbers. Namely, consider the statement that every real number is constructible by a countable ordinal. Instead of speaking of countable ordinals we can speak of suitable subsets of ω. The construction $\alpha \to F_\alpha$ for $\alpha \leq \alpha_0$, where α_0 is countable, can be completely described if one merely gives all pairs (α, β) such that $F_\alpha \in F_\beta$. This in turn can be coded as a real number if one enumerates the ordinals. In this way one only speaks about real numbers and yet has an undecidable statement in ZF. One cannot push this farther and express any of the set-theoretic questions that we have treated as statements about integers alone. Indeed one can postulate as a rather vague article of faith that any statement in arithmetic is decidable in "normal" set theory, i.e., by some recognizable axiom of infinity. This is of course the case with the undecidable statements of Gödel's theorem which are immediately decidable in higher systems."
>
> . . . *Cohen: (*[**Co66**]*, p.151)*

Cohen appears to assert here that if ZF is consistent, then we can 'postulate as a rather vague article of faith' that the Continuum Hypothesis is subjectively true for the integers under some model of ZF, but—along with the Generalised Continuum Hypothesis—we cannot objectively (i.e., on the basis of *evidence-based* reasoning) assert it to be true for the integers[4] since it is not provable in ZF, and hence not true in all models of ZF.

However, by this argument, Gödel's undecidable arithmetical propositions, too, can be similarly postulated to be subjectively true for the integers under the *weak* standard interpretation $\boldsymbol{M}$ of PA (as defined in §A, Appendix A), but cannot be objectively (i.e., on the basis of *evidence-based* reasoning) asserted to be true for the integers since the statements are not provable in an ω-consistent PA, and hence they are not true in all models of an ω-consistent PA!

The latter is, essentially, John Lucas' well-known Gödelian argument ([**Lu61**]), forcefully argued by Roger Penrose in his popular expositions, 'Shadows of the Mind' ([**Pe94**]) and 'The Emperor's New Mind' ([**Pe90**]). As argued in The Reasoner ([**An07a**]; [**An07b**]; [**An07c**]), the thesis is plausible, but the specific argument unsound. It is based on a misinterpretation—of what Gödel actually proved formally in his 1931 paper—for which, moreover, neither Lucas nor Penrose ought to be taken to account ([**An07b**]; [**An07c**]). Moreover, the appropriate argument for Lucas' Gödelian thesis ought to be the one in §27

The distinction sought to be drawn by Cohen is curious, since we have shown that his argument—which assumes that constructively well-defined interpretations of ZF can appeal to Aristotle's particularisation—actually establishes that constructively well-defined interpretations of ZF cannot appeal to Aristotle's particularisation; just as it follows from Corollary 11.6 that Gödel's argument—in [**Go31**], p.24, Theorem VI—actually establishes that PA is not ω-consistent, whence any constructively well-defined interpretation of PA, too, cannot appeal to Aristotle's particularisation.

Loosely speaking, the cause of the undecidability of the Continuum Hypothsis—and of the Axiom of Choice—in ZF as shown by Cohen, and that of Gödel's undecidable proposition in Peano Arithmetic, is common; it is interpretation of the existential quantifier under an interpretation as Aristotlean particularisation.

In Cohen's case, such interpretation is made explicitly and unrestrictedly in the underlying predicate logic ([**Co66**], p.4) of ZF, and in its interpretation in classical predicate logic ([**Co66**] p.112).

In Gödel's case it is made explicitly—but formally to avoid attracting intuitionistic objections—through his specification of what he believed (cf. §15.1) to be a 'much weaker assumption' of ω-consistency for his formal system P of Peano Arithmetic ([**Go31**], p.9 & pp.23-24).

[4]Compare with the *evidence-based* proof in §19.3 that $\aleph_0 \longleftrightarrow 2^{\aleph_0}$ in constructive mathematics; also with Hilbert's remarks on the continuum problem in [**Hi25**], pp.384-385.

CHAPTER 19

Functions as *explications* of non-terminating *processes*

We shall argue next that (in view of Theorem 19.7), instead of defining real numbers as the putative limit of putatively definable Cauchy sequences[1] that 'exist' in some Platonic sense in the interpretation of an arithmetic, we can alternatively define—from the perspective of constructive mathematics, and without any loss of generality—such numbers instead by their *evidence-based*, algorithmically *verifiable*, number-theoretic functions (see §5) that formally express—in the sense of Carnap's 'explication' —the corresponding Cauchy sequences viewed now as non-terminating *processes* in the standard interpretation of the arithmetic that may, sometimes, tend to a discontinuity (see §24.3, Case 2(a) and 2(b)).

> "By the procedure of *explication* we mean the transformation of an inexact, prescientific concept, the *explicandum*, into a new exact concept, the *explicatum*. Although the explicandum cannot be given in exact terms, it should be made as clear as possible by informal explanations and examples. . . . A concept must fulfill the following requirements in order to be an adequate explicatum for a given explicandum: (1) similarity to the explicandum, (2) exactness, (3) fruitfulness, (4) simplicity."
>
> *. . . Carnap:* [**Ca62a**]*, p.3 & p.5.*

19.1. A constructive arithmetical perspective on Cantor's Continuum Hypothesis

We first show that the distinction[2] between algorithmically *verifiable*, and algorithmically *computable*, number-theoretic functions (see Theorem 5.4) yields an unusual, constructive, arithmetical perspective of Cantor's Continuum Hypothesis (CH).

> **Cantor's Continuum Hypothesis**: There is no set whose cardinality is strictly between the cardinality $\aleph_0$ of the integers and the cardinality $2^{\aleph_0}$ of the real numbers.

We note that Gödel showed in 1939 ([**Go40**]) that CH is consistent with the usual Zermelo-Fraenkel (ZF) axioms for set theory if ZF is consistent. He defined a putative model of ZF in which both the Axiom of Choice (AC) and CH hold.

[1] 'putatively definable' since not all Cauchy sequences are algorithmically computable (Theorem 5.4). The significance of this distinction for the physical sciences is highlighted in §29.6 and §29.7

[2] The distinction was introduced—and its significance highlighted—in [**An16**]. Since set-theoretic functions are defined extensionally, it is not obvious how—or even whether—this distinction can be reflected within ZF.

Further, Cohen showed in 1963 ([**Co66**]) that the negations of AC and CH are also consistent with ZF; in particular, CH can fail while AC holds in a putative model of ZF if ZF is consistent.

We now show how—justifying Skolem's 'apparent paradox' observations in [**Sk22**] (p.295; see also [**Kl52**], p.427)) Gödel's β-function uniquely corresponds each real number to an algorithmically verifiable arithmetical function.

We conclude that, although the Continuum Hypothesis is independent of the axioms of ZF if ZF is consistent, the arithmetic interpretation of $\aleph_0 \longleftrightarrow 2^{\aleph_0}$ follows from the axioms of PA (which is consistent by Theorem 9.10).

19.2. Gödel's β-function

We note that Gödel's β-function is defined as ([**Me64**], p.131):

$$\beta(x_1, x_2, x_3) = rm(1 + (x_3 + 1) \star x_2, x_1)$$

where $rm(x_1, x_2)$ denotes the remainder obtained on dividing x_2 by x_1.

We also note that:

LEMMA 19.1. *For any non-terminating sequence of values $f(0), f(1), \ldots,$ we can construct natural numbers b_k, c_k such that:*

(i) $j_k = max(k, f(0), f(1), \ldots, f(k))$;

(ii) $c_k = j_k!$;

(iii) $\beta(b_k, c_k, i) = f(i)$ *for* $0 \leq i \leq k$.

PROOF. This is a standard result ([**Me64**], p.131, Proposition 3.22). □

Now we have the standard definition ([**Me64**], p.118):

DEFINITION 19.2. A number-theoretic function $f(x_1, \ldots, x_n)$ is said to be representable in the first order Peano Arithmetic PA if, and only if, there is a PA formula $[F(x_1, \ldots, x_{n+1})]$ with the free variables $[x_1, \ldots, x_{n+1}]$, such that, for any given natural numbers $k_1, \ldots, k_{n+1}$:

(i) if $f(k_1, \ldots, k_n) = k_{n+1}$ then PA proves: $[F(k_1, \ldots, k_n, k_{n+1})]$;

(ii) PA proves: $[(\exists_1 x_{n+1})F(k_1, \ldots, k_n, x_{n+1})]$.

The function $f(x_1, \ldots, x_n)$ is said to be strongly representable in PA if we further have that:

(iii) PA proves: $[(\exists_1 x_{n+1})F(x_1, \ldots, x_n, x_{n+1})]$. □

We also have that:

LEMMA 19.3. *$\beta(x_1, x_2, x_3)$ is strongly represented in PA by $[Bt(x_1, x_2, x_3, x_4)]$, which is defined as follows:*

$$[(\exists w)(x_1 = ((1 + (x_3 + 1) \star x_2) \star w + x_4) \wedge (x_4 < 1 + (x_3 + 1) \star x_2))].$$

PROOF. This is a standard result ([**Me64**], p.131, proposition 3.21). □

19.3. Why $\aleph_0 \longleftrightarrow 2^{\aleph_0}$ in constructive mathematics

The following argument now reveals the sense in which we can assert $\aleph_0 \longleftrightarrow 2^{\aleph_0}$ in constructive mathematics:

THEOREM 19.4. *The cardinality $2^{\aleph_0}$ of the real numbers cannot exceed the cardinality $\aleph_0$ of the integers.*

PROOF. Let $\{r(n)\}$ be the denumerable sequence defined by the denumerable sequence of digits in the decimal expansion $\sum_{n=1}^{\infty} r(n).10^{-n}$ of a putatively given real number $\mathbb{R}$ in the interval $0 < \mathbb{R} \leq 1$.

By lemma 19.1, for any given natural number k, we can define natural numbers b_k, c_k such that, for any $1 \leq n \leq k$:

$$\beta(b_k, c_k, n) = r(n).$$

By lemma 19.3, $\beta(b_k, c_k, n)$ is uniquely represented in the first order Peano Arithmetic PA by $[Bt(b_k, c_k, n, x)]$ such that, for any $1 \leq n \leq k$:

If $\beta(b_k, c_k, n) = r(n)$ then PA proves $[Bt(b_k, c_k, n, r(n))]$.

We now define the arithmetical formula $[R(b_k, c_k, n)]$ for any $1 \leq n \leq k$ by:

$[R(b_k, c_k, n) = r(n)]$ if, and only if, PA proves $[Bt(b_k, c_k, n, r(n))]$.

Hence every putatively given real number $\mathbb{R}$ in the interval $0 < \mathbb{R} \leq 1$ can be uniquely corresponded to an algorithmically *verifiable* arithmetical formula $[R(x)]$ since:

> For any k, the primitive recursivity of $\beta(b_k, c_k, n)$ yields an algorithm $AL_{(\beta,\mathbb{R},k)}$ that provides evidence for deciding the unique value of each formula in the finite sequence $\{[R(1), R(2), \ldots, R(k)]\}$ by evidencing the truth under a constructively well-defined interpretation of PA for:
>
> $[R(1) = R(b_k, c_k, 1)]$
> $[R(b_k, c_k, 1) = r(1)]$
>
> $[R(2) = R(b_k, c_k, 2)]$
> $[R(b_k, c_k, 2) = r(2)]$
>
> $\ldots$
>
> $[R(k) = R(b_k, c_k, k)]$
> $[R(b_k, c_k, k) = r(k)]$.

The correspondence is unique because, if $\mathbb{R}$ and $\mathbb{S}$ are two different putatively given reals in the interval $0 < \mathbb{R},\ \mathbb{S} \leq 1$, then there is always some m for which:

$$r(m) \neq s(m).$$

Hence we can always find corresponding arithmetical functions $[R(n)]$ and $[S(n)]$ such that:

$[R(n) = r(n)]$ for all $1 \leq n \leq m$.

$[S(n) = s(n)]$ for all $1 \leq n \leq m$.

$[R(m) \neq S(m)]$.

Since PA is first order, the cardinality of the reals cannot, therefore, exceed that of the integers.

The theorem follows.[3] □

COROLLARY 19.5. $\aleph_0 \longleftrightarrow 2^{\aleph_0}$ □

We conclude further that, since Theorem 9.10 establishes that PA is finitarily provable as consistent:

COROLLARY 19.6. *CH follows from the axioms of PA.* □

19.4. Cantor's diagonal argument in constructive mathematics

We note that—as entailed by Cantor's diagonal argument—there is no algorithmically computable function $F(n)$ that can be defined to yield *all* algorithmically computable real numbers.

We *cannot*, however, conclude from this that that there are *unspecifiable* real numbers, since:

THEOREM 19.7. *Every real number is specifiable in PA.*

PROOF. Since every real number is the putative limit of a Cauchy sequence, it is *specifiable* in PA because it can be represented by an algorithmically *verifiable* arithmetical function which, by Lemma 19.3, is representable in PA. □

We note that the classical conclusion $\aleph_0 \not\longleftrightarrow 2^{\aleph_0}$ reflects the Platonic assumption that there are 'set-theoretically completed' Cauchy sequences which cannot be expressed in PA.[4]

Theorem 19.4 shows that such an assumption is invalid, and that Cauchy sequences which are defined as algorithmically *verifiable*, but not algorithmically *computable*, correspond to 'essentially incompletable' real numbers whose Cauchy sequences cannot, in a sense, be known 'completely' even to Laplace's 'intellect' (see §29.2).

In other words, the numerical values of algorithmically *verifiable*, but not algorithmically *computable*, sequences must be treated as formally specifiable, first-order, non-terminating processes which are 'eternal work-in-progress' in the sense of Theorem 19.4 (a perspective suggested by the way dimensionless constants are viewed in the physical sciences, as highlighted in §29.6 and §29.7).

Thus, from an *evidence-based* perspective (see Chapter 5), Theorem 19.4 implies that real numbers do not exist in some Platonic universe of points that constitute a line, but are mathematically constructed by number-theoretic definitions that are algorithmically *verifiable*, but not necessarily algorithmically *computable*.

---

[3]We note—but do not consider further as it is not germane to the intent of this investigation—that Theorem 19.4 offers an arithmetical resolution of Hilbert's First Problem ([**Hi00**]), which asks whether there is a set whose cardinality is strictly between the cardinality $\aleph_0$ of the integers and the cardinality $2^{\aleph_0}$ of the real numbers.

[4]Such a conclusion can also be viewed as another instance (see, for instance, §22.4) of ignoring Skolem's cautionary remarks in [**Sk22**] about unrestrictedly corresponding putative mathematical entities across domains of different axiom systems.

They assume significance (which can, debatably, be termed as 'existence') mathematically only when such a definition is made explicit formally in an argumentation (compare with Brouwer's parallel perspective cited in §5.3).

CHAPTER 20

Why BPCM need not admit non-standard arithmetical structures

The significance of ω-consistency for constructive mathematics lies in BPCM's tacit acceptance that Gödel's proof of the existence of formally undecidable arithmetical propositions compels constructive mathematics to accommodate the gamut of putative non-standard arithmetical structures that are entailed by the assumption that PA is ω-consistent.

If so, the challenge faced by BPCM with respect to PA ought, then, to be the assignment of *evidence-based* truth values to the constructively interpretable PA propositions in such structures.

However we note that:

- any such assignment cannot yield a non-standard model of PA which contains numbers other than the natural numbers (Corollary 11.2);

and that:

- PA is ω-*in*consistent (Corollary 11.6).

We now show *why* conventional arguments for perforce admission of a gamut of such non-standard structures under any interpretations of PA collapse, partially because assuming ω-consistency implies Aristotle's particularisation.

20.1. The case against non-standard models of PA

Once we accept as logically sound the set-theoretically based meta-argument (by which we mean arguments such as in [**Ka91**], where the meta-theory is taken to be a set-theory such as ZF or ZFC, and the logical consistency of the meta-theory is not considered relevant to the argumentation) that a first-order Peano Arithmetic PA (e.g., the theory S defined in [**Me64**], pp.102-103) can be forced—by an ante-computationalist interpretation of the Compactness Theorem—into admitting non-standard models which contain an 'infinite' integer, then the set-theoretical properties of the algebraic and arithmetical structures of such putative models should perhaps follow without serious foundational reservation (as argued, for instance, in [**Ka91**]; [**Bov00**]; [**BBJ03**], ch.25, p.302; [**KS06**]; [**Ka11**]).

> **Compactness Theorem** ([**BBJ03**]. p.147): If every finite subset of a set of sentences has a model, then the whole set has a model.

Now, we note that even from a post-computationalist, *evidence-based*, arithmetical perspective (as introduced in [**An12**]; see also [**An16**]) anchored *strictly* *within* the framework of classical logic[1], we *can* conclude incontrovertibly by the Compactness Theorem that:

LEMMA 20.1. *If the collection $Th(\mathbb{N})$ of all true $\mathcal{L}_A$-sentences is the $\mathcal{L}_A$-theory of the standard model of Arithmetic ([**Ka91**], p.10-11), then we may consistently add to it the following as an additional—not necessarily independent—axiom:*

$$(\exists y)(y > x). \qquad \square$$

However, we shall argue that even though $(\exists y)(y > x)$ is algorithmically computable (Definition 5.3 above) as always true in the standard model of Arithmetic considered above—whence all of its instances are in $Th(\mathbb{N})$—we *cannot* conclude by the Compactness Theorem that (as argued, for instance, in [**Ka91**], p.10-11):

$$(*)\ \cup_{k\in\mathbb{N}}\{Th(\mathbb{N}) \cup \{c > \underline{n} \mid n < k\}\}$$

is consistent and has a model M_c which contains an 'infinite' integer.

Reason: We shall argue that the condition '$k \in \mathbb{N}$' in (*) above requires, first of all, that we must be able to extend $Th(\mathbb{N})$ by the addition of a 'relativised' axiom (cf. [**Fe92**]; [**Me64**], p.192), such as:

$$(\exists y)((x \in \mathbb{N}) \rightarrow (y > x)).$$

Only then may we conclude that if a model M_c of:

$$\{Th(\mathbb{N}) \cup (\exists y)((x \in \mathbb{N}) \rightarrow (y > x))\}$$

exists, then it must have an 'infinite' integer c such that:

$$M_c \models c > \underline{n}$$

for all $n \in \mathbb{N}$.

However, we shall then argue that even this would not yield a model for $Th(\mathbb{N})$, since every model of $Th(\mathbb{N})$ is by definition a model of (the provable formulas of) PA, and we shall show (Theorem 20.2) that we cannot introduce a 'completed' infinity such as c into either PA or any model of PA!

20.2. A post-computationalist doctrine

More generally, we shall argue that, if our interest is in the arithmetical properties of models of PA, then we first need to make explicit any appeal to non-constructive considerations such as Aristotle's particularisation.

We shall then argue that, even from a classical perspective, there are serious foundational, post-computationalist, reservations to accepting that a consistent PA can be forced by the Compactness Theorem into admitting non-standard models which contain elements other than the natural numbers.

Reason: Any arithmetical application of the Compactness Theorem to PA can neither ignore currently accepted post-computationalist doctrines of objectivity—nor contradict the *evidence-based* assignments of satisfaction and truth to the

[1]*Classical logic*: By 'classical logic' we mean the standard first-order predicate calculus FOL where the Law of the Excluded Middle is a theorem, but we do not assume that FOL is ω-consistent; i.e., we do not assume that Aristotle's particularisation (Definition 3.1) must hold under any interpretation of the logic.

atomic formulas of PA (therefore to the compound formulas under Tarski's inductive definitions) in terms of either algorithmical *verifiability* or algorithmic *computability* (Definitions Definition 5.2 and Definition 5.3)—as expressed, for instance, by the post-computationalist doctrine (cf. [**Mu91**]) that one can view the *values* of a simple functional language as specifying *evidence* for propositions in a constructive logic.

The significance of this doctrine is that it helps highlight how the algorithmically *verifiable* (Definition 5.2) formulas of PA define the classical non-finitary[2] standard interpretation $\boldsymbol{M}$ of PA over $\mathbb{N}$ (as defined in §A, Appendix A), to which standard arguments for the existence of non-standard models of PA critically appeal.

Accordingly, we shall show that standard arguments (eg., [**BBJ03**], p.155, Lemma 13.3, Model existence lemma) which appeal to the ante-computationalist interpretation of the Compactness Theorem—for forcing non-standard models of PA which contain an 'infinite' integer—cannot admit constructive assignments of satisfaction and truth (in terms of algorithmical verifiability) to the atomic formulas of their putative extension of PA[3].

We shall conclude that such arguments therefore questionably postulate by axiomatic fiat that which they seek to 'prove'!

20.3. Standard arguments for non-standard models of PA

As examples, we shall consider here the following three standard arguments for the existence of non-standard models of the first-order Peano Arithmetic PA:

(i) If PA is consistent, then we obtain a non-standard model for PA which contains an 'infinite' integer by applying the Compactness Theorem to the union of the set of formulas that are satisfied or true in the classical 'standard' model of PA (§A, Appendix A) and the countable set of all PA-formulas of the form $[c_n = S(c_{n+1})]$.

(ii) If PA is consistent, then we obtain a non-standard model for PA which contains an 'infinite' integer by adding a constant c to the language of PA and applying the Compactness Theorem to the theory $\mathbf{P} \cup \{c > \underline{n} : \underline{n} = \underline{0}, \underline{1}, \underline{2}, \ldots\}$.

(iii) If PA is consistent, then we obtain a non-standard model for PA which contains an 'infinite' integer by adding the PA formula $[\neg(\forall x)R(x)]$ as an axiom to PA, where $[(\forall x)R(x)]$ is a Gödelian formula that is unprovable in PA, even though $[R(n)]$ is provable in PA for any given PA numeral $[n]$ ([**Go31**], p.25(1)[4]).

---

[2] *Comment*: 'Non-finitary' because even though the Axiom Schema of Finite Induction interprets as true under the standard interpretation $\boldsymbol{M}$ of PA over $\mathbb{N}$ with respect to 'truth' as defined by the algorithmically *verifiable* formulas of PA (Lemma 7.3), the compound formulas of PA are not decidable finitarily under the standard interpretation $\boldsymbol{M}$ of PA over $\mathbb{N}$ with respect to algorithmically verifiable 'satisfaction' and 'truth'.

[3] For instance, the standard set-theoretical assignment-by-postulation **(S5)** of the satisfaction properties **(S1)** to **(S8)** in [**BBJ03**], p.153, Lemma 13.1 (Satisfaction properties lemma), appeals non-constructively to Aristotle's particularisation.

[4] In his seminal 1931 paper [**Go31**], Gödel defines, and refers to, the formula corresponding to $[R(x)]$ only by its 'Gödel' number r (op. cit., p.25, Eqn.(12)), and to the formula corresponding to $[(\forall x)R(x)]$ only by its 'Gödel' number 17 *Gen* r.

We shall first argue that (i) and (ii)—which appeal to Thoraf Skolem's ante-computationalist reasoning (in [**Sk34**]) for the existence of a non-standard model of PA—should be treated as foundationally fragile from a finitary, post-computationalist perspective within classical logic.

We shall then argue that although (iii)—which appeals to Gödel's (also ante-computationalist) reasoning (in [**Go31**]) for the existence of a non-standard model of PA—does yield a model other than the classical 'standard' model of PA, we cannot conclude by even classical (albeit post-computationalist) reasoning that the domain is other than the domain $\mathbb{N}$ of the natural numbers unless we invalidly (see Corollary 11.6) assume that a consistent PA is necessarily ω-consistent.

20.4. The significance of Aristotle's particularisation for the first-order predicate calculus

We recall that in a formal language the formula '$[(\exists x)P(x)]$' is an abbreviation for the formula '$[\neg(\forall x)\neg P(x)]$'; and that the commonly accepted interpretation of this formula tacitly appeals to Aristotlean particularisation.

Further, as Brouwer had noted in his seminal 1908 paper on the unreliability of logical principles ([**Br08**]), the commonly accepted interpretation of this formula is ambiguous if interpretation is intended over an infinite domain.

Brouwer essentially argued that:

— even supposing the formula '$[P(x)]$' of a formal Arithmetical language interprets as an arithmetical relation denoted by '$P^*(x)$',

— and the formula '$[\neg(\forall x)\neg P(x)]$' as the arithmetical proposition denoted by '$\neg(\forall x)\neg P^*(x)$',

— the formula '$[(\exists x)P(x)]$' need not interpret as the arithmetical proposition denoted by the usual abbreviation '$(\exists x)P^*(x)$';

— and that such postulation is invalid as a general logical principle in the absence of a means for constructing some putative object a for which the proposition $P^*(a)$ holds in the domain of the interpretation.

Hence we shall follow the convention that the assumption that '$(\exists x)P^*(x)$' is the intended interpretation of the formula '$[(\exists x)P(x)]$'—which is essentially the assumption that Aristotle's particularisation holds over the domain of the interpretation—must always be explicit.

20.5. The significance of Aristotle's particularisation for PA

We also note that, in order to avoid intuitionistic objections to his reasoning, Gödel introduced the syntactic property of ω-consistency (see §15.1) as an explicit assumption in his formal reasoning in his seminal 1931 paper on formally undecidable arithmetical propositions ([**Go31**], p.23 and p.28).

Gödel explained at some length (in his introduction on p.9 of [**Go31**]) that his reasons for introducing ω-consistency explicitly was to avoid appealing to the semantic concept of classical arithmetical truth in classical predicate logic (which presumes Aristotle's particularisation).

We further note that, if PA is consistent, then PA is ω-consistent *if* Aristotle's particularisation holds under the standard interpretation $\boldsymbol{M}$ of PA (Lemma 15.8).

20.6. The ambiguity in admitting an 'infinite' constant

We begin our consideration of standard arguments for the existence of non-standard models of PA which contain an 'infinite' integer by first highlighting and eliminating an ambiguity in the argument as it is usually found in standard texts (e.g., [**HP98**], p.13, §0.29; [**Me64**], p.112, Ex. 2), such as, for instance, the argument:

> "**Corollary**. There is a non-standard model of **P** with domain the natural numbers in which the denotation of every nonlogical symbol is an arithmetical relation or function.
>
> *Proof.* As in the proof of the existence of nonstandard models of arithmetic, add a constant ∞ to the language of arithmetic and apply the Compactness Theorem to the theory
>
> $\mathbf{P} \cup \{\infty \neq \mathbf{n}\colon\ n \ = \ 0,\ 1,\ 2,\ \ldots\}$
>
> to conclude that it has a model (necessarily infinite, since all models of **P** are). The denotations of ∞ in any such model will be a non-standard element, guaranteeing that the model is non-standard. Then apply the arithmetical Löwenheim-Skolem theorem to conclude that the model may be taken to have domain the natural numbers, and the denotations of all nonlogical symbols arithmetical."
>
> ... [**BBJ03**], p.306, Corollary 25.3.

20.7. We cannot force PA to admit a transfinite ordinal

The ambiguity lies in a possible interpretation of the symbol ∞ as a 'completed' infinity (such as Cantor's first limit ordinal ω) in the context of non-standard models of PA. To eliminate this possibility we establish trivially that, and briefly examine why:

THEOREM 20.2. *No model of PA can admit a transfinite ordinal such as Cantor's first limit ordinal ω.*

PROOF. Let $[G(x)]$ denote the PA-formula:

$$[x = 0 \vee \neg(\forall y)\neg(x = Sy)]$$

We note that $[G(x)]$ entails under any *evidence-based* interpretation of PA that:

> If x denotes an element in the domain D of a model of PA, then either x is 0, or there is no algorithm which will evidence that, for any given k in D, x is not a 'successor' of k.

Further, in every model of PA, if $G(x)$ denotes the interpretation of $[G(x)]$:

(a) $G(0)$ is true;

(b) If $G(x)$ is true, then $G(Sx)$ is true.

Hence, by Gödel's completeness theorem[5]:

(c) PA proves $[G(0)]$;

(d) PA proves $[G(x) \rightarrow G(Sx)]$.

Further, by Generalisation[6]:

(e) PA proves $[(\forall x)(G(x) \rightarrow G(Sx))]$;

Hence, by Induction[7]:

(f) $[(\forall x)G(x)]$ is provable in PA.

Since $[G(x)]$ is provable in PA, it also entails under the *weak* standard interpretation $\boldsymbol{M}$ of PA that:

> If x denotes an element in the domain D of a model of PA, then either x is 0, or it is not the case that, for any given k in D, there is an algorithm which will evidence that x is not a 'successor' of k (i.e., it follows from the PA axioms that x is either 0 or a 'successor' of some k in D).

In other words, except 0, every element in the domain of any model of PA is a 'successor'. Further, the standard PA axioms ensure that x can only be a 'successor' of a unique element in any model of PA.

Since Cantor's first limit ordinal ω is not the 'successor' of any ordinal in the sense required by the PA axioms, and since there are no infinitely descending sequences of ordinals (cf. [**Me64**], p.261) in a model—if any—of a first order set theory such as ZF, the theorem follows. $\square$

20.8. Why we cannot force PA to admit a transfinite ordinal

Theorem 20.2 reflects the fact that we can define the usual order relation '<' in PA so that every instance of the PA Axiom Schema of Finite Induction, such as, say:

(i) $[F(0) \rightarrow ((\forall x)(F(x) \rightarrow F(Sx)) \rightarrow (\forall x)F(x))]$

yields the weaker PA theorem:

(ii) $[F(0) \rightarrow ((\forall x)((\forall y)(y < x \rightarrow F(y)) \rightarrow F(x)) \rightarrow (\forall x)F(x))]$

Now, if we interpret PA without relativisation in ZF (in the sense indicated by Feferman [**Fe92**])— i.e., numerals as finite ordinals, $[Sx]$ as $[x \cup \{x\}]$, etc.— then (ii) always translates in ZF as a theorem:

(iii) $[F(0) \rightarrow ((\forall x)((\forall y)(y \in x \rightarrow F(y)) \rightarrow F(x)) \rightarrow (\forall x)F(x))]$

However, (i) does not always translate similarly as a ZF-theorem, since the following is not necessarily provable in ZF:

[5] *Gödel's Completeness Theorem*: In any first-order predicate calculus, the theorems are precisely the logically valid well-formed formulas (*i. e. those that are true in every model of the calculus*).

[6] *Generalisation in PA*: $[(\forall x)A]$ follows from $[A]$.

[7] *Induction Axiom Schema of PA*: For any formula $[F(x)]$ of PA:

$$[F(0) \rightarrow ((\forall x)(F(x) \rightarrow F(Sx)) \rightarrow (\forall x)F(x))]$$

(iv) $[F(0) \rightarrow ((\forall x)(F(x) \rightarrow F(x \cup \{x\})) \rightarrow (\forall x)F(x))]$

Example: Define $[F(x)]$ as '$[x \in \omega]$'.

We conclude that, whereas the language of ZF admits as a constant the first limit ordinal ω, which would interpret in any putative model of ZF as the ('completed' infinite) set ω of all finite ordinals:

Corollary 20.3. *The language of PA admits of no constant that interprets in any model of PA as the set* $\mathbb{N}$ *of all natural numbers.* □

We note that it is the non-logical Axiom Schema of Finite Induction of PA which does not allow us to introduce—contrary to what is suggested by standard texts (e.g., [**HP98**], p.13, §0.29; [**Ka91**], p.11 & p.12, fig.1; [**BBJ03**]. p.306, Corollary 25.3; [**Me64**], p.112, Ex. 2)—an 'actual' (*or 'completed'*) infinity disguised as an arbitrary constant (usually denoted by c or ∞) into either the language, or a putative model, of PA[8].

20.9. Forcing PA to admit denumerable descending dense sequences

The significance of Theorem 20.2 is seen in the next two arguments ([**Ln08**], and [**Ka91**], pp.10-11, p.74 & p.75, Theorem 6.4), which attempt to implicitly bypass the Theorem's constraint by appeal to the Compactness Theorem for forcing a non-standard model with denumerable descending dense sequences onto PA.

However, we argue in both cases that applying the Compactness Theorem constructively—even from a classical perspective—does not logically yield a non-standard model for PA with an 'infinite' integer as claimed (and as suggested also by standard texts in such cases; eg. [**BBJ03**]. p.306, Corollary 25.3; [**Me64**], p.112, Ex. 2).

20.10. An argument for a non-standard model of PA

The first is the argument ([**Ln08**]) that we can define a non-standard model of PA with an infinite descending chain of successors, where the only non-successor is the null element 0:

1. Let $<\mathbb{N}$ (*the set of natural numbers*); $=$ (*equality*); S (*the successor function*); $+$ (*the addition function*); $*$ (*the product function*); 0 (*the null element*)$>$ be the structure that serves to define a model of PA, say N;
2. Let $\mathrm{T}[N]$ be the set of PA-formulas that are satisfied or true in N;
3. The PA-provable formulas form a subset of $\mathrm{T}[N]$;
4. Let Γ be the countable set of all PA-formulas of the form $[c_n = Sc_{n+1}]$, where the index n is a natural number;
5. Let T be the union of Γ and $\mathrm{T}[N]$;
6. $\mathrm{T}[N]$ plus any finite set of members of Γ has a model, e.g., N itself, since N is a model of any finite descending chain of successors;
7. Consequently, by Compactness, T has a model; call it M;

[8] A possible reason why the Axiom Schema of Finite Induction does not admit non-finitary reasoning into either PA, or into any model of PA, is suggested in §20.15

8. M has an infinite descending sequence with respect to S because it is a model of Γ;
9. Since PA is a subset of T, M is a non-standard model of PA.

20.11. Why the above argument is logically fragile

However if—as claimed above—N is a model of T$[N]$ plus any finite set of members of Γ, and the PA term $[c_n]$ is constructively well-defined for any given natural number n then, necessarily:

(a) All PA-formulas of the form $[c_n = Sc_{n+1}]$ are PA-provable,

(b) Γ is a proper sub-set of the PA-provable formulas, and

(c) T is identically T$[N]$.

Reason: The argument cannot be that some PA-formula of the form $[c_n = Sc_{n+1}]$ is true in N, but not PA-provable, as this would imply that if PA is consistent then PA+$[\neg(c_n = Sc_{n+1})]$ has a model other than N; in other words, it would presume that which is sought to be proved, namely that PA has a non-standard model[9]!

Consequently, the postulated model M of T in (7) above by 'Compactness' is the model N that defines T$[N]$. However, N has no infinite descending sequence with respect to S, even though it is a model of Γ.

Hence the argument does not establish the existence of a non-standard model of PA with an infinite descending sequence with respect to the successor function S.

20.12. Kaye's argument for a non-standard model of PA

The second is Richard Kaye's more formal argument ([**Ka91**], pp.10-11; attributed by Kaye as essentially Skolem's argument in [**Sk34**]):

> "Let $Th(\mathbb{N})$ denote the complete $\mathcal{L}_A$-theory of the standard model, i.e. $Th(\mathbb{N})$ is the collection of all true $\mathcal{L}_A$-sentences. For each $n \in \mathbb{N}$ we let $\underline{n}$ be the closed term $(\ldots(((1+1)+1)+\ldots+1)))(n\ 1s)$ of $\mathcal{L}_A$; $\underline{0}$ is just the constant symbol 0. We now expand our language $\mathcal{L}_A$ by adding to it a new constant symbol c, obtaining the new language $\mathcal{L}_c$, and consider the following $\mathcal{L}_c$-theory with axioms
>
> ρ (for each $\rho \in Th(\mathbb{N})$)
>
> and
>
> $c > \underline{n}$ (for each $n \in \mathbb{N}$)
>
> This theory is consistent, for each finite fragment of it is contained in

[9]To place this distinction in perspective, Adrien-Marie Legendre and Carl Friedrich Gauss independently conjectured in 1796 that, if $\pi(x)$ denotes the number of primes less than x, then $\pi(x)$ is asymptotically equivalent to $x/\text{In}(x)$. Between 1848/1850, Pafnuty Lvovich Chebyshev confirmed that if $\pi(x)/\{x/\text{In}(x)\}$ has a limit, then it must be 1. However, the crucial question of whether $\pi(x)/\{x/\text{In}(x)\}$ has a limit at all was answered in the affirmative using analytic methods independently by Jacques Hadamard and Charles Jean de la Vallée Poussin only in 1896, and using only elementary methods by Atle Selberg and Paul Erdös in 1949.

$$T_k = Th(\mathbb{N}) \cup \{c > \underline{n} \mid n < k\}$$

for some $k \in \mathbb{N}$, and clearly the $\mathcal{L}_c$-structure $(\mathbb{N},\ k)$ with domain $\mathbb{N}$, 0, 1, $+$, $\cdot$ and $<$ interpreted naturally, and c interpreted by the integer k, satisfies T_k. Thus by the compactness theorem $\cup_{k\in\mathbb{N}}T_k$ is consistent and has a model M_c. The first thing to note about M_c is that

$$M_c \models c > \underline{n}$$

for all $n \in \mathbb{N}$, and hence it contains an 'infinite' integer."

20.13. Why the preceding argument too is logically fragile

We note again that, from an arithmetical perspective, any application of the Compactness Theorem to PA cannot ignore currently accepted post-computationalist doctrine of objectivity ([**Mu91**]) that one can view the *values* of a simple functional language as specifying *evidence* for propositions in a constructive logic, and contradict the constructive assignment of satisfaction and truth to the atomic formulas of PA (therefore to the compound formulas under Tarski's inductive definitions) in terms of either algorithmical verifiability or algorithmic computability (Definitions 5.2 and 5.3).

Accordingly, from an arithmetical perspective we can only conclude by the Compactness Theorem that if $Th(\mathbb{N})$ is the $\mathcal{L}_A$-theory of the standard model (interpretation), then we may consistently add to it the following as an additional—not necessarily independent—axiom:

$$(\exists y)(y > x).$$

Moreover, even though $(\exists y)(y > x)$ is algorithmically computable as always true in the standard model—whence all instances of it are also therefore in $Th(\mathbb{N})$—we have that:

LEMMA 20.4. *If $Th(\mathbb{N})$ denotes the complete $\mathcal{L}_A$-theory of the standard model $\boldsymbol{M}$ of PA, and $T_k = Th(\mathbb{N}) \cup \{c > \underline{n} \mid n < k\}$, we cannot conclude by the Compactness Theorem that $\cup_{k\in\mathbb{N}}T_k$ is consistent and has a model M_c which contains an 'infinite' integer.*

PROOF. The condition '$k \in \mathbb{N}$' in $\cup_{k\in\mathbb{N}}T_k$ requires, first of all, that we must be able to extend $Th(\mathbb{N})$ by the addition of a 'relativised' axiom (cf. [**Fe92**]; [**Me64**], p.192) such as:

$$(\exists y)((x \in \mathbb{N}) \rightarrow (y > x))$$

from which we may conclude the existence of some c, and a model M_c of PA such that:

$$M_c \models c > \underline{n}$$

for all $n \in \mathbb{N}$

However, since every model of $Th(\mathbb{N})$ is by definition a model of (the provable formulas of) PA and, by Theorem 20.2, we cannot introduce a 'completed' infinity such as c into into either PA or any model of PA, the Compactness Theorem cannot yield a model for $Th(\mathbb{N})$ that contains an 'infinite' integer without inviting contradiction. $\Box$

We note that the argument in [**Ka91**], pp.10-11, seeks to violate finitarity by adding a new constant c to the language $\mathcal{L}_A$ of PA that is not definable in $\mathcal{L}_A$ and, ipso facto, adding an atomic formula $[c = x]$ to PA whose satisfaction under any interpretation of PA is not algorithmically *verifiable*!

Since the atomic formulas of PA are algorithmically *verifiable* under the standard interpretation $\boldsymbol{M}$ (Theorem 7.1), the above conclusion too postulates that which it seeks to prove!

Moreover, the postulation would be false if $Th(\mathbb{N})$ were categorical.

Since $Th(\mathbb{N})$ must have a non-standard model if it is not categorical, we consider next whether we may conclude from Gödel's incompleteness argument (in [**Go31**]) that any such model can have an 'infinite' integer.

20.14. Gödel's argument for a non-standard model of PA

We consider the Gödelian formula $[(\forall x)R(x)]$ which is unprovable in PA if PA is consistent, even though the formula $[R(n)]$ is provable in a consistent PA for any given PA numeral $[n]$.

Now, it follows from Gödel's reasoning ([**Go31**], p.25(1) & p.25(2)) that:

THEOREM 20.5. *If PA is consistent, then we may add the PA formula* $[\neg(\forall x)R(x)]$ *as an axiom to PA without inviting inconsistency.* $\Box$

THEOREM 20.6. *If PA is ω-consistent, then we may add the PA formula* $[(\forall x)R(x)]$ *as an axiom to PA without inviting inconsistency.* $\Box$

It follows from this that:

COROLLARY 20.7. *If PA is ω-consistent, then there are at least two distinctly different models of PA.* $\Box$

If we assume that a consistent PA is necessarily ω-consistent, then it follows that one of the two putative models postulated by Corollary 20.7 must contain elements other than the natural numbers.

We conclude that Gödel's justification for the assumption—that non-standard models of PA containing elements other than the natural numbers are logically feasible—lies in his non-constructive, and logically fragile, assumption that a consistent PA is necessarily ω-consistent.

20.15. Why Gödel's assumption is logically fragile

Now, whereas Gödel's proof of Corollary 20.7 appeals to the non-constructive Aristotle's particularisation, a constructive proof of the Corollary follows trivially from the *evidence-based* interpretations of PA considered in §6

We detail there how Tarski's inductive definitions allow us to provide *finitary* satisfaction and truth certificates to all atomic (and ipso facto to all compound) formulas of PA over the domain ℕ of the natural numbers in *two* essentially different ways:

(1) In terms of algorithmic verifiabilty; and

(2) In terms of algorithmic computability.

Moreover, we show that neither the 'algorithmically *verifiable*' model, nor the 'algorithmically *computable*' model, of PA defined by these finitary satisfaction and truth assignments to the atomic formulas of PA contains elements other than the natural numbers.

20.16. Any algorithmically *verifiable* model of PA is over ℕ

For instance if, in the first case, we assume that the algorithmically *verifiable* atomic formulas of PA determine an algorithmically *verifiable* model of PA over the domain ℕ of the PA numerals, then such a model would be isomorphic to the standard model of PA over the domain ℕ of the natural numbers (an immediate consequence of Theorem 7.6).

However, such a model of PA over ℕ would not be constructively well-defined (in the sense of Definition 21.7) since, if the formula $[(\forall x)F(x)]$ were to interpret as true in it, then we could only conclude that, for any numeral $[n]$, there is a deterministic algorithm $AL_{F,n}$ which will finitarily certify the formula $[F(n)]$ as true under an algorithmically *verifiable* interpretation in ℕ.

We could not conclude that there is a single deterministic algorithm AL_F which, for any numeral $[n]$, will finitarily certify the formula $[F(n)]$ as true under the algorithmically *verifiable* interpretation in ℕ.

Consequently, even though the PA Axiom Schema of Finite Induction can be shown to interpret as true under the algorithmically *verifiable* interpretation of PA over the domain ℕ of the PA numerals, the interpretation is not a constructively well-defined model of PA.

We note that if we were to assume that the algorithmically *verifiable* interpretation of PA is a constructively well-defined model of PA (in the sense of Definition 21.7), then it would follow that:

- PA is ω-consistent;
- Aristotle's particularisation holds over ℕ.

20.17. The algorithmically *computable* model of PA is over ℕ

The second case is where the algorithmically *computable* atomic formulas of PA determine an algorithmically *computable* model of PA over the domain ℕ of the natural numbers (§9).

The algorithmically *computable* model of PA is constructively well-defined, since we can show that, if the formula $[(\forall x)F(x)]$ interprets as true under it, then we may always conclude that there is a single deterministic algorithm AL_F which, for any numeral $[n]$, will finitarily certify the formula $[F(n)]$ as true in ℕ under the algorithmically computable interpretation.

Consequently we can show that all the PA axioms—including the Axiom Schema of Finite Induction—interpret finitarily as true in ℕ under the algorithmically *computable* interpretation of PA, and the PA Rules of Inference preserve such truth finitarily (Theorem 9.7).

Thus the algorithmically *computable* interpretation of PA is a constructively well-defined model of PA from which we may conclude that:

- PA is consistent (Theorem 9.10).

20.18. Why Gödel's assumption that PA is ω-consistent cannot be justified

By the way the above finitary interpretation §20.17 is defined under Tarski's inductive definitions (§9), if a PA-formula $[F]$ interprets as true in the corresponding finitary model of PA, then there is a single deterministic algorithm AL_F that provides a certificate for such truth for $[F]$ in $\mathbb{N}$; whilst if $[F]$ interprets as false in the above finitary model of PA, then there is no single deterministic algorithm that can provide such a truth certificate for $[F]$ in $\mathbb{N}$ (an immediate consequence of Theorem 9.7).

Now, if there is no single deterministic algorithm that can provide such a truth certificate for the Gödelian formula $[R(x)]$ in $\mathbb{N}$, then we would have by definition first that the PA formula $[\neg(\forall x)R(x)]$ is true in the model, and second by Gödel's reasoning that the formula $[R(n)]$ is true in the model for any given numeral $[n]$. Hence Aristotle's particularisation would not hold in the model.

However, by definition if PA were ω-consistent then Aristotle's particularisation must necessarily hold in every model of PA.

It follows that, in the absence of a cogent argument for the existence of a single deterministic algorithm AL_R which could provide such a truth certificate for the Gödelian formula $[R(x)]$ in $\mathbb{N}$, we cannot justify Gödel's unqualified assumption that a consistent PA is necessarily ω-consistent.

20.19. The domain of every constructively well-defined interpretation of PA is $\mathbb{N}$

We have argued that standard arguments for the existence of non-standard models of the first-order Peano Arithmetic PA with domains other than the domain $\mathbb{N}$ of the natural numbers should be treated as logically fragile even from within classical logic.

In particular we have argued that even if Gödel's argument for the existence of a non-standard model of PA does yield a model of PA other than the classical non-finitary 'standard' model, we cannot conclude from it that the domain is other than the domain $\mathbb{N}$ of the natural numbers.

Part 5

The significance of *evidence-based* reasoning for some grey areas in the foundations of Classical Logic, Mathematics and Philosophy

CHAPTER 21

The ambiguity in Brouwer-Heyting-Kolmogorov realizability

Now, the reason that constructive mathematics such as BPCM are compelled to admit the gamut of non-constructive set-theoretical, and non-standard arithmetical, structures lies in the following ambiguity that is implicit in the rules—such as those of Brouwer-Heyting-Kolmogorov realizability (compare §4.3)—that seek to constructively assign unique truth values to the quantified propositions of a mathematical language. For instance:

(a) Is '$\forall x \in A.P(x)$ is realized' to be interpreted *constructively* as:

- 'For any a, $P(a)$' is realised if, and only if,
  - $*$ for *any specified* a in A,
  - $*$ there is a program p_a that
  - $*$ maps (a representation of) $a \in A$ to a realizer of $P(a)$?

  Comment: In which case $\exists x \in A.P(x)]$ is realized if, and only if, there is a pair (p, q) such that p represents some $a \in A$ and q realizes $P(a)$.

or:

(b) Is '$\forall x \in A.P(x)$ is realized' to be interpreted *finitarily* as:

- 'For all a, $P(a)$' is realized if, and only if,
  - $*$ there is a program p that ,
  - $*$ for *any specified* a in A,
  - $*$ maps (a representation of) $a \in A$ to a realizer of $P(a)$?

  Comment: In which case $\exists x \in A.P(x)]$ is realized if, and only if, there is no pair (p, q) such that p represents some $a \in A$ and q realizes $\neg P(a)$.

The significance of this distinction is that if $\forall x \in A.P(x)$ is intended to be read as *'For any a, $P(a)$'*, then it must be consistently interpreted in the language of realizability as (cf. Definition 5.2):

Definition 21.1. **Verifiable realizability**[1]:

A number-theoretical relation $P(x)$ is verifiably realized if, and only if, for any specified natural number n, there is a realizer p_n which can provide evidence for deciding the truth/falsity of each proposition in the finite sequence $\{P(1), P(2), \ldots, P(n)\}$. $\square$

[1]We note that 'verifiable realizability' corresponds to the more intuitive language of 'algorithmic verifiability'—see Definition 5.2—preferred in this investigation.

Whereas if $\forall x \in A.P(x)$ is intended to be read as *'For all x, $P(x)$'*, then it must be consistently interpreted in the language of realizability as (cf. Definition 5.3):

DEFINITION 21.2. **Computable realizability**[2]:

A number theoretical relation $P(x)$ is computably realized if, and only if, there is a realizer p that can provide evidence for deciding the truth/falsity of each proposition in the denumerable sequence $\{P(1), P(2), \ldots\}$. □

We note that:

- Computable realizability implies the existence of a single deterministic algorithm that can finitarily decide the truth/falsity of each proposition in a constructively well-defined denumerable sequence of propositions; whereas
- Verifiable realizability does not imply the existence of a single deterministic algorithm that can finitarily decide the truth/falsity of each proposition in a constructively well-defined denumerable sequence of propositions.

Moreover, it follows from argumentation similar to that for Theorem 5.4 that although every computably realizable relation is verifiably realizable, the converse is not true.

21.1. Brouwerian interpretations of $\wedge, \vee, \rightarrow, \exists, \forall$

The significance of the above distinction for constructive mathematics is seen in the following, presumably standard, intuitionistic interpretations of $\wedge, \vee, \rightarrow, \exists, \forall$, as detailed by Bishop in [**Bi18**]:

> "Each formula of Σ represents a constructively meaningful assertion, in that it denotes a constructively meaningful assertion for given values of the free variables, if we interpret $\wedge, \vee, \rightarrow, \exists, \forall$ in the constructive (Brouwerian) sense. Here is a brief summary of Brouwer's interpretations. (The interpretations hold for all fixed values of the free variables.)
>
> (a) $A \wedge B$ asserts A and also asserts B.
>
> (b) $A \vee B$ either asserts A or asserts B, and we have a finite method for deciding which of the two it does assert.
>
> (c) $A \rightarrow B$ asserts that if A is true, then so is B. (To prove $A \rightarrow B$ we must give some method that converts each proof of A into a proof of B.)
>
> (d) $\forall x A(x)$ asserts that $A(f)$ holds for each (constructively) defined functional f of the same type as the variable x, where $A(f)$ is obtained from $A(x)$ by substituting f for all free occurrences of x.
>
> (e) $\exists x A(x)$ asserts that we know an algorithm for constructing a functional f for which $A(f)$ holds."
>
> ... *Bishop:* [**Bi18**], *pp.6-7.*

We note that although Bishop asserts the above interpretations as constructive, they are ambiguous as to the intended meaning of the words 'all' and 'each', since the interpretations do not distinguish between:

[2]We note that 'computable realizability' corresponds to the more intuitive language of 'algorithmic computability'—see Definition 5.3—preferred in this investigation.

(i) whether there is an algorithm which, for 'all' permissible values of the free variables, evidences that the formula Σ denotes a constructively meaningful assertion; or

(ii) whether, for 'any/each' given permissible values of the free variables, there is an algorithm which evidences that the formula Σ denotes a constructively meaningful assertion.

Accordingly, they cannot accommodate an interpretation of Gödel's first-order arithmetical formula $[R(x)]$ (see §11.4), which:

(1) is such that the PA-formula $[R(n)]$ is PA-provable for any substitution of the numeral $[n]$ for the variable $[x]$ in the PA-formula $[R(x)]$, even though the formula $[(\forall x)R(x)]$ is not PA-provable;

(2) interprets as an arithmetical relation, say $R^{*}(x)$, such that, for any given natural number n, there is *always* some algorithm that will evidence the proposition $R^{*}(n)$ as true, but there is *no* algorithm that, for any given natural number n, will evidence $R^{*}(n)$ as a true arithmetical proposition (see Corollary 11.5).

Curiously, although (1) is essentially the first half of Gödel's 'undecidability' argument in [**Go31**][3], the significance of interpretation (2) apparently escaped Gödel's attention; even though what we have termed as an ambiguity—reflecting a failure to constructively define, and distinguish between, the concepts 'for each/any' and 'for all'—in the intuitionistic interpretation of quantification can, reasonably, be seen as something that Gödel too viewed with disquietude as a 'vagueness' in Heyting's formalisation of intuitionistic logic—a vagueness which he, however, seemed to view as an *unsurmountable* barrier[4] towards the furnishing of a constructive intuitionistic proof of consistency for classical arithmetic:

> "Gödel's 1933 lecture is concerned with the question of a constructive consistency proof for classical arithmetic. In considering what should count as constructive mathematics, Gödel there argues against accepting impredicative definitions, and insists on inductive definitions. Gödel discusses the prospects for a consistency proof for classical arithmetic using intuitionistic logic, then best known from Heyting's formalisation 'Die formalen Regeln der intuitionistischen Logik' (Heyting, 19301,b,c), as well as Heyting's Königsberg lecture of 1931, 'Die intuitionistiche Grundlegung der Mathematik', published as Heyting 1931.
>
> [...]
>
> The principles in Heyting's formalisation that have Gödel's special interest are those for 'absurdity', that is, intuitionistic negation. But Gödel goes on to argue that this notion is not constructive in his sense, and hence of no use for a constructive consistency proof of classical arithmetic. The problem he sees is that their intuitionistic explanation involve a reference to the totality of all constructive proofs. The example he gives is
>
> $$p \supset \neg\neg p$$
>
> which, he says, means 'If p has been proved, then the assumption $\neg p$ leads to a contradiction. Gödel says that these axioms are not about constructions on

[3] p.25: "1. 17 *Gen* r is not κ-provable".

[4] Surmountable though, once the source of the ambiguity is identified and removed, as we show in Theorem 9.11.

a substrate of numbers but rather on a substrate of proofs, and therefore the example may be explicated as 'Given *any* proof for a proposition p, you can construct a reductio ad absurdum for the proposition $\neg p$'. He the comments that

> Heyting's axioms concerning absurdity and similar notions [...] violate the principle, which I stated before, that the word 'any' can be applied only to those totalities for which we have a finite procedure for generating all their elements [...] The totality of all possible proofs certainly does not possess this character, and nevertheless the word 'any' is applied to this totality in Heyting's axioms [...] Totalities whose elements cannot be generated by a well-defined procedure are in some sense vague and indefinite as to their borders. And this objection applies particularly to the totality of intuitionistic proofs because of the vagueness of the notion of constructivity. Therefore this foundation of classical arithmetic by means of the notion of absurdity is of doubtful value. (Gödel, 1933b, p.53)

A draft of this passage in Gödel's archive does not quite end with rejection of Heyting's logic. Instead, it reflects:

> Therefore you may be doubtful [sic] as to the correctness of the notion of absurdity and as to the value of a proof for freedom from contradiction by means of this notion. But nevertheless it may be granted that this foundation is at least more satisfactory than the ordinary platonistic interpretation [...]

Either way, the doubt about, or objection to, the notion of absurdity immediately generalises to implication as such.

It is remarkable, given the construction of Gödel's talk, in which the discussion of the intuitionistic logical connectives is preceded by an argument against the use of impredicative definitions for foundational purposes, that the objection Gödel puts forward is not that Heyting's principles for absurdity are impredicative, but that they are vague. Impredicativity of course entails constructive undefinability and in that sense vagueness, and it is possible that Gödel had seen the problem of impredicativity but thought that, in the context of a consistency proof that is looked for because of its epistemic interest, vagueness is the more important thing to note, even if impredicativity is the cause of it."

... van Atten: [**At17**]*, pp.6-7.*

21.2. Defining constructive mathematics and its goal

We consider some philosophical consequences—for constructive mathematics—of removing the above ambiguity in the rules for Brouwer-Heyting-Kolmogorov realizability, which now allows us to formally distinguish between a first-order language (see Appendix A) and:

- a *first-order theory* that seeks—on the basis of *evidence-based* reasoning—to assign the values 'provable/unprovable' to the well-formed formulas of the language under a *proof-theoretic logic*;
- a *first-order theory* that seeks—on the basis of *evidence-based* reasoning—to assign the values 'true/false' to the well-formed formulas of the language under a *model-theoretic logic*.

where:

Definition 21.3. The *proof-theoretic logic* of a first-order theory S is a set of rules consisting of:

- a selected set of well-formed formulas of S labelled as 'axioms/axiom schemas' that are assigned the value 'provable'; and
- a finitary set of rules of inference in S;

that assign *evidence-based* values of 'provable' or 'unprovable' to the well-formed formulas of S by means of the axioms and rules of inference of S.

Definition 21.4. The *model-theoretic logic* of a first-order theory S with a *proof-theoretic logic* is a set of rules that assign *evidence-based* truth values of 'satisfaction', 'truth', and 'falsity' to the well-formed formulas of S under an interpretation $\mathcal{I}$ such that the axioms of S interpret as 'true' under $\mathcal{I}$, and the rules of inference of S preserve such 'truth' under $\mathcal{I}$.

and, somewhat more generally:

Definition 21.5. A finite set λ of rules is a constructively well-defined logic of a formal mathematical language $\mathcal{L}$ if, and only if, λ assigns unique, *evidence-based*, truth-values:

(a) Of provability/unprovability to the formulas of $\mathcal{L}$; and
(b) Of truth/falsity to the sentences of the Theory $T(\mathcal{U})$ which is defined semantically by the λ-interpretation of $\mathcal{L}$ over a given structure $\mathcal{U}$ that may, or may not, be constructively well-defined; such that
(c) The provable formulas interpret as true in $T(\mathcal{U})$. □

We contrast Definition 21.5 with the epistemically grounded perspective of conventional wisdom (such as, for instance, [**Mur06**]) when it fails to distinguish between the multi-dimensional nature of the logic of a formal mathematical language (as defined above), and the one-dimensional nature of the veridicality of its assertions (articulated either informally as in, for example, [**LR00**][5], or implicitly as, for instance, in [**Shr13**]):

> "Logic, the investigation suggests, is grounded in the formal aspect of reality, and the outline proposes an account of this aspect, the way it both constrains and enables logic (gives rise to logical truths and consequences), logic's role in our overall system of knowledge, the relation between logic and mathematics, the normativity of logic, the characteristic traits of logic, and error and revision in logic.
> ...
> It is an interesting fact that, with a small number of exceptions, a systematic philosophical foundation for logic, a foundation for logic rather than for mathematics or language, has rarely been attempted (fn1: One recent exception is Maddy [2007, Part III], which differs from the present attempt in being thoroughly naturalistic. Another psychologically oriented attempt is Hanna [2006]. Due to limitations of space and in accordance with my constructive goal, I will limit comparisons and polemics to a minimum).
> ...
> By a philosophical foundation for logic I mean in this paper a substantive philosophical theory that critically examines and explains the basic features

[5] A more appropriate title for which, from such a perspective, would be *Where the Veridicality of Mathematical Propositions Comes From.*

> of logic, the tasks logic performs in our theoretical and practical life, the veridicality of logic - including the source of the truth and falsehood of both logical and meta-logical claims, ... the grounds on which logical theories should be accepted (rejected, or revised), the ways logical theories are constrained and enabled by the mind and the world, the relations between logic and related theories (e.g., mathematics), the source of the normativity of logic, and so on. The list is in principle open-ended since new interests and concerns may be raised by different persons and communities at present and in the future. In addition, the investigation itself is likely to raise new questions (whether logic is similar to other disciplines in requiring a grounding in reality, what the distinctive characteristics of logical operators are, etc.).
>
> ...
>
> The motivation for engaging in a foundational project of this kind is both general and particular, both intellectual and practical, both theoretical and applicational. Partly, the project is motivated by an interest in providing a foundation for knowledge in general - i.e., a foundation both for human knowledge as a whole and for each branch of knowledge individually (logic being one such branch). Partly, the motivation is specific to logic, and is due to logic's unique features: its extreme "basicness", generality, modal force, normativity, ability to prevent an especially destructive type of error (logical contradiction, inconsistency), ability to expand all types of knowledge (through logical inference), etc. In both cases the interest is both intellectual and practical. Finally, our interest is both theoretical and applicational: we are interested in a systematic theoretical account of the nature, credentials, and scope of logical reasoning, as well as in its applications to specific fields and areas.
>
> ...
>
> If the bulk of our criticisms is correct, the traditional foundationalist strategy for constructing a foundation for logic (and for our system of knowledge in general) should be rejected. It is true that for a long time the foundationalist strategy has been our only foundational strategy, and as a result many of its features have become entangled in our conception of a foundation, but this entanglement can and ought to be unraveled. ... My goal is an epistemic strategy that is both free of the unnecessary encumbrances of the foundationalist strategy and strongly committed to the grounding project. Following Shapiro [1991], I will call such a strategy a *foundation without foundationalism*."
>
> *... Sher:* [**Shr13**], *pp.145-146, 151.*

Definition 21.6. Constructive mathematics is the study of formal mathematical languages that have a constructively well-defined logic. □

For a formal mathematical language $\mathcal{L}$ to, then, precisely express and objectively (i.e., on the basis of *evidence-based* reasoning) communicate effectively characteristics of some structure $\mathcal{U}$ that may, or may not, be constructively well-defined, it must be able to categorically represent some Theory $T(\mathcal{U})$ whose characteristic is that:

Definition 21.7. The Theory $T(\mathcal{U})$ defined semantically by the λ-interpretation of a formal mathematical language $\mathcal{L}$ over the structure $\mathcal{U}$ is a constructively well-defined model of $\mathcal{L}$ if, and only if, λ is a constructively well-defined Logic of $\mathcal{L}$. □

The significance of Definitions 21.3 to 21.6 is illustrated by the following account by Carl J. Posy of the purported ways in which:

> "... adopting intuitionistic logic limits the ways in which a constructivist can carry out a mathematical proof. A standard example is the classical proof that there are irrational r and s such that r^s is a rational number: either $\sqrt{2}^{\sqrt{2}}$ is rational or it is irrational. If it is rational, then take $r = s = \sqrt{2}$. If it is irrational, then take $r = \sqrt{2}^{\sqrt{2}}$ and $s = \sqrt{2}$. In this case $r^s = (\sqrt{2}^{\sqrt{2}})^{\sqrt{2}} = (\sqrt{2})^2 = 2$. The constructivist cannot make that initial assumption that $\sqrt{2}^{\sqrt{2}}$ is either rational or irrational."
>
> *... Posy:* [**Pos13**], *p.109.*

Though—as the author notes—this theorem is in fact constructively recoverable, the question—left unaddressed here by both classical and constructive theories—is not whether a particular formula is rational or irrational, but whether the logic that assigns truth assignments to the formulas of the concerned language is sufficiently well-defined so as to *evidence* the decidability of whether a formula is either rational or irrational.

21.3. Wittgenstein's 'notorious' paragraph about the Gödel Theorem

We note that such an *evidence-based* perspective reflects in essence the views Ludwig Wittgenstein emphasised in his 'notorious paragraph'[6], where he writes that:

> "I imagine someone asking my advice; he says: "I have constructed a proposition (I will use '*P*' to designate it) in Russell's symbolism, and by means of certain definitions and transformations it can be so interpreted that it says: '*P* is not provable in Russell's system.' Must I not say that this proposition on the one hand is true, and on the other hand is unprovable? For suppose it were false; then it is true that it is provable. And that surely cannot be! And if it is proved, then it is proved that it is not provable. Thus it can only be true, but unprovable."
>
> Just as we ask, "Provable' in what system?," so we must also ask, "True' in what system?" "True in Russell's system" means, as was said, proved in Russell's system, and "false in Russell's system" means the opposite has been proved in Russell's system.—Now what does your "suppose it is false" mean? In the Russell sense it means, "suppose the opposite is proved in Russell's system"; if that is your assumption you will now presumably give up the interpretation that it is unprovable. And by "this interpretation" I understand the translation into this English sentence.—If you assume that the proposition is provable in Russell's system, that means it is true in the Russell sense, and the interpretation "P is not provable" again has to be given up. If you assume that the proposition is true in the Russell sense, the same thing follows. Further: if the proposition is supposed to be false in some other than the Russell sense, then it does not contradict this for it to be proved in Russell's system. (What is called "losing" in chess may constitute winning in another game.)"
>
> *... Wittgenstein:* [**Wi78**], *Appendix III 8.*

In their paper "A note on Wittgenstein's 'notorious paragraph' about the Gödel Theorem", Juliet Floyd and Hilary Putnam draw attention to Wittgenstein's remarks, and argue that this paragraph contains a "philosophical claim of great interest" which (cf. §17.5):

[6]In footnote 9 of [**FP00**], Floyd and Putnam note that: "The 'notorious' paragraph RFM I Appendix III 8 was penned on 23 September 1937, when Wittgenstein was in Norway (see the Wittgenstein papers, CD Rom, Oxford University Press and the University of Bergen, 1998, Item 118 (Band XIV), pp. 106ff)".

> "…is simply this: if one assumes (and, a fortiori if one actually finds out) that $\neg P$ is provable in Russell's system one should …give up the "translation" of P by the English sentence "P is not provable".
>
> …*Floyd and Putnam:* [**FP00**].

Now, although Wittgenstein's reservations on Gödel's interpretation of his own formal reasoning are, indeed, of historical importance, the uneasiness that academicians and philosophers such as Floyd and Putnam—and, more recently, Timm Lampert in [**Lam17**]—have continued to sense, express, and debate, over standard (text-book) interpretations of Gödel's formal reasoning—even eighty five years after the publication of the latter's seminal 1931 paper ([**Go31**]) on formally undecidable arithmetical propositions—is of much greater significance, and relevance, to us today.

> "Contrary to Wittgenstein's early critics, Shanker [1988], Floyd & Putnam[2000] and Floyd [2001] argue that Wittgenstein does not question Gödel's undecidability proof itself. Instead, they say, Wittgenstein's remarks are concerned with the semantic and philosophical consequences of Gödel's proof; those remarks represent, according to Floyd and Putnam, a "remarkable insight" regarding Gödel's proof. I share the view that Wittgenstein believed that it is not the task of philosophy to question mathematical proofs. However, I argue that from Wittgenstein's perspective, Gödel's proof is not a mathematical proof. Instead, it is a proof that relies on "prose" in the sense of meta-mathematical interpretations, and thus, it is a valid object of philosophical critique.Thus, I deny that Wittgenstein views Gödel's undecidability proof as being just as conclusive as mathematical impossibility proofs. Wittgenstein's simplied, rather general way of referring to an ordinary language interpretation of G without specifying exactly where questionable meta-mathematical interpretations are relevant to Gödel's proof might have led to the judgment that Wittgenstein's critique is not relevant to Gödel's proof.
>
> Contrary to Floyd and Putnam, Rodych [1999] and Steiner [2001] assume that Wittgenstein argues against Gödel's undecidability proof. According to their interpretation, Wittgenstein's objection against Gödel's proof is that from proving G or $\neg G$, it does not follow that PM is inconsistent or ω-inconsistent. Instead, one could abandon the meta-mathematical interpretation of G. However, according to both authors, this critique is inadequate because Gödel's proof does not rely on a meta-mathematical interpretation of G. By specifying where Wittgenstein's critique is mistaken, they wish to decouple Wittgenstein's philosophical insights from his mistaken analysis of Gödel's mathematical proof. I agree with Rodrych and Steiner that Wittgenstein's critique does not offer a sufficient analysis of the specific manner in which a meta-mathematical interpretation is involved in Gödel's reasoning. However, in contrast to these authors, I will explain why both Gödel's semantic proof and his so-called syntactic proof do rely on a meta-mathematical interpretation.
>
> Priest [2004], Berto [2009a] and Berto [2009b] view Wittgenstein as a pioneer of paraconsistent logic. They are especially interested in Wittgenstein's analysis of Gödel's proof as a proof by contradiction. Like Rodych and Steiner, they maintain that Wittgenstein's remarks are not, in fact, pertinent to Gödel's undecidability proof because Wittgenstein refers not to a syntactic contradiction within PM but rather to a contradiction between the provability of G and its meta-mathematical interpretation. However, according to them, Wittgenstein's critique is not mistaken. Rather, it is concerned with the interpretation and consequences of Gödel's undecidability

> proof. Presuming Wittgenstein's rejection of any distinction between (i) metalanguage and object language and (ii) provability and truth, they show that engaging with Gödel's proof depends on philosophical presumptions. I do not question this. However, I will argue that Wittgenstein's critiqued can be interpreted in a way that is indeed relevant to Gödel's undecidability proof.
>
> The intention of this paper is not to enter into an exegetical debate on whether Wittgenstein understands Gödel's proof and whether he indeed objects to it. For the sake of argument, I assume that to be given. Furthermore, similarly to, e.g., Rodych and Steiner, I take "Wittgenstein's objection" to Gödel's proof to be as follows: "Instead of inferring the incorrectness or (ω-)inconsistency of PM (or PA) from a proof of G (or $\neg G$), one might just as validly abandon the meta-mathematical interpretation of G. Therefore, Gödel's proof is not compelling because it rests on a doubtful meta-mathematical interpretation." I recognize that this is highly controversial, to say the least. However, the literature seems to agree that such an objection, be it Wittgenstein's or not, has no relation to Gödel's undecidability proof and thus is not reasonable. The intention of this paper is to show that this is not true. This objection can, indeed, be related to Gödel's method of defining provability within the language of PM, and it questions this essential element of Gödel's meta-mathematical proof method by measuring its reliability on the basis of an algorithmic conception of proof."
>
> ... *Lampert:* [**Lam17**].

We shall argue further that Wittgenstein's reservations in [**Wi78**], as also the uneasiness expressed by, amongst others, Floyd and Putnam in [**FP00**] and Lampert in [**Lam17**], can—and arguably must, as we advocate in this investigation—be seen as indicating specific points of ambiguity that need to be addressed on both technical *and* philosophical grounds, rather than be dismissed on mere technicalities, since both Wittgenstein and Gödel can be held guilty of conflating 'ω-consistency' with 'correctness'.

That the onus of guilt must fall heavier on Gödel follows not only from his misleading assertion that the semantic concept of 'truth' can be replaced by the 'purely formal and much weaker assumption' of ω-consistency:

> "The method of proof which has just been explained can obviously be applied to every formal system which, first, possesses sufficient means of expression when interpreted according to its meaning to define the concepts (especially the concept "provable formula") occurring in the above argument; and, secondly, in which every provable formula is true. In the precise execution of the above proof, which now follows, we shall have the task (among others) of replacing the second of the assumptions just mentioned by a purely formal and much weaker assumption."
>
> ... *Gödel:* [**Go31**], *p.9.*

but also from his implicit—and equally misleading—footnote 48a on page 28 of [**Go31**], which suggests that assuming any formal system of arithmetic—such as, for instance, the first-order Peano Arithmetic PA—to be ω-consistent is intuitionistically unobjectionable, and may be treated *as a matter of fact*:

> "In the proof of Theorem VI no properties of the system P were used other than the following:

> 1. The class of axioms and the rules of inference (i.e. the relation "immediate consequence") are recursively definable (when the primitive symbols are replaced in some manner by natural numbers).
>
> 2. Every recursive relation is definable within the system P (in the sense of Theorem V).
>
> Hence, in every formal system which satisfies assumptions 1, 2 and is ω-consistent, there exist undecidable propositions of the form $(x)F(x)$, where F is a recursively defined property of natural numbers, and likewise in every extension of such a system by a recursively definable ω-consistent class of axioms. To the systems which satisfy assumptions 1, 2 belong, as one can easily confirm, the Zermelo-Fraenkel and the v. Neumann axiom systems for set theory, and, in addition, the axiom system for number theory which consists of Peano's axioms, recursive definitions (according to schema (2)) and the logical rules. Assumption 1 is fulfilled in general by every system whose rules of inference are the usual ones and whose axioms (as in F) result from substitution in finitely many schemata.[48a]
>
> [Footnote 48a] The true reason for the incompleteness which attaches to all formal systems of mathematics lies, as will be shown in Part II of this paper, in the fact that the formation of higher and higher types can be continued into the transfinite."
>
> *... Gödel:* [**Go31**]*, p.28.*

That both of Gödel's assertions are misleading follows since PA is *both* strongly consistent by Theorem 9.10 in §9.2—hence 'correct'—*and* ω-*in*consistent by Corollary 11.6 in §11.4 and, independently, by Theorem 8.5 in §8.5.

We note, moreover, that the latter proof appears to reflect Lampert's interpretation of Wittgenstein's argument in [**Wi78**]:

> "In I, §17, Wittgenstein suggests to look at proofs of unprovability "in order to see *what* has been proved". To this end, he distinguishes two types of proofs of unprovability. He mentions the first type only briefly: "Perhaps it has here been proved that such-and-such forms of proof do not lead to P." (P is Wittgenstein's abbreviation for Gödel's formula G). In this section, I argue that Wittgenstein refers in this quote to an algorithmic proof proving that G is not provable within PM. Such a proof of unprovability would, to Wittgenstein, be a compelling reason to give up search for a proof of G within PM. Wittgenstein challenges Gödel's proof because it is not an unprovability proof of this type. This is also why Wittgenstein does not consider algorithmic proofs of unprovability in greater detail in his discussion of Gödel's proof. Such proofs represent the background against which he contrasts Gödel's proof to a type of proof that is beyond question.
>
> Unfortunately, Wittgenstein does not follow his own suggestion to more carefully evaluate unprovability proofs with respect to Gödel's proof. Instead, he distinguishes different types of proofs of unprovability in his own words and in a rather general way; cf. I, §8-19. His critique focuses on a proof of unprovability that relies on the representation of provability within the language of the axiom system in question. Thus, following his initial acknowledgement of algorithmic unprovability proofs in I, §17, Wittgenstein repeats, at rather great length, his critique of a meta-mathematical unprovability proof. It is this type of unprovability proof that he judges unable to provide a compelling reason to give up the search for a proof of G. The most crucial aspect of any comparison of two different types of unprovability proofs is the question of what serves as the "criterion of unprovability" (I, §15). According to Wittgenstein, such a criterion should be a purely syntactic criteria independent of any meta-mathematical interpretation of formulas. It

is algorithmic proofs relying on nothing but syntactic criteria that serve as a measure for assessing meta-mathematical interpretations, not vice-versa.

[...]

Gödel's proof is not an algorithmic unprovability proof. Instead, Gödel's proof is based on the representation of provability within the language of PM. Based on this assumption, Gödel concludes that PM would be inconsistent (or ω-inconsistent) if G (or $\neg G$) were provable. Thus, given PM's (ω)-consistency, G is undecidable. This reasoning is based on the purely hypothetical assumption of the provability of G; it does not consider any specific proof strategies for proving formulas of a certain form within PM.

Given an algorithmic unprovability proof for G, the meta-mathematical statement that G is provable would be reduced to absurdity. This would be a compelling reason to abandon any search for a proof. Such a proof by contradiction would contain a "physical element" (I, §14) because a *meta*-mathematical statement concerning the provability of G is reduced to absurdity on the basis of an algorithmic, and thus purely mathematical, proof. Wittgenstein does not reject such a proof by contradiction in §14."

... *Lampert:* [**Lam17**].

We note further that, according to Lampert, Wittgenstein's remarks in [**Wi78**] can be interpreted as claiming that any 'intended interpretation' of quantification in 'an *instance* of a formula or of its abbreviation, such as G or $\neg\exists yB(y, \ulcorner G \urcorner)$' in Gödel's reasoning would introduce an element of 'prose' which—in the context of the *evidence-based* perspective of this investigation—may reasonably be taken to be an assumption such as that of Aristotle's particularisation (Definition 3.1 in §3.1; see also §14.2), which is stronger than both Gödel's ω-consistency (see §15.7) and Rosser's Rule C (see §15.6):

"The proofs by contradiction of the type to which Wittgenstein objects are proofs that involve *interpretation* of logical formulas: the inconsistency concerns the relation between the provability of a formula (proven or merely assumed) and its interpretation. Here, "interpretation" is not to be understood in terms of purely formal semantics underlying proofs of correctness or completeness. Formal semantics assign extensions to formal expressions without considering specific instances of formal expressions that are meant to refer to extensions. Instead, in proofs of contradiction Wittgenstein is concerned with an "interpretation of a formula" refers to an *instance* of a formula or of its abbreviation, such as G or $\neg\exists yB(y, \ulcorner G \urcorner)$, stated as a sentence in ordinary language or a standardized fragment of an ordinary language. Interpretations of this kind are so-called "intended interpretations" or "standard interpretations", which are intended to identify extensions such as truth values, truth functions, sets or numbers by means of ordinary expressions. As soon as interpretations of this kind become involved, one departs from the realm of mathematical calculus and "prose" comes into play, in Wittgenstein's view. Therefore, Wittgenstein's "non-revisionist" attitude does not apply to proofs by contradiction that rest on intended interpretations. A rigorous mathematical proof should not be affected by the problem that some intended interpretation may not refer to that to which it is intended to refer, which is a genuinely philosophical problem."

... *Lampert:* [**Lam17**].

21.4. What is mathematics?

Without attempting to address the issue in its broader dimensions, we take Wittgenstein's remarks in [**Wi78**] as implicitly suggesting that:

(i) Mathematics is to be considered as a set of precise, symbolic, languages.

(ii) Any language of such a set, say the first order Peano Arithmetic PA (or Russell and Whitehead's PM in Principia Mathematica, or the Set Theory ZF), is intended to express—in a finite, unambiguous, and communicable manner—relations between elements that are external to the language PA (or to PM, or to ZF).

(iii) Moreover, each such language is two-valued if we assume that a specific relation either holds or does not hold externally under any valid interpretation of the language.

(iv) Further:

- A selected, finite, number of primitive formal assertions about a finite set of selected primitive relations of, say, a language $\mathcal{L}$ are defined as axiomatically $\mathcal{L}$-provable;
- All assertions about relations that can be effectively defined in terms of the primitive relations are termed as $\mathcal{L}$-provable if, and only if, there is a finite sequence of assertions of $\mathcal{L}$, each of which is either a primitive assertion or which can effectively be determined in a finite number of steps as an immediate consequence of any two assertions preceding it in the sequence by a finite set of finitary rules of consequence.

We note that the semiotics of the *evidence-based* perspective of §21.2 to §21.4 (see also §23) is reflected in Brian Rotman's broader analysis:

> "Insofar, the, as the subject matter of mathematics is the whole numbers, we can say that its objects—the things which it countenances as existing and which it is said to be 'about'—are unactualized possibles, the potential sign production of a counting subject who operates in the presence of a notational system of signifiers. Such a thesis, though, is by no means restricted to the integers. Once it is accepted that the integers can be characterized in this way, essentially the same sort of analysis is available for numbers in general. The real numbers, for example, exist and are created as signs in the presence of the familiar extension of Hindu numerals—the infinite decimals—which act as their signifiers. Of course, there are complications involved in the idea of signifiers being infinitely long, but from a semiotic point of view the problem they present is no different from that presented by *arbitrarily* long finite signifiers. And moreover, what is true of numbers is in fact true of the entire totality of mathematical objects: they are all signs—thought/scribbles—which arise as the potential activity of a mathematical subject.
>
> Thus mathematics, characterized here as a discourse whose assertions are predictions about the future activities of its participants, is 'about'—insofar as this locution makes sense—itself. The entire discourse refers to, is 'true' about, nothing other than its own signs. And since mathematics is entirely a human artefact, the truths it establishes—if such is what they are—are attributes of the mathematical subject: the tripartite agency of Agent/Mathematician/Person who reads and writes mathematical signs and suffers its persuasions.
>
> But in the end, 'truth' seems to be no more than the unhelpful relic of the platonist obsession with a changeless eternal heaven. The question of whether a mathematical assertion, a prediction, can be said to *be* 'true' (or accurate or correct) collapses into a problem about the tense of the verb. A prediction—about some determinate world for which true and

> false make sense—might in the future be seen to be true, but only *after* what it foretold has come to pass; for only then, and not before, can what was pre-dicted be dicted. Short of fulfillment, as is the condition of all but trivial mathematical cases, predictions can only be believed to be true. Mathematicians believe because they are persuaded to believe; so that what is salient about mathematical assertions is not their supposed truth about some world that precedes them, but the inconceivability of persuasively creating a world in which they are denied. Thus, instead of a picture of logic as a form of truth-preserving inference, a semiotics of mathematics would see it as an inconceivability-preserving mode of persuasion—with no mention of "truth' anywhere."
>
> *... Rotman:* [**Rot88**], *pp.33-34.*

21.5. An interpretation must be effectively decidable

We take Rotman's semiotic perspective as echoing the essence of Wittgenstein's remarks, if we view the latter as indicating that an effective interpretation $\mathcal{I}_{L(D)}$ of a language L into the domain D of another language L' with a well-defined logic is essentially the specification of an effective method by which any assertion of L is translated unambiguously into a unique assertion in L'.

Clearly, if an assertion is provable in L, then it should be effectively decidable as true under any interpretation of L in the domain D of L'—since a finite deduction sequence of L would, prima facie, translate as a finite logical consequence in D under the interpretation.

21.6. Is the converse necessarily true?

The question arises:

Query 21.8. If an assertion of L is decidable as true/false under an interpretation $\mathcal{I}_{L(D)}$ in the domain D of L', then does such decidability also ensure an effective method of deciding its corresponding provability/unprovability in L?

Obviously, such a question can only be addressed unambiguously if there is an effective method for determining whether an assertion of L is decidable as true/false in D under the interpretation $\mathcal{I}_{L(D)}$. If there is no such effective method, then we are faced with the following thesis that is implicit in, and central to, Wittgenstein's 'notorious' remark:

Thesis 21.9. If there is no effective method for the unambiguous decidability of the assertions of a mathematical language L under any interpretation $\mathcal{I}_{L(D)}$ of L in the domain D of a language L', then L can only be considered a mathematical language of subjective expression, but not a mathematical language of effective, and unambiguous, communication under interpretation in L'.

What this means is that, in the absence of an effective method of decidability of the truth/falsity of the formulas of a mathematical language such as PA in the domain N of the natural numbers under the standard interpretation $\boldsymbol{M}$ of PA, it is meaningless to ask whether, in general, a specific assertion of PA is decidable as true or not in N under the interpretation $\boldsymbol{M}$ (the question of whether the assertion is decidable in PA as provable or not is, then, an issue of secondary consequence).

21.7. Tarskian truth under the standard interpretation

The philosophical dimensions of this thesis emerge if we consider the standard interpretation $\boldsymbol{M}$ of PA over the structure of the natural numbers where (cf. [**Me64**]):

(a) The set of non-negative integers is the domain $\mathbb{N}$;

(b) The integer 0 is the interpretation of the symbol '0' of PA;

(c) The successor operation (addition of 1) is the interpretation of the '′' function (i.e. of f_1^1 in [**Me64**]);

(d) Ordinary addition and multiplication are the interpretations of '+' and '$\star$';

(e) The interpretation of the predicate letter '=' is the equality relation.

Now, post-Gödel, classical theory seems to hold that:

(f) $\boldsymbol{M}$ is a well-defined interpretation of PA in $\mathbb{N}$;

(g) PA formulas are decidable under $\boldsymbol{M}$ in $\mathbb{N}$ by Tarski's definitions of satisfiability and truth (cf. [**Me64**], p49-53);

(h) However, the truth and satisfiability of a PA formula under $\boldsymbol{M}$ is not always effectively verifiable in $\mathbb{N}$[7].

However, the question, implicit in Wittgenstein's argument regarding the possibility of a semantic ambiguity in Gödel's reasoning, then arises:

Query 21.10. How can we assert that a PA formula (whether PA-provable or not) is true under the standard interpretation $\boldsymbol{M}$ of PA, so long as such truth remains effectively unverifiable under $\boldsymbol{M}$?

Since the issue is not resolved unambiguously by Gödel in his 1931 paper (nor, prima facie, by subsequent standard interpretations of his formal reasoning and conclusions), Wittgenstein's remark can be taken to argue that, although we may validly draw various conclusions from Gödel's formal reasoning and conclusions, the Platonic existence of a true or false assertion under the standard interpretation $\boldsymbol{M}$ of PA cannot be amongst them.

21.8. Is PA categorical?

A related philosophical issue is, then, the question:

Is PA categorical?

In other words, since PA is intended as a finitary (first-order) formalisation of the arithmetic of the natural numbers as expressed by the categorical second-order formulation of the Peano-Dedekind axioms[8], is such formalisation unique?

[7]Expressed formally by Tarski's 1936 Theorem (cf. [**Me64**], Corollary 3.38, p151):

"The set Tr of Gödel-numbers of wfs of PA which are true in the standard model is not arithmetical, i.e. there is no wf $A(x)$ of PA such that Tr is the set of numbers k for which $A(x)$ is true in the standard model."

[8]We note that Dedekind proved that these axioms are categorical, in the sense that any two putative models of the axioms would be isomorphic.

The standard response to this question seems to lie at the heart of Wittgenstein's reservations, and to be the cause of the uneasiness felt by subsequent philosophers who question the standard interpretations of classical mathematical theory.

Now, this investigation is based on the premise that a negative answer—which would imply that intuitively self-evident PA axioms cannot be taken as a faithful first-order formalisation of our intuitive arithmetic of the natural numbers—is a philosophically unappealing and implicitly self-limiting admission.

An affirmative answer, on the other hand, whilst validating PA as a finitary formalisation of the second-order Dedekind-Peano axioms, would further imply that, since an assertion would then be effectively decidable in PA if, and only if, it were effectively decidable under an interpretation in $\mathbb{N}$ (cf. Theorem 10.2), there must be some effective method of defining Tarskian satisfiability and truth under an interpretation in $\mathbb{N}$.

21.9. Defining effective satisfiability and truth

Although Wittgenstein does not appear to have attempted such a definition—possibly as it may have seemed to involve technicalities beyond the scope of his reflections—we note in [**An16**] that such an effective method is, indeed, made available to us by, curiously, a constructive, *weak*, 'Wittgensteinian' interpretation of Gödel's reasoning and conclusions (as detailed in Chapter 8); an interpretation that is, ironically, more in sympathy with Wittgenstein's constructive approach than Gödel's Platonic one.

21.10. Undecidability in PA

Now, a thesis—in this investigation—of a constructive interpretation of Gödel's reasoning and conclusions is that (see Definitions 5.2 and 5.2 in §5.1), under any constructively well-defined interpretation of PA, we may not interpret the meta-assertion:

PA proves: $[(\forall x)F(x)]$

as the non-verifiable, Tarskian meta-assertion:

$F(x)$ is satisfied by any natural number x in N.

We must interpret it, instead, as either one of the *evidence-based* meta-assertions:

(i) For any given natural n of N, there is an algorithm that will evidence $F(n)$ as satisfied in N;

(ii) There is an algorithm that, given any natural number n of N, will evidence $F(n)$ as satisfied in N.

It follows that in the second case (ii)—a possibility hitherto unsuspected by conventional wisdom—both the meta-assertions:

PA does not prove $[(\forall x)F(x)]$

and:

PA proves $[\neg(\forall x)F(x)]$

interpret under any constructively well-defined interpretation of PA as the meta-assertion:

> There is no algorithm that, given any natural number n of N, will evidence $F(x)$ as satisfied in N.

Consequently, an *evidence-based* interpretation of Gödel's reasoning and conclusions implies that there can be no undecidable propositions in PA; in other words, that PA is syntactically complete (in the sense of §10.2)!

21.11. How definitive is the usual interpretation of Gödel's reasoning?

However we are then faced with the question:

QUERY 21.11. Since the usual textbook interpretations of Gödel's reasoning and conclusions assert that PA is syntactically incomplete, how definitive are such interpretations?

Now, in Theorem VI of his seminal 1931 paper [**Go31**], Gödel defines a formal system P of arithmetic, and a P-proposition, say $[(\forall x)R(x)]$, such that:

(i) $[(\forall x)R(x)]$ is not P-provable;

(ii) $[(R(n)]$ is P-provable for any given numeral $[n]$.

Gödel then explicitly remarks (as implicitly self-evident) that any system of arithmetic such as P is ω-consistent, and concludes that P is essentially incomplete since:

(iii) $[\neg(\forall x)R(x)]$ is not P-provable if P is ω-consistent.

We note that Wittgenstein's remarks indicate that, prima facie, there appear no intuitively significant philosophical grounds for treating the ω-consistency of P as self-evident.

Justifying Wittgenstein's reservations, we note that not only was Gödel's intuition misleading, but it is the ω-inconsistency of PA that—by Gödel's own formal reasoning (see Theorem 8.5; also Corollary 11.6)—is natural, and intuitively unobjectionable, under a constructively well-defined interpretation of the concept of 'PA proves: $[(\forall x)F(x)]$' as described earlier.

Under such interpretation, an ω-inconsistent PA does not imply that PA, or any of its interpretations, are either inconsistent or unnaturally consistent; it simply implies that there are (algorithmically *verifiable* but not algorithmically *computable*) arithmetical relations that cannot be verified uniformly by a common algorithm over the domain of their interpretation.

Thus, it may have been the absence of an adequately technical counter-argument that has left Wittgenstein's viewpoint—and that of others such as Lucas ([**Lu61**]) and Penrose ([**Pe90**] and [**Pe94**]), who have shared his reservations on intuitively sound philosophical grounds—vulnerable to the arguments advanced by the usual textbook interpretations of Gödel's reasoning and conclusion; these implicitly imply—on the basis of purely technical, but misleading, considerations that follow from the invalidly assumed ω-consistency of PA—that any interpretation of Gödel's reasoning and conclusion are essentially counter-intuitive philosophical concepts which must be accepted as extending our intuition.

21.12. When does a formal assertion 'mean' what it represents?

Another important philosophical issue—which is implicit in the key thesis of Floyd and Putnam's paper [**FP00**]—is reflected in Wittgenstein's remark:

> "If you assume that the proposition is provable in Russell's system, that means it is true in the Russell sense, and the interpretation 'P is not provable' ... has to be given up."
> *... Wittgenstein:* [**Wi78**]*, Appendix III 8.*

We may state this issue explicitly as:

QUERY 21.12. When does a formal assertion 'mean' what it represents?

Now if, as argued earlier, we accept that PA formalises our intuitive arithmetic of the natural numbers, and that there is a constructively well-defined interpretation of PA, it follows that every well-formed formula of PA interprets as a well-defined arithmetical expression in $\mathbb{N}$, and every well-defined arithmetical expression in $\mathbb{N}$ can be represented as a PA-formula.

The question then arises:

QUERY 21.13. When is an arbitrary number-theoretic function or relation representable in PA?

21.13. Formal expressibility and representability

Now, the classical PA-expressibility and representability of number-theoretic functions and relations is addressed by the following three definitions (cf. [**Me64**], p117-118):

(a) A number-theoretic relation $R(x_1, \ldots, x_n)$ is said to be *expressible* in PA if, and only if, there is a well-formed formula $[A(x_1, \ldots, x_n)]$ of PA with n free variables such that, for any natural numbers $k_1, \ldots, k_n$:
   - (i) if $R(k_1, \ldots, k_n)$ is true, then PA proves: $[A(k_1, \ldots, k_n)]$;
   - (ii) if $R(k_1, \ldots, k_n)$ is false, then PA proves: $[\neg A(k_1, \ldots, k_n)]$.

(b) A number-theoretic function $f(x_1, \ldots, x_n)$ is said to be *representable* in PA if, and only if, there is a well-formed formula $[A(x_1, \ldots, x_n, y)]$ of PA, with the free variables $[x_1, \ldots, x_n, y]$, such that, for any natural numbers $k_1, \ldots, k_n, l$:
   - (i) if $f(k_1, \ldots, k_n) = l$, then PA proves: $[A(k_1, \ldots, k_n, l)]$,
   - (ii) PA proves: $[(\exists! y)A(k_1, \ldots, k_n, y)]$[9].

(c) A number-theoretic function $f(x_1, \ldots, x_n)$ is said to be *strongly representable* in PA if, and only if, there is a well-formed formula $[A(x_1, \ldots, x_n, y)]$ of PA, with the free variables $[x_1, \ldots, x_n, y]$, such that, for any natural numbers $k_1, \ldots, k_n, l$:

---

[9] *Definition*([**Me64**], p.79): $[(\exists! x)A(x)] \equiv [(\exists x)A(x) \wedge (\forall x)(\forall y)(A(x) \wedge A(y)) \supset x = y]$

(i) if $f(k_1, \ldots, k_n) = l$, then PA proves: $[A(k_1, \ldots, k_n, l)]$,

(ii) PA proves: $[(\exists! y)A(x_1, \ldots, x_n, y)]$,

21.14. When may we assert that $A^*(x_1, \ldots, x_n)$ 'means' $R(x_1, \ldots, x_n)$?

We can, thus, re-phrase our query 21.12 as:

QUERY 21.14. If a number-theoretic relation $R(x_1, \ldots, x_n)$ is expressible by a PA-formula $[A(x_1, \ldots, x_n)]$, when may we assert that the standard interpretation, $A^*(x_1, \ldots, x_n)$ of $[A(x_1, \ldots, x_n)]$ 'means' $R(x_1, \ldots, x_n)$?

Now we note that, if $R(x_1, \ldots, x_n)$ is arithmetical, then the standard interpretation of one of its PA-representation $[A(x_1, \ldots, x_n)]$ *is necessarily* $R(x_1, \ldots, x_n)$.

Hence every arithmetical relation $R(x_1, \ldots, x_n)$ is the standard interpretation of some PA-formula that expresses $R(x_1, \ldots, x_n)$ in PA, and we can adapt this to give a formal definition of the term 'means':

DEFINITION 21.15. If a number-theoretic relation $R(x_1, \ldots, x_n)$ is expressible by a PA-formula $[A(x_1, \ldots, x_n)]$, then we say that the standard interpretation $A^*(x_1, \ldots, x_n)$ of $[A(x_1, \ldots, x_n)]$ means $R(x_1, \ldots, x_n)$ if, and only if, $R(x_1, \ldots, x_n)$ is the standard interpretation of some PA-formula that expresses $R(x_1, \ldots, x_n)$ in PA.

The query 21.12 can now be expressed precisely as:

QUERY 21.16. When is a number-theoretic relation the standard interpretation of some PA-formula that expresses it in PA?

Now, by definition, the number-theoretic relation $R(x_1, \ldots, x_n)$, and the arithmetic relation $A^*(x_1, \ldots, x_n)$, can be effectively shown as equivalent for any given set of natural number values for the free variables contained in them.

However, for $R(x_1, \ldots, x_n)$ to mean $A^*(x_1, \ldots, x_n)$, we must have, in addition, that $R(x_1, \ldots, x_n)$ can be effectively transformed into an arithmetical expression, so that it can be the standard interpretation of some PA-formula that expresses it in PA.

21.15. PA has a constructively well-defined logic

The significance of Wittgenstein's *notorious* paragraph (§21.3) is thus that, if interpreted appropriately, it establishes that Wittgenstein's philosophical perspective on 'logic' and 'truth' does, indeed, allow us to:

- Define a finitary, computably realizable, interpretation $\boldsymbol{B}$ of PA over the structure $\mathbb{N}$ of the natural numbers (§9);
- Equate the provable formulas of the first order Peano Arithmetic PA with the PA formulas that are 'true' under $\boldsymbol{B}$ (Theorem 10.2);

from which we can conclude that:

THEOREM 21.17. *PA has a constructively well-defined logic.*

PROOF. By Theorem 10.2 the set of axioms and rules of inference of PA+FOL constructively assign unique truth-values:

(a) Of provability/unprovability to the formulas of PA; and

(b) Of computably realizable truth/falsity to the sentences of Dedekind's Peano Arithmetic which is defined semantically by the computably realizable interpretation $\boldsymbol{B}$ of PA over the structure $\mathbb{N}$ of the natural numbers.

The theorem follows. □

21.16. What is an axiom

From the perspective of §21.2, it would thus follow that the axioms and rules of inference of a language:

- are not intended to correlate the 'provable' propositions of a language with the (platonically?) 'true' propositions under a constructively well-defined interpretation of the language (though that might be an incidental consequence),
- but are essential logical rules of the language that are intended to constructively assign 'truth' values to the propositions of the language under the interpretation,
- with the sole intention of enabling unambiguous and effective communication about various characteristics of the structure—which may, or may not, be constructively well-defined—over which the interpretation is defined.

21.17. Do the axioms circumscribe the ontology of an interpretation?

If so, it would further follow that the ontology of any interpretation of a language is circumscribed not by the 'logic' of the language—which is intended solely to assign unique 'truth' values to the declarative sentences of the language—but by the rules that determine the 'terms' that can be admitted into the language without inviting contradiction in the broader sense of how, or even whether, the brain—viewed as the language defining and logic processing part of any intelligence—can address contradictions (see §23.11).

We contrast the above perspective with a more classical perspective such as that, for instance, of Weyl which, from an early-intuitionistic point of view, posits axioms as 'implicit definitions' (as does Solomon Feferman later in [**Fe99**]; see also [**Fe97**], p.2):

> "You all know that Descartes' introduction of coordinates seems to reduce geometry to arithmetic (understood in the widest sense, i.e., as a theory of the real numbers). Given Pieri's formulation of geometry, which remains entirely within the geometric realm, we can perform the reduction to arithmetic by means of the following three propositions (in which, as before, I limit myself to plane geometry):
>
> 1. A pair of real numbers (x, y) is called a point.
>
> 2. If $(x_1, y_1), (x_2, y_2), (x_3, y_3)$ are three points, then they satisfy relation E if and only if
>
> $$(x_2 - x_1)^2 + (y_2 - y_1)^2 = (x_3 - x_1)^2 + (y_3 - y_1)^2.$$

3. We count as geometric point-relations only those numerical relations between the coordinates of the points that are invariant under translation and orthogonal transformation.

Would it be right to treat these propositions as definitions of "point," "geometry," and the fundamental relation E? Surely they are definitions only in a severely extended sense. We earlier altered the significant content (*Vorstel-lungsinhalt*) of such expressions as "three points lie on a straight line"—but only in a way that preserved the scope of these concepts. We have now replaced the original concepts with others that, at first glance, are entirely different.

Nonetheless, if a proposition of Euclidean geometry is true when taken in its proper sense, it will remain true when we take its constituent expressions in the new arithmetical sense. This situation has a kind of complement in our ability to express the same significant content in various languages in entirely different ways. Here, however, the same verbal expression receives thoroughly different contents because we assign a new meaning to each concept. The procedure applied here might best be described as follows. There are two systems of objects. Certain relations $\varepsilon_1, \varepsilon_1', \ldots$ obtain between objects of the first system while relations $\varepsilon_2, \varepsilon_2', \ldots$ obtain between those of the second. If there is a one-to-one correlation between the objects and relations of the one system and the objects and relations of the other such that correlated relations always hold between correlated objects—if the systems are, in this sense, completely isomorphic with one another—then there is also a one-to-one correlation between the true propositions of the two systems and we could, without falling into any errors, *identify* the two systems with one another. The discovery of such an isomorphism is obviously important and has benefits quite analogous to those mathematics derives from abstract group theory: unication, great economy of thought, but also an expansion of the methods available to researchers. Thanks to Descartes' discovery, I can not only use numerical analysis to prove geometric theorems; I can use geometric intuition to discover truths about numbers. It is in the spirit of this identification of isomorphic systems (an identification justified from the mathematical point of view) that we treat the axioms of, say, geometry not as fundamental statements about spatial relations obtaining in the actual space surrounding us, but merely as implicit definitions of certain relations devoid in themselves of any intuitive content. These axioms, construed as implicit definitions, certainly do not make those concepts entirely definite. But that does not matter because, even in geometry, we only care about the properties asserted in the axioms. The significant content of Euclidean geometry, what we call space and spatial relations, is not exhausted by that geometry's assertions. This strikes me as a situation of philosophical interest.

The method of implicit definition—a method that does not clarify concepts on the basis of other concepts whose sense is taken to be understood, but only offers a system of propositions or axioms in which the concepts occur—this method has been employed frequently in mathematics. It has the advantage of highlighting, at the very start, the most important properties of the concepts to be defined, properties that might be only remote consequences of a proper definition. However, an implicit definition through axioms is always provisional in that you can rely on it only if the axioms are consistent, i.e., only if you can identify a system of explicitly defined concepts that satisfies the axioms. A good example of what we are discussing is Lebesgue's treatment of the concept of the integral in Ch. VII of his "Leçons sur l'intégration" (Paris 1904). There he distinguishes between explicit and implicit definitions drawing a contrast between the "constructive" and the "descriptive.""

. . . Weyl: [**We10**]*, pp.5-6.*

CHAPTER 22

The curious consequence of Goodstein's argumentation in $\mathbf{ACA_0}$

To illustrate Wittgenstein's point (§21.3 to 21.16), we consider a curious consequence—of a failure to constructively assign unique 'truth' values to the axioms of a formal language under an interpretation—in the following analysis of Goodstein's argumentation in support of the 'Theorem' that bears his name.

> **Goodstein's Theorem**: Every Goodstein sequence defined over the natural numbers terminates in 0.

22.1. The gist of Goodstein's argument

We note that, for any natural number m, R. L. Goodstein ([**Gd44**]) constructs a natural number sequence of terms with two arguments:

$$S(m) \equiv \{s_1(m,2),\ s_2(m,3), \ldots, s_i(m,i+1), \ldots\}$$

by an unusual, but valid, algorithm (§22.8).

Viewed from a pedantic perspective, Goodstein then considers the corresponding sequence of finite ordinals[1]:

$$T(m_o) \equiv \{t_1(m_o,2_o),\ t_2(m_o,3_o), \ldots, t_i(m_o,(i+1)_o), \ldots\}$$

and constructs a corresponding sequence of transfinite ordinals:

$$U(m_o) \equiv \{u_1(m_o,\omega),\ u_2(m_o,\omega), \ldots, u_i(m_o,\omega), \ldots\}$$

where $U(m_o)$ is obtained from $T(m_o)$ by replacing, for each $i \geq 1$, the ordinal number $(i+1)_o$ in the term $t_i(m_o,(i+1)_o)$ of $T(m_o)$ with Cantor's first transfinite ordinal ω.

He then shows that the ordinal inequality $u_i(m_o,\omega) >_o u_{i+1}(m_o,\omega)$ holds for all $i \geq 1$, and so the sequence $U(m_o)$ of ordinals is bounded above by some transfinite ordinal.

Since we cannot have an infinitely descending sequence of ordinals, he concludes that $U(m_o)$, and ipso facto $T(m_o)$ and $S(m)$, must necessarily terminate finitely[2]; thus yielding Goodstein's Theorem that $s_i(m,i+1) = 0$ for some i in the sequence $S(m)$.

[1] Where a second-order, set-theoretically-defined, ordinal number n_o is constructed by a Comprehension Axiom—such as that of the subsystem ACA_0 (see §22.3)—from the first-order, arithmetically-defined natural number n.

[2] *Terminate finitely*: By Goodstein's algorithm, after a 0 all subsequent members of the sequence necessarily remain 0, and the sequence is said in such a case to terminate finitely at its first 0 value.

22.2. The anomaly in Goodstein's argument

Consider, however, the corresponding natural number sequence of functions:

$$F(m) \equiv \{f_1(m,x),\ f_2(m,x), \ldots, f_i(m,x,\ldots\}$$

obtained by replacing, for each $i \geq 1$, the natural number $i+1$ in the term $s_i(m, i+1)$ of $S(m)$ with the variable x.

It is tedious, but straightforward (see §22.7), to show that the algebraic inequality $f_i(m,x) > f_{i+1}(m,x)$ holds for all $i \geq 1$.

However, in this case we can conclude from the algebraic inequality that $S(m)$ must necessarily terminate finitely (i.e. $s_i(m, i+1) = 0$ for some i) if, and only if, $S(m)$ is bounded above by some finite natural number.

We now see that Goodstein's transfinite reasoning only establishes that the ordinal sequence $T(m_o)$ corresponding to the natural number sequence $S(m)$ is bounded above by some transfinite ordinal number.

Whilst this may be both necessary and sufficient to conclude that the second order Comprehension Axioms entail that the second-order, set-theoretically-defined, ordinal sequence $T(m_o)$ must terminate finitely, it is not sufficient—as Skolem has observed (see §22.4)—to conclude that the first order axioms of PA must also entail that the natural number sequence $S(m)$ terminates finitely.

In other words constructive mathematics, which cannot admit transfinite elements (see §20.7), must admit the possibility that $S(m)$ may not terminate finitely!

It follows that if we treat the subsystem ACA_0 of second-order arithmetic (as defined in, say, [**Fe97**], pp.12-13) as a conservative extension[3] of PA (cf. [**Fe97**], p.18) that is *equiconsistent* with PA, then we are led to the anomalous conclusion—since PA is consistent by Theorem 9.10—that:

> Goodstein's sequence $G_o(m_o)$ over the finite ordinals in ACA_0 *terminates* with respect to the ordinal inequality '$>_o$' even if Goodstein's sequence $G(m)$ over the natural numbers in ACA_0 *does not terminate* with respect to the natural number inequality '$>$' in any putative model of ACA_0 (Theorem 22.3).

22.3. The subsystem ACA_0

We note that ACA_0 is defined as the extension of PA with the PA variables, say $[m], [n], \ldots$, ranging now over the ACA_0 numerals; with additional set variables $[X], [Y], [Z], \ldots]$ ranging over ACA_0 sets; and with an additional arithmetical Comprehension Axiom schema where, if $[\varphi(n)]$ is a formula with a free numeral variable $[n]$—and possibly other free variables such as, say, $[m]$ and $[X]$, but not the set variable $[Z]$—the Comprehension Axiom for $[\varphi]$ is the formula that defines sets in ACA_0 by:

$$[(\forall m)(\forall X)(\exists Z)(\forall n)(n \in Z \leftrightarrow \varphi(n))]$$

---

[3] *Conservative extension*: A theory T_2 is a (proof theoretic) conservative extension of a theory T_1 if the language of T_2 extends the language of T_2; that is, every theorem of T_1 is a theorem of T_2, and any theorem of T_2 in the language of T_1 is already a theorem of T_1.

If $[\varphi(n)]$ is a unary formula, the ACA_0 comprehension axiom for $[\varphi]$ thus makes it possible to form the set:

$$[Z = \{n|\varphi(n)\}]$$

of numerals satisfying $[\varphi(n)]$ in any putative model of ACA_0.

Taking $[\varphi(n)]$ as $[n = n]$ would thus admit a constant $[Z]$ as a term in ACA_0 that would interpret in any putative model of ACA_0 as the set $\mathbb{N}$ of all natural numbers.

We view the curious conclusion of Goodstein's argumentation as reflecting the circumstance that the 'truth' of the Comprehension Axioms of ACA_0 under an interpretation is not constructively well-definable, since they contain an existential quantifier that is intended to admit Aristotle's particularisation under any interpretation.

We conclude that:

THEOREM 22.1. *The subsystem ACA_0 of second-order arithmetic is not a conservative extension of PA.*

PROOF. By Theorem 9.10 PA is consistent and has a model. If ACA_0 is a conservative extension of PA, then it too is consistent[4] and has a model which admits Aristotle's particularisation, and which is also a model of PA. However, by Corollary 15.10, Aristotle's particularisation cannot hold in any model of PA. The theorem follows. □

We note that Theorem 22.1 contradicts conventional wisdom:

(a) "In other words, ACA_0 is a *conservative extension* of first order arithmetic. This may also be expressed by saying that Z_1, or equivalently PA, is the *first order part* of ACA_0."
... Simpson: [**Sim06**], *§I.3, REMARK I.3.3, p.8.*

(b) "As a logical footnote to that, the system $\mathbf{ACA}_0$, which I described here, is a conservative extension of Peano Arithmetic, even though it employs second order concepts."
... Feferman: [**Fe97**], *p.18.*

22.4. Goodstein's Theorem defies belief: *justifiably*!

We also note that, even prima facie, the set-theoretical argument for Goodstein's Theorem meets William Gasarch's criteria ([**Ga10**]) of an argument that defies belief.

In this case, though, the disbelief is justified since, as we have outlined in §22.2, Goodstein's argument can be carried out completely over the structure $\mathbb{N}$ of the natural numbers without appealing to any properties of transfinite ordinal sequences.

However we cannot conclude from the arithmetical argument that every Goodstein sequence over the natural numbers (defined formally in §22.8) must terminate finitely.

We shall now argue that Goodstein's argument is a curious case of proving a Theorem involving the set-theoretical membership-based relation '$>_o$' over the

[4] "If T' is a conservative extension of T, then T' is consistent iff T is consistent." *... Shoenfield:* [**Sh67**], *p.42.*

structure of the ordinals below ϵ_o and—ignoring Thoraf Skolem's cautionary remarks about unrestrictedly corresponding putative mathematical entities across domains of different axiom systems ([**Sk22**])—*invalidly* postulating that a corresponding theorem involving the natural number inequality relation '$>$' *must* therefore hold over the structure of the natural numbers.

We note that, in a 1922 address delivered in Helsinki before the Fifth Congress of Scandinavian Mathematicians, Skolem improved upon both the argument and statement of Löwenheim's 1915 theorem ([**Lo15**], p.235, Theorem 2)—subsequently labelled as the (downwards) Löwenheim-Skolem Theorem ([**Sk22**], p.293).

> **(Downwards) Löwenheim-Skolem Theorem** ([**Lo15**], p.245, Theorem 6; [**Sk22**], p.293): If a first-order proposition is satisfied in any domain at all, then it is already satisfied in a denumerably infinite domain.

Skolem then drew attention to a:

> **Skolem's (apparent) paradox**: "...peculiar and apparently paradoxical state of affairs. By virtue of the axioms we can prove the existence of higher cardinalities, of higher number classes, and so forth. How can it be, then, that the entire domain B can already be enumerated by means of the finite positive integers? The explanation is not difficult to find. In the axiomatization, "set" does not mean an arbitrarily defined collection; the sets are nothing but objects that are connected with one another through certain relations expressed by the axioms. Hence there is no contradiction at all if a set M of the domain B is non-denumerable in the sense of the axiomatization; for this means merely that *within* B there occurs no one-to-one mapping Φ of M onto Z_o (Zermelo's number sequence). Nevertheless there exists the possibility of numbering all objects in B, and therefore also the elements of M, by means of the positive integers; of course such an enumeration too is a collection of certain pairs, but this collection is not a "set" (that is, it does not occur in the domain B)."
>
> *... Skolem:* [**Sk22**]*, p.295.*

22.5. Goodstein's argument over the natural numbers

Now, we note that, for any natural number m, R. L. Goodstein ([**Gd44**]) uses the properties of the hereditary representation of m to construct a sequence $G(m) \equiv \{g_1(m),\ g_2(m),\ \ldots\}$ of natural numbers by an unusual, but valid, algorithm (§22.8).

> *Hereditary representation*: The representation of a number as a sum of powers of a base b, followed by expression of each of the exponents as a sum of powers of b, etc., until the process stops. For example, we may express the hereditary representations of 266 in base 2 and base 3 as follows:
>
> $$266_{[2]} \equiv 2^{8_{[2]}} + 2^{3_{[2]}} + 2 \equiv 2^{2^{(2^{2^0}+2^0)}} + 2^{2^{2^0}+2^0} + 2^{2^0}$$
>
> $$266_{[3]} \equiv 2.3^{4_{[3]}} + 2.3^{3_{[3]}} + 3^{2_{[3]}} + 1 \equiv 2.3^{(3^{3^0}+3^0)} + 2.3^{3^{3^0}} + 3^{2.3^0} + 3^0$$

For the moment we shall ignore the peculiar manner of constructing the individual members of the Goodstein sequence, since these are not germane to understanding the essence of Goodstein's argument. We need simply accept for now that $G(m)$ is well-defined over the structure $\mathbb{N}$ of the natural numbers, and has the following properties:

(i) For any given natural number $k > 0$ we can construct a hereditary representation—denoted[5] by $g_k(m)_{[k+1]}$—of $g_k(m)$ in the base $[k+1]$;

Example: The hereditary representations of the first two terms $g_1(266) = 266$ and $g_2(266) = (3^{81} + 83)$ of $G(266)$ are[6]:

$$g_1(266)_{[2]} \equiv 2^{2^{2+1}} + 2^{2+1} + 2$$

$$g_2(266)_{[3]} \equiv 3^{3^{3+1}} + 3^{3+1} + 2$$

(ii) We can also define a Goodstein Functional Sequence:

$$G(m)_{[x]} \equiv \{g_k(m)_{[(k+1) \hookrightarrow x]} : k > 0\} \text{ over } \mathbb{N}$$

by replacing the base $[k+1]$ in $g_k(m)_{[k+1]}$ with the variable x for each $k > 0$[7].

Example: The first two terms of $G(266)_{[x]}$ are thus:

$$g_1(266)_{[2 \hookrightarrow x]} \equiv x^{x^{x+1}} + x^{x+1} + x$$

$$g_2(266)_{[3 \hookrightarrow x]} \equiv x^{x^{x+1}} + x^{x+1} + 2$$

(iii) We can show that some member of Goodstein's sequence $G(m)$ evaluates to 0 if, and only if, there is some natural number z such that for any given natural number $k > 0$:

- If $g_k(m)_{[(k+1) \hookrightarrow z]} > 0$ in $G(m)_{[z]}$,
- Then $g_k(m)_{[(k+1) \hookrightarrow z]} > g_{k+1}(m)_{[(k+2) \hookrightarrow z]}$.

The proof of (iii)—which depends, of course, on the peculiar nature of Goodstein's algorithm—is straightforward and detailed in §22.7 The main point to note is that the proof appeals only to the *arithmetical* properties of the natural numbers.

The question arises:

QUERY 22.2. Are we free to *postulate* the existence of such a natural number z, and conclude that some member of $G(m)$ must evaluate to 0 in $\mathbb{N}$?

Though it appears absurd, the following theorem shows that this is precisely the freedom to which the ordinal-based argument for Goodstein's Theorem (Section 22.12) lays claim (albeit implicitly)!

THEOREM 22.3. *Goodstein's sequence $G_o(m_o)$[8] over the finite ordinals in any putative model $\mathbb{M}$ of ACA_0 terminates with respect to the ordinal inequality '$>_o$'*

---

[5]From a pedantic perspective the denotation should, of course, be: $(g_k(m))_{[k+1]}$.

[6]*Notation*: For ease of expression, we shall henceforth express 'a^0' as '1', and 'a^{b^0}' as 'a' unless indicated to the contrary.

[7]*Notation*: We prefer the notation $\hookrightarrow$ to that of the usual 'base bumping' function (cf. [**Cai07**]) as it makes the argument in §37 more transparent.

[8]*Notation*: For convenience of expression, we shall henceforth denote by m_o the ordinal (set) in $\mathbb{M}$ corresponding to the natural number m in $\mathbb{M}$; by '$+_o$' and '$>_o$' the function/relation

even if Goodstein's sequence $G(m)$ over the natural numbers does not terminate with respect to the natural number inequality '$>$' in $\mathbb{M}$.

PROOF. Assume that Goodstein's sequence $G(m) \equiv \{g_k(m)_{[(k+1)} : k > 0\}$ of natural numbers *does not terminate* with respect to the natural number inequality '$>$' in any putative model $\mathbb{M}$ of ACA_0.

Let n_{max} be the largest term amongst the first n terms of $G(m)$. It is tedious but straightforward to show that, by our assumption, n_{max} is a monotonically increasing sequence. Hence there is *no* natural number z such that:

$$g_k(m)_{[(k+1) \hookrightarrow z]} > g_{k+1}(m)_{[(k+2) \hookrightarrow z]} \text{ for all } k > 0.$$

Consider next Goodstein's ordinal number sequence $G_o(m_o) \equiv \{g_k(m_o) : k > 0\}$ over the finite ordinals.

Goodstein shows that, in the arithmetic of transfinite ordinals, the axiomatically postulated transfinite ordinal ω is such that:

$$g_k(m_o)_{[(k+1) \hookrightarrow \omega]} >_o g_{k+1}(m_o)_{[(k+2) \hookrightarrow \omega]} \text{ for all } k > 0.$$

Since there are no infinite descending sequences of ordinals with respect to the ordinal inequality '$>_o$', Goodstein's ordinal number sequence $G_o(m_o)$ must terminate finitely with respect to the ordinal inequality '$>_o$' in any putative model $\mathbb{M}$ of ACA_0. □

Moreover, since the finite ordinals can be meta-mathematically put into a 1-1 correspondence with the natural numbers, it follows that:

COROLLARY 22.4. *The relationship of terminating finitely with respect to the ordinal inequality '$>_o$' over an infinite set Z_0 of ordinals containing a transfinite ordinal cannot be corresponded to the relationship of terminating finitely with respect to the natural number inequality '$>$' over the set of natural numbers in any putative model $\mathbb{M}$ of ACA_0.* □

We now analyse the argument of Goodstein's Theorem in greater detail.

22.6. The argument of Goodstein's Theorem

The argument of Goodstein's Theorem (cf. [**Cai07**]) is that:

(i) The natural number considerations involved in the construction of Goodstein's sequence can all be formalised over the finite ordinals (sets) in any putative model $\mathbb{M}$ of ACA_0;

(ii) The comprehension axiom of ACA_0 does allow us to *postulate* the existence of an ordinal—Cantor's first limit ordinal ω—such that:

  (a) if $\{g_k(m_o)\}$—say $G_o(m_o)$—is the sequence of finite ordinals in $\mathbb{M}$ that corresponds to Goodstein's natural number sequence $G(m)$ in $\mathbb{M}$,

  (b) and $\{g_k(x)_{[(k_o+_o 1_o) \hookrightarrow x]} : k > 0\}$—say $G_o(m_o)_{[x]}$—the corresponding Goodstein Functional Sequence over $\mathbb{M}$,

---

letters relating to ordinals in $\mathbb{M}$ that correspond to the function/relation letters '+' and '$>$' that correspond to the natural numbers in $\mathbb{M}$, etc.

(c) then for any given natural number $k > 0$:

$$\text{If } g_k(m_o)_{[(k_o +_o 1_o) \hookrightarrow \omega]} >_o 0_o \text{ in } G_o(m_o)_{[\omega]},$$
$$\text{then } g_k(m_o)_{[(k_o +_o 1_o) \hookrightarrow \omega]} >_o g_{k+1}(m_o)_{[(k_o +_o 2_o) \hookrightarrow \omega]};$$

(iii) The sequence $\{g_1(m_o)_{[2_o \hookrightarrow \omega]},\ g_2(m_o)_{[3_o \hookrightarrow \omega]}, \ldots\}$ of ordinals cannot descend infinitely in $\mathbb{M}$;

(iv) Hence $G_o(m_o)$ terminates finitely in $\mathbb{M}$.

If ACA_0 is consistent, then such a $\mathbb{M}$ must 'exist' and the above argument is valid in $\mathbb{M}$. However, Goodstein's Theorem is the conclusion that Goodstein's sequence must *therefore* terminate finitely in $\mathbb{N}$!

Prima facie such a conclusion from the ordinal-based reasoning challenges belief insofar as we shall show that—at heart—the argument *essentially* appears to be that, since Goodstein's natural number sequence $G(m)$ obviously 'terminates finitely'[9] if, and only if, it is bounded above in $\mathbb{N}$ with respect to the arithmetical relation '$>$', we may conclude the existence of such a bound since Goodstein's ordinal sequence $G_o(m_o)$:

(a) *is* bounded above by ω in $\mathbb{M}$;

(b) 'terminates finitely' with respect to the ordinal relation '$>_o$';

(c) can be put in a 1-1 correspondence with $G(m)$;

and since the natural numbers can be put into a 1-1 correspondence with the finite ordinals!

We now show why such disbelief is justified since—as we detail in §20.7 to 20.8—the above invalidly[10] presumes that the structure $\mathbb{N}$ of the natural numbers is isomorphic to the sub-structure of the finite ordinals in the structure of the ordinals below ϵ_0, and so the property of 'terminating finitely' in any putative model of ACA_0 must interpret as the property of 'terminatingly finitely' in any model of PA.

22.7. The ordinal-based 'proof' of Goodstein's Theorem

For any given natural number m we can express $G(m)$ so that each term is expressed in it's hereditary representation:

$$G(m) \quad \equiv \quad \{g_1(m)_{[2]},\ g_2(m)_{[3]},\ g_3(m)_{[4]}, \ldots\} \tag{22.1}$$

where the first term $g_1(m)_{[2]}$ denotes the unique hereditary representation of the natural number m in the natural number base [2]:

$$\text{e.g., } g_1(9)_{[2]} \equiv 1.2^{(1.2^{1.2^0}+1.2^0)} + 0.2^{(1.2^{1.2^0}+0.2^0)} + 0.2^{1.2^0} + 1.2^0$$

and if $n > 1$ then $g_{(n)}(m)_{[n+1]}$ is defined recursively from $g_{(n-1)}(m)_{[n]}$ as below.

[9] *Comment*: Although we do not address the question here, it can be shown without appealing to any transfinite considerations that $G(m)$ cannot oscillate for any natural number m.

[10] But not unusually! See, for instance, [**MM01**], p.454, where the authors remark that: "We denote the least infinite ordinal by ω or N, so $\omega = N = \{0, 1, 2, \ldots\}$.".

22.8. The recursive definition of Goodstein's Sequence

For $n > 1$ let the $(n-1)^{th}$ term $g_{(n-1)}(m)$ of the Goodstein sequence $G(m)$ be expressed syntactically by its hereditary representation as:

$$g_{(n-1)}(m)_{[n]} \quad \equiv \quad \sum_{i=0}^{l} a_i.n^{i_{[n]}} \tag{22.2}$$

where:

(a) $0 \leq a_i < n$ over $0 \leq i \leq l$;

(b) $a_l \neq 0$;

(c) for each $0 \leq i \leq l$ the exponent i too is expressed syntactically by its hereditary representation $i_{[n]}$ in the base $[n]$; as also are all of its exponents and, in turn, all of their exponents, etc.

We then define the n^{th} term of $G(m)$ as:

$$g_n(m) \quad = \quad \sum_{i=0}^{l} (a_i.(n+1)^{i_{[n \hookrightarrow (n+1)]}}) - 1 \tag{22.3}$$

22.9. The hereditary representation of $g_n(m)$

Now we note that:

(a) if $a_0 \neq 0$ then the hereditary representation of $g_n(m)$ is:

$$g_n(m)_{[n+1]} \quad \equiv \quad \sum_{i=1}^{l} (a_i.(n+1)^{i_{[n \hookrightarrow (n+1)]}}) + (a_0 - 1) \tag{22.4}$$

(b) whilst if $a_i = 0$ for all $0 \leq i < k$, then the hereditary representation of $g_n(m)$ is:

$$g_n(m)_{[n+1]} \quad \equiv \quad \sum_{i=k+1}^{l} (a_i.(n+1)^{i_{[n \hookrightarrow (n+1)]}}) + c_{k\,[n+1]} \tag{22.5}$$

where:

$$\begin{aligned} c_k &= a_k.(n+1)^{k_{[n \hookrightarrow (n+1)]}} - 1 \\ &= (a_k - 1).(n+1)^{k_{[n \hookrightarrow (n+1)]}} + \left\{(n+1)^{k_{[n \hookrightarrow (n+1)]}} - 1\right\} \\ &= (a_k - 1).(n+1)^{k_{[n \hookrightarrow (n+1)]}} + n\left\{(n+1)^{k_{[n \hookrightarrow (n+1)]}-1} + (n+1)^{k_{[n \hookrightarrow (n+1)]}-2} \ldots + 1\right\} \end{aligned}$$

and so its hereditary representation in the base $(n+1)$ is given by:

$$c_{k\,[n+1]} \quad \equiv \quad (a_k - 1).(n+1)^{k_{1[n+1]}} + n\left\{(n+1)^{k_{2[n+1]}} + (n+1)^{k_{3[n+1]}} \ldots + 1\right\}$$

where $k_{1[n+1]} \equiv k_{[n \hookrightarrow (n+1)]}$ and $k_1 > k_2 > k_3 > \ldots \geq 1$.

22.10. Goodstein's argument in arithmetic

For $n > 1$ we consider the difference:

$$d_{(n-1)} = \left\{g_{(n-1)}(m)_{[n]} - g_n(m)_{[n+1]}\right\}$$

Now:

(a) if $a_0 \neq 0$ we have:

$$d_{(n-1)} = \sum_{i=0}^{l}(a_i.n^{i_{[n]}}) - \sum_{i=1}^{l}(a_i.(n+1)^{i_{[n \hookrightarrow (n+1)]}}) - (a_0 - 1) \tag{22.6}$$

(b) whilst if $a_i = 0$ for all $0 \leq i < k$ we have:

$$\begin{aligned} d_{(n-1)} &= \sum_{i=k}^{l}(a_i.n^{i_{[n]}}) - \sum_{i=(k+1)}^{l}(a_i.(n+1)^{i_{[n \hookrightarrow (n+1)]}}) - \\ & \quad (a_k - 1).(n+1)^{k_{1[n+1]}} - \\ & \quad n\left\{(n+1)^{k_{2[n+1]}} + (n+1)^{k_{3[n+1]}} \ldots + 1\right\} \end{aligned} \tag{22.7}$$

Further:

(c) if in equation 22.6 we replace the base $[n]$ by the variable $[z]$ in each term of:

$$\sum_{i=0}^{l} a_i.n^{i_{[n]}} \tag{22.8}$$

and, similarly, the base $[n+1]$ also by the variable $[z]$ in each term of:

$$\sum_{i=k+1}^{l}(a_i.(n+1)^{i_{[n \hookrightarrow (n+1)]}}) + (a_0 - 1) \tag{22.9}$$

then we have:

$$\begin{aligned} d'_{(n-1)} &= \sum_{i=0}^{l}(a_i.z^{i_{[n \hookrightarrow z]}}) - \sum_{i=1}^{l}(a_i.z^{i_{[n \hookrightarrow z]}}) - (a_0 - 1) \\ &= 1 \end{aligned} \tag{22.10}$$

since $(i_{[n \hookrightarrow (n+1)]})_{[(n+1) \hookrightarrow z]} \equiv i_{[n \hookrightarrow z]}$;

(d) whilst if in equation 22.7 we replace the bases similarly, then we have:

$$\begin{aligned} d'_{(n-1)} &= \sum_{i=k}^{l}(a_i.z^{i_{[n \hookrightarrow z]}}) - \sum_{i=(k+1)}^{l}(a_i.z^{i_{[n \hookrightarrow z]}}) - \\ & \quad (a_k - 1).z^{k_{1[(n+1) \hookrightarrow z]}} - n\left\{z^{k_{2[(n+1) \hookrightarrow z]}} + z^{k_{3[(n+1) \hookrightarrow z]}} \ldots + 1\right\} \\ &= a_k.z^{k_{[n \hookrightarrow z]}} - (a_k - 1).z^{k_{1[(n+1) \hookrightarrow z]}}) - n(z^{k_{2[(n+1) \hookrightarrow z]}} + z^{k_{3[(n+1) \hookrightarrow z]}} \ldots + 1) \end{aligned}$$

$$= z^{k_{1[(n+1) \hookrightarrow z]}} - n(z^{k_{2[(n+1) \hookrightarrow z]}} + z^{k_{3[(n+1) \hookrightarrow z]}} \ldots + 1) \tag{22.11}$$

where $k_{1[(n+1) \hookrightarrow z]} \equiv k_{[n \hookrightarrow z]}$, and $k_{1[(n+1) \hookrightarrow z]} > k_{2[(n+1) \hookrightarrow z]} > k_{3[(n+1) \hookrightarrow z]} > \ldots \geq 1$.

We consider now the sequence:

$$G(m)_{[z]} \equiv (g_1(m)_{[2 \hookrightarrow z]},\ g_2(m)_{[3 \hookrightarrow z]},\ g_3(m)_{[4 \hookrightarrow z]}, \ldots)$$

obtained from Goodstein's sequence by replacing the base $[n+1]$ in each of the terms $g_n(m)_{[n+1]}$ by the base $[z]$ for all $n \geq 1$.

Clearly if $z > n$ for all non-zero terms of the Goodstein sequence, then $d'_{(n-1)} > 0$ in each of the cases—equation 22.10 and equation 22.11—since we have in equation 22.11:

$$d'_{(n-1)} \geq (z^k - (z-1)(z^{(k-1)} + z^{(k-2)} + z^{(k-3)} + \ldots + 1)) = 1$$

The sequence $G(m)_{[z]}$ is then a descending sequence of natural numbers, and must terminate finitely in $\mathbb{N}$, if $z > n$.

Since $g_n(m)_{[(n+1) \hookrightarrow z]} \geq g_n(m)_{[n+1]}$ if $z > n$, Goodstein's sequence $G(m)$ too must terminate finitely in $\mathbb{N}$ if $z > n$.

Obviously, since we can always find a $z > n$ for all non-zero terms of the Goodstein sequence if it terminates finitely in $\mathbb{N}$, the condition that we can always find some $z > n$ for all non-zero terms of any Goodstein sequence is equivalent to the assumption that any Goodstein sequence terminates finitely in $\mathbb{N}$.

22.11. Goodstein's argument in set theory

Now the set-theoretical form of the argument due to Goodstein is essentially that:

(a) if we take the value of x in the Goodstein Functional Sequence $G_o(m_o)_{[x]}$ over the finite ordinals to be the first limit ordinal ω,

(b) and consider the—necessarily decreasing in this case—ordinal sequence (corresponding to the conditionally decreasing natural number sequence $G(m)_{[z]}$):

$$G_o(m_o)_{[\omega]} \equiv \{g_1(m_o)_{[2_o \hookrightarrow \omega]},\ g_2(m_o)_{[3_o \hookrightarrow \omega]},\ g_3(m_o)_{[4_o \hookrightarrow \omega]}, \ldots\}$$

(c) then—since, by the axioms of set theory, there are no infinitely descending ordinal sequences—the sequence $G_o(m_o)_{[\omega]}$ must terminate finitely in some putative model of ACA_0;

(d) hence—since the ordinal numbers are well-ordered, and contain a subset of ω that can be put in a 1-1 correspondence with the set of natural numbers—we need not bother to establish a proof that some natural number $z > n$, too, always exists for all non-zero terms of any Goodstein sequence over the natural numbers in the model;

(e) and, since $G(m)$ and $G_o(m_o)_{[\omega]}$ can always be put in a 1-1 correspondence meta-mathematically—where any ordinal term t_o of $G_o(m_o)_{[\omega]}$ corresponds to the natural number term t of $G(m)$—we may conclude meta-mathematically that every Goodstein sequence over the natural numbers must also terminate finitely over the structure $\mathbb{N}$ of the natural numbers.

However we note that if there is no natural number z such that $z > n$ for all non-zero terms of some Goodstein sequence, then:

(i) For *any* given n, we can find a z such that the the first n terms of the sequence $G(m)_{[z]}$ are a descending sequence of natural numbers in $\mathbb{N}$;

(ii) The sequence $G_o(m_o)_{[\omega]}$ is a finite descending sequence of ordinal numbers in $\mathbb{M}$.

The ordinal-based proof of Goodstein's Theorem is thus the postulation that since $G(m)_{[z]}$ and $G_o(m_o)_{[\omega]}$ can always be put in a 1-1 correspondence (as in (e)), the above is a contradiction from which we may conclude that there is always some natural number z such that $z > n$ for all non-zero terms of the Goodstein sequence $G(m)$!

Such a conclusion, however, ignores the cautionary remarks (§22.4) by Thoraf Skolem about unrestrictedly corresponding meta-mathematically putative mathematical entities across domains of different axiom systems.

22.12. Why Goodstein's Theorem may be vacuously true

Formally, Goodstein's ordinal-based argument is that since there are no infinitely descending sequences of ordinals, the sequence of ordinal numbers:

$$G_o(m_o)_{[\omega]} \equiv \{g_1(m_o)_{[2_o \hookrightarrow \omega]},\ g_2(m_o)_{[3_o \hookrightarrow \omega]},\ g_3(m_o)_{[4_o \hookrightarrow \omega]}, \ldots\}$$

can be shown to terminate finitely for any given finite ordinal m_o in any putative model $\mathbb{M}$ of ACA_0.

Hence the following proposition—where $g_y(X)$ denotes the y^{th} term of the Goodstein ordinal sequence $G_o(X)$—would hold in every putative model of ACA_0:

$$(\forall X)((X \in \omega) \rightarrow (\exists y)((y \in \mathbb{N}) \wedge g_y(X) = 0_o))$$

Goodstein's Theorem over the natural numbers is then the conclusion that:

$$(\exists y)(g_y(m)) = 0$$

holds for any given natural number m in the standard interpretation of the first order Peano Arithmetic PA.

However this argument would be vacuously true if ACA_0 does not have a constructively well-defined interpretation.

Moreover, it admits the possibility that Goodstein's natural number function $G(m)$ is algorithmically *verifiable*, but not algorithmically *computable*.

Part 6

Some inter-disciplinary philosophical issues

CHAPTER 23

Natural science-philosophy-mathematics

Before considering the suggested applicability of the mathematical consequences of *evidence-based* reasoning to the Physical Sciences and Quantum Mechanics (Chapters 27 to 29), Computational Complexity (Chapters 30.1 to 32), and the Theory of Numbers (Chapters 33 to 42), we briefly address some philosophical issues raised by Feferman ([**Fe99**], [**FFMS**]) and Wittgenstein ([**Wi78**]) concerning the role axioms play in formal mathematics, the perspective from within which we view 'mathematics', and the significance to be given to such a view.

For instance, let us, for the moment, make an arbitrary distinction between (compare [**Ma08**]; see also [**Fe99**]):

- The ***natural scientist's hat***, whose wearer's responsibility is recording—as precisely and as objectively as possible—our sensory observations (corresponding to computer scientist David Gamez's 'Measurement' in [**Gam18**], Fig.5.2, p.79) and their associated perceptions of a 'common' external world (corresponding to Gamez's 'C-report' in [**Gam18**], Fig.5.2, p.79; and to what some cognitive scientists, such as Lakoff and Núñez in [**LR00**], term as 'conceptual metaphors');
- The ***philosopher's hat***, whose wearer's responsibility is abstracting a coherent—albeit informal and not necessarily objective—holistic perspective of the external world from our sensory observations and their associated perceptions (corresponding to Carnap's *explicandum* in [**Ca62a**]; and to Gamez's 'C-theory' in [**Gam18**], F, p.79); and
- The ***mathematician's hat***, whose wearer's responsibility is providing the tools for adequately expressing such recordings and abstractions in a symbolic language of unambiguous communication (corresponding to Carnap's *explicatum* in [**Ca62a**]; and to Gamez's 'P-description' and 'C-description' in [**Gam18**], Fig.5.2, p.79).

  Comment: I intend the word 'symbol' in this context to mean something used for or regarded as representing something else. Thus a symbol can be a word, phrase, image, emblem, token, sign, signal (visual, aural, tactile, electrical, electromagnetic, etc.), or the like having a complex of associated meanings and perceived as having inherent value separable from that which is symbolized, as being part of that which is symbolized, and as performing its normal function of standing for or representing that which is symbolized: usually conceived as deriving its meaning chiefly from the structure in which it appears.

This distinction can also be viewed as corresponding to Rotman's semiotic description of the essence of mathematical activity, where:

- The wearer of the Natural Scientist's hat acts as an Agent who observes and records without interpretation the signifiers that correspond to conceptual metaphors of natural or experiential phenomena;
- The wearer of the Mathematician's hat acts as the Subject who provides the symbols and rules of an, ideally categorical, language for manipulating such symbolisms in terms of declarative propositions that can be unambiguously interpreted as corresponding to putative relationships between that which is sought to be signified by the Agents signifiers; and
- The wearer of the Philosopher's hat acts as the Person who provides the truth assignations (i.e., the logic in the sense of §21.2) to the propositions of the language that allow building of a *persuasive* narrative that faithfully corresponds to a description of the Agents activities.

> "Let me summarize the tripartite structure of the technology of mathematical persuasion sketched here. There are three semiotic figures. The Agent, an automaton with no capacity to imagine, who performs imaginary acts on ideal marks, on signifiers; the Subject who manipulates not signifiers but signs interpreted in terms of the Agent's activities; the Person who uses metasigns to observe and interpret the Subject's on-going engagement with signs. In terms of these agencies any piece of mathematical reasoning is organized into three simultaneous narratives. In the metaCode the underlying story organizing the proof-steps is related by the Person (the dream is told); in the Code the formal deductive correctness of these steps is worked through by the Subject (the dream is dreamed); and in what we might call the subCode the mathematical operations witnessing these steps are executed (the dream is enacted) by the Agent.
>
> It is possible, as I've shown elsewhere,[11] to use this tripartite scheme to give a unified critique of the three standard accounts—Hilbert's formalism, Brouwer's intuitionistic constructivism, Fregean Platonism—of mathematics. Briefly, the move one makes is to consider the triad of signifier, signified, Subject and show how each of the standard accounts systematically occludes one of the three elements. Thus, intuitionism, relying on a idealized mentalism, denies any but an epiphenomenal role to signifiers in the construction of mathematical objects; formalism, fixated on external marks, has no truck with meanings or signifieds of any kind; Platonism (the current orthodoxy), dedicated to discovering eternal, transhistorical truths, repudiates outright any conception of the (in fact, any humanly occupiable) Subject position in mathematics. Plainly, the valorization of a proper, formally sanctioned Code over an improper and merely supplemental metaCode deeply misperceives how mathematics traffics with signs. A misperception intrinsic to and formative of Platonism, since in order to deny the presence of persuasion within mathematical reasoning it has to understand the language of mathematics as a transparent, inert medium which manages (somehow) to express adequations between human description and heavenly truth. On the contrary, only by understanding language as constitutive of that which it "describes"—only through such a post-realist reversal of mathematical "things" and signs in which, for example, numbers are as much the result of numeral systems as numerals are the names of numbers which antedate them—can one make sense of an historically produced apparatus of persuasion and an historically conditioned account of the—human—engenderment of the numbers. But this is in the future: the history of the Subject, Agent, Person no less than the history of mathematics as a sign practice of which these semiotic agencies would be a part has yet to be written."
> ... *Rotman:* [**Rot99**]

23.1. The function of mathematics is to eliminate ambiguity

From such an *evidence-based* perspective, eliminating ambiguity in critical cases—such as communication between mechanical artefacts, or a putative communication between terrestrial and/or extra-terrestrial intelligences—would seems to be the very raison d'être of mathematical activity (but see also §14).

Such activity could, reasonably, be viewed:

(1) First, as the construction of richer and richer mathematical languages[1] that can symbolically express those of our informally expressed—i.e., in language of common discourse—abstract concepts (corresponding to Carnap's *explicandum* in §14) which can be subjectively addressed unambiguously;

   (a) By 'subjectively address unambiguously' we intend in this context that there is essentially a subjective acceptance of identity by us between an abstract concept in our mind (defined by Lakoff and Núñez as 'conceptual metaphor' in [**LR00**], p.5[2]) that we intended to express symbolically in a language, and the abstract concept created in our mind each time we subsequently attempt to understand the import of the symbolic expression (a process which can be viewed in engineering terms as analogous to formalising the specifications, i.e., Carnap's *explicatum*[3], of a proposed structure from a prototype).

and:

(2) Thereafter, the study of the ability of the mathematical languages[4] to precisely express and objectively communicate the formal expression (corresponding to Carnap's *explicatum* in §14) of such informally expressed concepts effectively.

   (a) By 'objectively communicate effectively' we intend in this context that there is essentially:

      (i) first, an objective (i.e., on the basis of *evidence-based* reasoning) acceptance of identity by another mind between the abstract concept created in the other mind when first attempting to understand the import of what we have expressed symbolically in a language, and the abstract concept created in the other

---

[1] Languages such as, for instance, the first-order Set Theory ZF, which can be well-defined formally but which have no constructively well-defined model that would admit *evidence-based* assignments of 'truth' values to set-theoretical propositions by a mechanical intelligence.

[2] Which, prima facie, may be taken to correspond to computer scientist David Gamez's definition in [**Gam18**] (Definition **D5**, p.54) of a *CC* set: A *correlate of conscious state* is a minimal set of one or more spatiotemporal structures in the physical world. This set is present when the conscious state is present and absent when the conscious state is absent. This will be referred to as a CC *set*.

[3] Which, prima facie, may be taken to correspond to Gamez's definition in [**Gam18**] (Definition **D10** and Fig.52, p.79) of a *c-theory*: A *c-theory* is a compact expression of the relationship between consciousness and the physical world. A c-theory can generate a c-description from a p-description, and generate a p-description from a c-description.

[4] Languages such as, for instance, the first order Peano Arithmetic PA, which can not only be well-defined formally but which have a finitary model (Corollary 9.8 and Corollary 9.9) that admits *evidence-based* assignments of 'truth' values to arithmetical propositions by a mechanical intelligence.

mind each time it subsequently attempts to understand the import of the symbolic expression (a process which can also be viewed in engineering terms as analogous to confirming that the formal specifications, i.e., Carnap's *explicatum*, of a proposed structure do succeed in uniquely identifying the prototype, i.e., Carnap's *explicandum*[5]); and

(ii) second, an objective acceptance of functional identity between abstract concepts that can be 'objectively communicated effectively' based on the evidence provided by a commonly accepted doctrine such as, for instance, the view that a simple functional language can be used for specifying evidence for propositions in a constructive logic ([**Mu91**]).

23.2. The truth values of information

Now, one could reasonably argue that, both qualitatively and quantitatively, any piece of information (i.e., the perceived content of a well-defined declarative sentence) that we treat as a 'fact'[6] is necessarily associated with a suitably-defined truth assignation that must fall into one or more of the following three categories:

(i) information that we *believe* to be 'true' in an absolute, Platonic, sense, and have in common with others holding similar *beliefs* as absolute, Platonic, 'truths';

(ii) information that we *hold* to be 'true'—short of Platonic *belief*—since it *can* be treated as *self-evident*, and have in common with others who also *hold* it as similarly *self-evident*;

(iii) information that we *agree* to *define* as 'true' on the basis of a *convention*, and have in common with others who accept the same *convention* for assigning *truth* values to such assertions.

Clearly the three categories of information have associated truth assignations with increasing degrees of objective accountability (i.e., accountability based on *evidence-based* reasoning) which must, in turn, influence the psyche of whoever is exposed to a particular category at a particular moment of time.

In mathematics, for instance, Platonists who hold axioms as truths in some 'absolute' Platonic sense—such as Gödel ([**Go51**]) and Saharon Shelah ([**She91**])—might be categorised as accepting all three of (i), (ii) and (iii) as definitive; those who hold axioms as reasonable hypotheses—such as Hilbert ([**Hi27**])[7]—as holding only (ii) and (iii) as definitive; and those who hold axioms as *evidence-based* propositions—such as Brouwer ([**Br13**])—as accepting only (iii) as definitive.

---

[5]Which, prima facie, may be taken to correspond to Gamez's definition in [**Gam18**] (Definition **D1**, p.26) of a *state of consciousness*: *Consciousness* is another name for *bubbles of experience*. A state of a consciousness is a state of a bubble of experience. Consciousness includes all of the properties that were removed from the physical world as scientists developed our modern invisible explanations.

[6]For the purposes of this investigation, we ignore the nuances involved in such a concept as detailed, for instance, in [**SP10**].

[7]And Huzurbazar as cited in §C.2

23.3. The value of contradiction

In the first case (i), it is obvious that contradictions between two intelligences, that arise solely on the basis of conflicting beliefs—baptised in current lexicon as 'alternative facts'—cannot yield any productive insight on the nature of the contradiction.

Although not obvious, it is the second case (ii)—of contradictions between two intelligences that arise on the basis of conflicting 'reasonability'—which yields the most productive insight on the nature of contradiction; since it compels us to address the element of a possibly implicit subjectivity underlying the contradiction that motivates us to seek (iii).

The third case (iii) is thus the holy grail of communication—one that admits unambiguous and effective communication without contradiction.

The question arises:

Query 23.1. Is there a universal language that admits unambiguous and effective communication without contradiction?

It may be pertinent to note here that some limitations on the efficacy of such a foundationalist perspective—in this case of 'information' and 'communication'—which may need to be kept in mind when addressing Query 23.1, are highlighted by Gila Sher:

> "It is inherent in the foundationalist method, many of its adherents would say, that the foundation of the basic units is different in kind from that of the other units. The former utilizes no knowledge-based resources, and in this sense it is free-standing - a foundation "for free", so to speak. Three contenders for a free-standing foundation of logic are: (a) pure intuition, (b) common-sense obviousness, and (c) conventionality. All, however, are highly problematic. From the familiar problems concerning Platonism to the fallibility of "obviousness" and the possibility of introducing error through conventions, it is highly questionable whether these contenders are viable."
> *... Sher:* [**Shr13**]*, p.151.*

23.4. Is there a universal language that admits unambiguous and effective communication?

Now, the issue of whether, or not, there is a universal logic capable of admitting effective, and unambiguous, communication is intimately linked with the question of whether Aristotle's logic of predicates can be validly applied to infinite domains. This issue lies at the heart of the 'constructivity' debate that seeks to distinguish the computer sciences from other mathematical disciplines.

In this investigation we briefly speculate on how the issue might be addressed, for instance, from the perspective of seekers of extra-terrestrial intelligence who may, conceivably, be faced with a situation where a lay person—whose financial support is sought for SETI—may reasonably require a reassuring response to the question:

Query 23.2. Is there a rational danger to humankind in actively seeking an extra-terrestrial intelligence?

The broader significance of this question was addressed in an informal article written in September 2006 by scientist David Brin, who feared that 'SETI has taken a worrisome turn into dangerous territory', and noted that:

> "...In *The Third Chimpanzee*, Jared Diamond offers an essay on the risks of attempting to contact ETIs, based on the history of what happened on Earth whenever more advanced civilizations encountered less advanced ones ...or indeed, when the same thing happens during contact between species that evolved in differing ecosystems. The results are often not good: in inter-human relations slavery, colonialism, etc. Among contacting species: extinction."
>
> *... Brin: http://lifeboat.com/ex/shouting.at.the.cosmos*

We shall restrict ourselves to briefly considering only one aspect of this complex issue:

Query 23.3. Is fear of actively seeking an ETI merely paranoia, or does it have a rational component?

23.5. Can contacting an extra-terrestrial intelligence be perilous?

Shorn of paranoiac overtones, this fear can be expressed as the query:

Query 23.4. Can we responsibly seek communication with an extra-terrestrial intelligence actively (as in the 1974 Aricebo message) or is there a logically sound possibility that we may be initiating a process which could imperil humankind at a future date?

To place the issue in a debatable perspective, we need to make some reasonable assumptions. For instance, we may reasonably assume that:

Premise 23.5. Any communication with an extra-terrestrial intelligence will involve periods of upto thousands of years between the sending of a message and receipt of a response.

Premise 23.6. We can only communicate with an essentially different form of extra-terrestrial intelligence in a platform-independent language of a mechanistically reasoning artificial intelligence.

Premise 23.7. Nature is not malicious and so, for an ETI to be malevolent towards us, they must perceive us as an essentially different form of intelligence that threatens their survival merely on the basis of our communications.

23.6. Recursive Arithmetic: The language of algorithms

Moreover, prima facie, it might seem reasonable to assume that:

Premise 23.8. The language of algorithmically computable functions and relations is platform-independent.

This is the algorithm-based machine-language defined by Gödel's recursive arithmetic ([**Go31**]), by Church's lambda calculus ([**Ch36**]), by Turing's computing machines ([**Tu36**]), and by Markov's theory of algorithms ([**Mar54**]).

As Mandelbrot has shown ([**Mn77**]), the language appears sufficiently rich to model a number of complex natural phenomena observed by us ([**Bar88**], [**BPS88**], [**PR86**]), which earlier appeared intractable.

To simplify the issue within reason, we may thus assume that:

PREMISE 23.9. All natural phenomena which are observable by human intelligence, and which can be modelled by deterministic algorithms, are interpretable isomorphically by an extra-terrestrial intelligence.

However, it is also reasonable to assume that:

PREMISE 23.10. There are innumerable, distinctly different, observable natural phenomena.

In other words, the language of deterministic algorithms must admit—and require—denumerable primitive symbols for expressing natural phenomena.

Now, an extra-terrestrial intelligence which observes natural phenomena under an interpretation that—although structurally isomorphic to ours—uses different means of observation, may not be able to recognise any of our symbolisms effectively. Hence:

PREMISE 23.11. A language of deterministic algorithms with a denumerable alphabet does not admit effective communication with an ETI.

23.7. PA—A universal language of arithmetic

Now, in his remarkable 1931 paper, Gödel showed that ([**Go31**], p.29, Theorem VII[8]):

LEMMA 23.12. *Every deterministic algorithm can be formally expressed by some formula of a first-order Peano Arithmetic, PA.*

PA is thus a good candidate for a language of unambiguous and effective communication without contradiction because it has a finite alphabet with finitary rules for:

(i) the formation of well-formed formulas;
(ii) deciding whether a given formula is a well-formed formula;
(iii) deciding whether a given formula is an axiom;
(iv) deciding whether a finite sequence of formulas is a valid deduction/proof sequence;
(v) deciding whether a formula is a consequence of the axioms (a theorem).

23.8. How we currently interpret PA

Currently our classically accepted 'standard' interpretation of PA is the one—over the structure N of the natural numbers—where the logical constants have their 'usual' interpretations in classical predicate logic, and:

(a) the set of non-negative integers is the domain;
(b) the integer 0 is the interpretation of the symbol [0];

[8] "Every recursive relation is arithmetical".

(c) the successor operation (addition of 1) is the interpretation of the [S] function;

(d) ordinary addition and multiplication are the interpretations of [+] and [*];

(e) the interpretation of the predicate letter [=] is the identity relation;

(f) the propositions of PA are interpreted as true or false by Tarski's inductive definitions of the 'satisfaction' and 'truth' of the formulas of a formal language under an interpretation.

23.9. Can PA admit contradiction?

Now, the case against accepting PA as a language of unambiguous and effective communication without contradiction appeals to Gödel's 1931 argument (in [**Go31**]) from which he concluded that:

- There is an 'undecidable' proposition in Peano Arithmetic;
- Two interpretations of which can, in principle, logically yield conflicting conclusions.

Since our current understanding of classical logic admits Gödel's conclusions, it can be argued that we must also then admit that there can be no language of unambiguous and effective communication without contradiction.

Moreover, it would then be unreasonable to seek further the source of contradictions that reflect conflicting interpretations; and, reasonably, one ought instead to pursue methods that would allow practical accommodation, rather than theoretical resolution, of such contradictions.

23.10. Does PA lend itself to essentially different interpretations?

So, the question is:

Query 23.13. Does PA really lend itself to essentially different—or even any—finitary interpretations?

This question of whether there is a PA formula which can interpret as false under a non-standard interpretation of PA, but true under its standard interpretation $\boldsymbol{M}$ (as defined in §A, Appendix A), is—almost universally—believed to have been settled in the affirmative by Gödel in his seminal 1931 paper on formally 'undecidable' arithmetical propositions.

However, we show in §20 that—and why—this belief is misleading, and that we need to read the fine print of Gödels argument carefully to see why this belief is founded on an untenable assumption, whose roots lie in the unjustified extrapolation of Aristotle's particularisation to infinite domains.

Moreover, as we show in §11.4, Corollary 11.1, any two mechanical intelligences will interpret the satisfaction, and truth, of the formulas of PA under a constructively well-defined interpretation of PA in precisely the same way without contradiction.

23.11. How does the human brain address contradictions?

We further note that whilst human intelligence (and, presumably, other organic intelligences) can accommodate algorithmically *computable* truths which do not admit contradiction, it can also accommodate algorithmically *verifiable*, but not algorithmically *computable*, truths that admit contradictory statements without inviting inconsistency *until* it can be factually determined (by events that lie outside the database of the reasoning at any moment[9]) which of the two statements is to be treated as consistent with, and added to, the existing set of algorithmically *verifiable* truths, and which is not.

Reason: It follows from Theorem 5.4 that we cannot conclude finitarily from Tarski's definitions (Definitions 6.1 to 6.6 in §6) whether or not a quantified PA formula $[(\forall x_i)R]$ is algorithmically *verifiable* as true under the classical 'standard' interpretation $\boldsymbol{M}$ of the first-order Peano Arithmetic PA if $[R]$ is algorithmically *verifiable* but not algorithmically *computable* under interpretation.

The significance of this is reflected in the case of quantum phenomena whose values can be consistently viewed as representable mathematically only by functions that are algorithmically *verifiable*, but not algorithmically *computable*.

For instance (see §29.14), concerning Erwin Schrödinger's famous poser in [**Sc35**] regarding the state of a putative cat in a closed system containing a potentially lethal radio-active element, the two contradictory statements: 'The cat is alive' and 'The cat is dead', are both consistent with any first-order formulation of the laws of quantum mechanics that admits a representation of the state of the cat at any moment before the system it seeks to represent is opened to examination. Thereafter, only one of the two statements can be assigned the truth value 'true'.

More than anything, this illustrates that all genuine contradictions—i.e., those which do not reflect contradictions in existing truth assignations—imply only a lack of sufficient knowledge (as argued by Einstein, Podolsky and Rosen in [**EPR35**]) within a system for assigning a truth assignment consistently.

The question to be addressed therefore may be whether a brain (human or mechanical) does by design, and if so how and to what extent, naturally seek to test any new 'truth' assignment to an emerging belief (or observation) for consistency with its existing set of 'truth' assignments; and how any such activity is (or can be) weakened or strengthened by time and circumstance.

In other words, the challenge for the physical sciences may be to recognise—and accept from an algorithmically *verifiable* perspective—that, in some 'emergent' sense, "at each level of complexity entirely new properties appear", as articulated by physicist Philip W. Anderson:

> The reductionist hypothesis may still be a topic for controversy among philosophers, but among the great majority of active scientists I think it is accepted without question. The workings of our minds and bodies, and of all the animate and inanimate matter of which we have any detailed knowledge, are assumed to be controlled by the same set of fundamental laws, which except under certain extreme conditions we feel we know pretty well.
>
> It seems inevitable to go on uncritically to what appears at first sight to be an obvious corollary of reductionism: that if everything obeys the same fundamental laws, then the only scientists who are studying anything really fundamental are those who are working on those laws. In practice, that amounts to some astrophysicists, some elementary particle physicists, some

[9]Such as, for example, under the *weak* classical 'standard' interpretation of the first-order Peano Arithmetic PA defined in Chapter Chapter 7.

logicians and other mathematicians, and few others. This point of view, which it [is] the main purpose of this article to oppose, is expressed in a rather well-known passage by Weisskopf (1):

> 'Looking at the development of science in the Twentieth Century one can distinguish two trends, which I will call "intensive" and "extensive" research, lacking a better terminology. In short: intensive research goes for the fundamental laws, extensive research goes for the explanation of phenomena in terms of known fundamental laws. As always, distinctions of this kind are not unambiguous, but they are clear in most cases. Solid state physics, plasma physics, and perhaps biology are extensive. High energy physics and a good part of nuclear physics are intensive. There is always much less intensive research going on than extensive. Once new fundamental laws are discovered, a large and ever increasing activity begins in order to apply the discoveries to hitherto unexplained phenomena. Thus, there are two dimensions to basic research. The frontier of science extends all along the a long line from the newest and most modern intensive research, over the extensive research recently spawned by by the intensive research of yesterday, to the broad and well developed web of extensive research activities based on intensive research of past decades.'

The effectiveness of this message may be indicated by the fact that I heard it quoted recently by a leader in the field of materials science, who urged the participants at a meeting dedicated to "fundamental problems in condensed physics" to accept that there were few or no such problems and that nothing was left but extensive science, which he seemed to equate with engineering.

The main fallacy in this kind of thinking is that the reductionist hypothesis does not by any means imply a "constructivist" one: The ability to reduce everything to simple fundamental laws does not imply the ability to start from those laws and reconstruct the universe. In fact, the more the elementary particle physicists tell us about the nature of the fundamental laws, the less relevance they seem to have to the very real problems of the rest of science, much less to those of society.

The constructionist hypothesis breaks down when confronted with the twin difficulties of scale and complexity. The behaviour of large and complex aggregates of elementary particles, it turns out, is not to be understood in terms of a simple extrapolation of the properties of a few particles. Instead, at each level of complexity entirely new properties appear, and the understanding of the new behaviours requires research which I think is as fundamental in its nature as any other. ..."

... *Anderson:* [**And72**].

23.12. The bias problem in science

Confronting such a challenge meaningfully, according to theoretical physicist Sabine Hossenfelder, requires first recognising the existence of, and then addressing and redressing, the problem of ingrained biases in scientific discourse:

> "Probably the most prevalent brain bug in science is confirmation bias. If you search the literature for support for your argument, there it is. If you look for a mistake because your result didn't match your expectations, there it is. If you avoid the person asking nagging questions, there it is. Confirmation bias is also the reason we almost end up preaching to the choir when we lay out the benefits of basic research. You knew that without discovering fundamentally new laws of nature, innovation would eventually run dry, didn't you?

> [...]
>
> There's also the false consensus effect: we tend to overestimate how many other people agree with us and how much they do so. And one of the most problematic distortions in science is that we consider a fact to be more likely the more often we have heard of it; this is called attentional bias or the mere exposure effect. We pay more attention to information especially when it is repeated by others in our community. This communal reinforcement can turn scientific communities into echo chambers in which researchers repeat their arguments back to each other over and over again, constantly reassuring themselves they are doing the right thing.
>
> Then there is the mother of biases, the blind spot—the insistence that we certainly are not biased. It's the reason my colleagues only laugh when I tell them biases are a problem, and why they dismiss my "social arguments," believing they are not relevant to scientific discourse. But the existence of these biases has been confirmed in countless studies. And there is no indication whatsoever that intelligence protects against them; research studies have found no links between cognitive ability and thinking biases.[17]
>
> Of course, it's not only theoretical physicists who have cognitive biases. You can see these problems in all areas of science. We're not able to abandon research directions that turn out to be fruitless; we're bad at integrating new information; we don't criticize our colleagues' ideas because we are afraid of becoming "socially undesirable." We disregard ideas that are out of the mainstream because these come from people "not like us." We play along in a system that infringes on our intellectual independence because everybody doe it. And we insist that our behavior is good scientific conduct, based purely on unbiased judgement, because we cannot possibly be influenced by social and psychological effects, no matter how well established.
>
> We've always had cognitive and social biases, of course. They are the reason scientists today use institutionalized methods to enhance objectivity, including peer review, measures for statistical significance, and guidelines for good scientific conduct. And science has progressed just fine, so why should we start paying attention now? (By the way, that's called the status quo bias.)
>
> Larger groups are less effective at sharing relevant information. Moreover, the more specialized a group is, the more likely its members are to hear only what supports their point of view. This is why understanding knowledge transfer in scientific networks is so much more important today than it was a century ago, or even two decades ago. And objective argumentation becomes more relevant the more we rely on logical reasoning detached from experimental guidance."
>
> *... Sabine Hossenfelder:* [**Hos18a**]*, pp.230-232.*

As our analysis of the dogmas that, from the *evidence-based* perspective of this investigation, we have labelled as Hilbert's theism and Brouwer's atheism in Chapter 3 illustrates, such biases can, sometimes, act as invisible barriers to the broadening of a perspective as may be needed to accommodate embarrassing data or seemingly incontrovertible arguments.

For instance, the roots of *all* the ambiguities sought to be addressed in this investigation can be seen to lie in the unquestioned, and *untenable* (Corollary 15.11) assumption that Aristotle's particularisation is valid over infinite domains.

Aristotle's particularisation is defined (Definition 3.1) as the postulation that, in any formal language L which subsumes the first-order logic FOL, the L-formula '$[\neg(\forall x)\neg F(x)]$—also denoted by $[(\exists x)F(x)]$—is provable in L' can *unrestrictedly* be interpreted as the assertion 'There exists an *unspecified* object a such that $F'(a)$ is true under any well-defined interpretation I of L', where $F'(x)$ is the interpretation of $[F(x)]$ under I.

Following Hilbert's formalisation of it in terms of his ε-operator in [**Hi25**], the assumption—as noted in §3.1 (footnote #3)—has been subsequently sanctified by prevailing wisdom in published literature and textbooks at such an early stage of any classical mathematical curriculum, and planted as a bias so deeply into students' minds, that thereafter most cannot even detect its presence—let alone need for its justification—in a proof sequence!

Similarly Brouwer's rejection of the Law of the Excluded Middle LEM—and ipso facto of the first order logic FOL, of which it is a theorem—as non-constructive, in the mistaken belief that LEM entails Aristotle's particularisation, resulted in as enduring—and as untenable—a bias that has constrained the development of a more encompassing, *evidence-based*, development of finitary mathematics.

It would not be unreasonable to conclude that such sub-conscious assumptions, especially where provably invalid (see, for instance, Corollary 15.11, and Corollary 9.11), has continued for over ninety years to unconsciously dictate, mislead, and so limit the perspective of not only active, but also emerging, scientists of any ilk who have depended upon classical mathematics for providing a language of adequate representation and effective communication for their abstract concepts.

CHAPTER 24

The paradoxes

We briefly consider, from an *evidence-based* perspective, the significance for the physical sciences of the semantic and logical paradoxes[1] which involve—either implicitly or explicitly—quantification over an infinitude.

Where such quantification is not, or cannot be, explicitly defined in formal logical terms—e.g., the classical expression of the Liar paradox as 'This sentence is a lie'[2]—the paradoxes per se cannot be considered as posing serious linguistic or philosophical concerns from an *evidence-based* perspective of constructive mathematics.

The practical significance of the semantic and logical paradoxes is, of course, that they illustrate the absurd extent to which languages of common discourse need to tolerate ambiguity; both for ease of expression and for practical—even if not theoretically unambiguous and effective—communication in non-critical cases amongst intelligences capable of a lingua franca.

Such absurdity is highlighted by the universal appreciation of Charles Dickens' Mr. Bumble's retort that 'The law is an ass'; a quote oft used to refer to the absurdities which sometimes surface[3] in cases when judicial pronouncements attempt to resolve an ambiguity by subjective fiat that appeals to the powers—and duties—bestowed upon the judicial authority for the practical resolution of precisely such an ambiguity, even when the ambiguity may be theoretically irresolvable!

In a thought-provoking Opinion piece, 'Desperately Seeking Mathematical Truth', in the August 2008 Notices of the American Mathematical Society, Melvyn B. Nathanson seeks to highlight the significance for the mathematical sciences when similar authority is vested by society—albeit tacitly—upon academic 'bosses' (a reference, presumably, to the collective of reputed—and respected—experts in any field of human endeavour):

> ' ... many great and important theorems don't actually have proofs. They have sketches of proofs, outlines of arguments, hints and intuitions that were obvious to the author (at least, at the time of writing) and that, hopefully, are understood and believed by some part of the mathematical community.
>
> But the community itself is tiny. In most fields of mathematics there are few experts. Indeed, there are very few active research mathematicians in the world, and many important problems, so the ratio of the number of mathematicians to the number of problems is small. In every field, there are

[1]Although commonly referred to as the paradoxes of 'self-reference', not all of them involve self-reference (e.g., the paradox constructed by Stephen Yablo [**Ya93**]).

[2]Or Lundgren's 'information liar paradox': "This is not semantic information", in [**Lun17**], §3, p.5.

[3]See www.shazbot.com/lawass/.

> "bosses" who proclaim the correctness or incorrectness of a new result, and its importance or unimportance.
>
> Sometimes they disagree, like gang leaders fighting over turf. In any case, there is a web of semi-proved theorems throughout mathematics. Our knowledge of the truth of a theorem depends on the correctness of its proof and on the correctness of all of the theorems used in its proof. It is a shaky foundation.'
>
> ... *Nathanson:* [**Na08**].

Nathanson's comments are intriguing, because addressing such ambiguity in critical cases—such as communication between mechanical artefacts, or a putative communication between terrestrial and extra-terrestrial intelligences—is the very raison d'être of mathematical activity!

Of course, it *would* be a matter of serious concern if the word 'This' in the English language sentence, 'This sentence is a lie', could be validly viewed as implicitly implying that:

(i) there is a constructive infinite enumeration of English language sentences;

(ii) to each of which a truth-value can be constructively assigned by the rules of a two-valued logic; and,

(iii) in which 'This' refers uniquely to a particular sentence in the enumeration.

In 1931, Kurt Gödel used the above perspective in his seminal paper on 'undecidable' arithmetical propositions:

(a) to show how the infinitude of formulas, in a formally defined Peano Arithmetic P ([**Go31**], pp.9-13), could be constructively enumerated and referenced uniquely by natural numbers ([**Go31**], p.13-14);

(b) to show how P-provability values could be constructively assigned to P-formulas by the rules of a two-valued logic ([**Go31**], p.13); and,

(c) to construct a P-formula which interprets as an arithmetical proposition that could, debatably (see §17.5), be viewed—under the standard interpretation of the Peano Arithmetic P—as expressing the sentence, 'This P-sentence is P-unprovable' ([**Go31**], p.37, footnote 67), *without* inviting a 'Liar' type of contradiction.

We note that where the quantification can be made explicit—e.g., Russells paradox or Yablos paradox—the significance of the question whether such quantication is constructive or not is immediately obvious.

> **Russell's paradox**: Define the set S by $\{All\ x : x \in S \text{ iff } x \notin x\}$; then $S \in S$ iff $S \notin S$.
>
> **Yablo's paradox**: Defining the sentence S_i for all $i \geq 0$ as 'For all $j > i$, S_j is not true' seems to lead to a contradiction ([**Ya93**]).

For instance, in Russell's case it could be cogently argued from an *evidence-based* perspective that the contradiction itself establishes that S cannot be constructively defined over the range of the quantifier.

In Yablo's case it could, as cogently, be argued that truth values cannot be constructively assigned to any sentence covered by the quantification since, in order to decide whether or not S_i can be assigned the value 'true' for any given $i \geq 0$, we first need to decide whether or not S_{i+1} has already been assigned the value 'true'!

There are two issues involved here—not necessarily independent—highlighted by Timothy Gowers as follows:

> "If you ask a philosopher what the main problems are in the philosophy of mathematics, then the following two are likely to come up: what is the status of mathematical truth, and what is the nature of mathematical objects? That is, what gives mathematical statements their aura of infallibility, and what on earth are these statements about?"
> . . . *Gowers:* [**Gow02**].

The first issue is whether the currently accepted interpretations of formal quantification—essentially as defined by David Hilbert ([**Hi27**]; see also §4.1) in his formalisation of Aristotle's logic of predicates in terms of his ε-function—can be treated as constructive over an infinite domain.

24.1. Is quantification currently interpreted constructively?

L. E. J. Brouwer ([**Br08**]) emphatically—and justifiably so far as number theory was concerned (see §4.2)—objected to such subjectivity, and asserted that Hilbert's interpretations of formal quantification were non-constructive.

Although Hilbert's formalisation of the quantifiers (an integral part of his formalisation of Aristotle's logic of predicates) appeared adequate, Brouwer rejected Hilbert's interpretations of them on the grounds that the interpretations were open to ambiguity, and could not, therefore, be accepted as admitting effective communication.

However, Brouwer's rejection of the Law of the Excluded Middle as a resolution of the objection was seen—also justifiably (see §14.1)—as unconvincingly rejecting a comfortable interpretation that—despite its Platonic overtones—appeared intuitively plausible to the larger body of academics that was increasingly attracted to, and influenced by, the remarkably expressive powers provided by Cantor-inspired set theories such as ZF.

Since Brouwer's seminal work preceded that of Alan Turing, it was unable to offer his critics an alternative—and intuitively convincing—constructive definition of quantification based on the view—gaining currency today—that a simple functional language can be used for specifying *evidence* for propositions in a constructive logic ([**Mu91**]).

Moreover, since Brouwer's objections did not gain much currency amongst mainstream logicians, they were unable to influence Turing who, it is our contention, could easily have provided the necessary constructive interpretations (introduced in [**An12**]) sought by Hilbert for number theory, had Turing not been influenced by Gödel's powerful presentation—and Gödel's persuasive Platonic, albeit (*contrary to accepted dogma*) logically rooted[4], interpretation of his own formal reasoning in [**Go31**].

[4] *Comment*: Although meriting a more complete discussion than is appropriate to the intent of this paper, it is worth noting that the rooting of Gödel's Platonism can be cogently argued as lying—contrary to generally held opinions—purely in a logical, rather than philosophical, presumption: more specifically in Gödel's belief that Peano Arithmetic is ω-consistent ([**Go31**], p.28). The belief seems unwittingly shared universally even by those who (cf. [**Pas95**], [**Fe02**])

Thus, in his 1939 paper ([**Tu39**]) on ordinal-based logics, Turing applied his computational method—which he had developed in his 1936 paper ([**Tu36**])—in seeking partial completeness in interpretations of Cantor's ordinal arithmetic (as defined in a set theory such as ZF)—rather than in seeking a categorical interpretation of PA. Turing perhaps viewed his 1936 paper as complementing and extending Gödel's and Cantor's reasoning.

For instance, Turing remarked that:

> "The well-known theorem of Gödel shows that every system of logic is in a certain sense incomplete, but at the same time it indicates means whereby from a system L of logic a more complete system L' may be obtained. By repeating the process we get a sequence of $L, L_1 = L', L_2 = L'_1, \ldots$ each more complete than the preceding. ...
>
> Proceeding in this way we can associate a system of logic with any constructive ordinal. It may be asked whether a sequence of logics of this kind is complete in the sense that to any problem A there corresponds an ordinal α such that A is solvable by means of the logic L_α. I propose to investigate this question in a more general case, and to give some other examples of ways in which systems of logic may be associated with constructive ordinals".
>
> ... *Turing:* [**Tu39**], *pp.155-156*.

Perhaps Turing did not see any reason to question either the validity of Gödel's belief that systems of Arithmetic such as PA are ω-consistent (as hinted at in [**Go31**], p.28), or Gödel's interpretation of his argument in [**Go31**] as having meta-mathematically *proven* that systems of Arithmetic such as PA are essentially incomplete!

It is our contention that Turing thus overlooked the fact that his 1936 paper ([**Tu36**]) actually conflicts with Gödel's and Cantor's interpretations of their own, formal, reasoning by admitting an objective definition of satisfaction that yields a sound, finitary, interpretation $\boldsymbol{B}$ of PA (see §9).

Specifically, whereas Gödel's and Cantor's reasoning implicitly presumes that satisfaction under the standard interpretation $\boldsymbol{M}$ of PA can only be defined non-constructively in terms of subjectively verifiable truth (reflecting the view that Tarski's Theorem—see [**Me64**], p.151—establishes the formal undefinability of arithmetical truth in arithmetic), it can be cogently argued that satisfaction under both $\boldsymbol{M}$ and $\boldsymbol{B}$ is definable constructively in terms of objectively verifiable Turing-computability (see §5.1).

As a result, conventional wisdom continues to essentially follow Hilbert's Platonically-influenced (hence, subjective) definitions and interpretations of the quantifiers (based on accepting Aristotle's particularisation as valid) when defining them under the standard interpretation $\boldsymbol{M}$ of PA.

Now, the latter definitions and interpretations (e.g., [**Me64**], pp.49-53) are, in turn, founded upon Tarski's analysis of the inductive definability of the truth of compound expressions of a symbolic language under an interpretation in terms of the satisfaction of the atomic expressions of the language under the interpretation ([**Ta35**]).

---

accept Gödel's formal arguments in [**Go31**] but claim to reject Gödel's 'Platonic' interpretations of them.

Tarski defines there the formal sentence P as True if and only if p—where p is the proposition expressed by P. In other words, the sentence 'Snow is white' is True if, and only if, it is *subjectively* true in all cases; and it is *subjectively* true in a particular case if, and only if, it expresses the *subjectively verifiable* fact that snow *is* white in that particular case. Thus, for Tarski the commonality of the satisfaction of the atomic formulas of a language under an interpretation is axiomatic (cf. [**Me64**], p.51(i)).

In this investigation we have highlighted the limitations of such subjectivity (in Chapters 5 and 6) and, in the case of the 'standard' interpretation $\boldsymbol{M}$ of the Peano Arithmetic PA, shown how to avoid violation of such constraints (in Chapter 7) by requiring that the axioms of PA, and its rules of inference, be interpretable as algorithmically (and, ipso facto, objectively) verifiable propositions.

24.2. When is the concept of a completed infinity consistent?

The second issue is when, and whether, the concept of a completed infinity is consistent with the interpretation of a formal language.

Clearly, the consistency of the concept would follow immediately in any constructively well-defined interpretation of the axioms (and rules of inference) of a set theory such as the Zermelo-Fraenkel ([**BF58**]) first-order theory ZF (whether such an interpretation exists at all is, of course, another question).

In view of the perceived power of ZFC as an unsurpassed language of rich and adequate expression of mathematically expressible abstract concepts precisely (see Thesis 44.1), it is not surprising that many of the semantic and logical paradoxes depend on the implicit assumption that the domain over which the paradox quantifies can always be treated as a well-defined mathematical object that can be formalised in ZFC, even if this domain is not explicitly defined set-theoretically.

This assumption is rooted in the questionable[5] belief that ZF can express all mathematical 'truths' (see, for instance, [**Ma18**] and [**Ma18a**]).

From this it is but a short step to non-constructive perspectives—such as Gödel's Platonic interpretation of his own formal reasoning in his 1931 paper ([**Go31**])—which argue (see §20.1) that PA must have non-standard models.

However, it is our contention that both of the above foundational issues need to be reviewed carefully, and that we need to recognize explicitly the limitations on the ability of highly expressive mathematical languages such as ZF to communicate effectively; and the limitations on the ability of effectively communicating mathematical languages such as PA to adequately express abstract concepts—such as those involving Cantor's first limit ordinal ω (see §20.7).

Prima facie, the semantic and logical paradoxes—as also the seeming paradoxes associated with 'fractal' constructions such as the Cantor ternary set, and the constructions described below—seem to arise out of a blurring of this distinction, and an attempt to ask of a language more than it is designed to deliver.

[5]'Questionable' since, in Chapter 22, we show how—in the case of Goodstein's Theorem—such a belief leads to a curious conclusion (Theorem 22.3).

24.3. Asking more of a language than it is designed to deliver

For instance, consider the claim (e.g., [**Bar88**], p.37, Theorem 1) that fractal 'constructions'—such as the Cantor ternary set, which is defined classically as a 'putative' set-theoretical limit ([**Ru53**], p34; [**Bar88**], pp.44-45) of an iterative process in the 'putative' completion of a metric space—yield valid mathematical objects (sets) in the 'limit' (*presumably in some Platonic mathematical model*).

Now, the Cantor Set T_∞ is defined as the putative 'fractal' limit of the set of points obtained by taking the closed interval $T_0 = [0, 1]$):

- removing the open middle third to yield the set $T_1 = \{[0, \frac{1}{3}] \cup [\frac{2}{3}, 1]\}$,
- then removing the middle third of each of the remaining closed intervals to yield the set $T_2 = [0, \frac{1}{9}] \cup [\frac{2}{9}, \frac{1}{3}] \cup [\frac{2}{3}, \frac{7}{9}] \cup [\frac{8}{9}, 1]$,
- and so on ad infinitum.

To see why such a limit needs to be treated as 'putative' from an *evidence-based* perspective (compare with Lakoff and Núñez's analysis of a similar 'length paradox' in [**LR00**], p.325-333), consider the equilateral triangle BAC of height h and side s in Fig.1 (below):

- Divide the base BC in half and construct two isosceles triangles of height $h.d$ and base $s/2$ on BC, where $1 \geq d > 0$.
- Iterate the construction on each constructed triangle ad infinitum.
- Thus, the height of each of the 2^n triangles on the base BC at the n'th construction is $h.d^n$, and the base of each triangle $s/2^n$.
- Hence, the total area of all these triangles subtended by the base BC is $s.h.d^n/2$.
- Now, if $d = 1$, the total area of all the constructed triangles after each iteration remains constant at $s.h/2$, although the total length of all the sides opposing the base BC increases monotonically.
- However, if $1 > d > 0$, it would appear that, geometrically, the base BC of the original equilateral triangle will always be the 'limiting' configuration of the sides opposing the base BC.

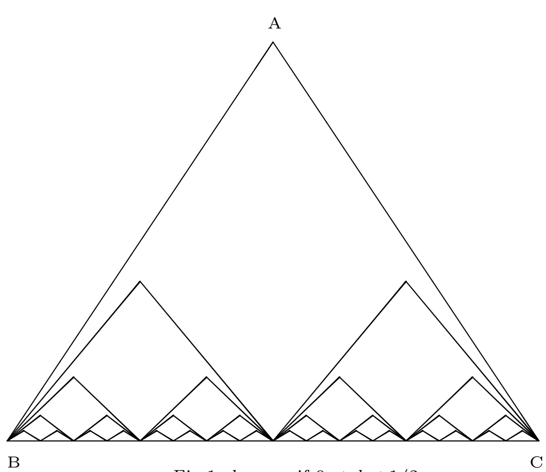

Fig.1: $l_n \rightarrow s$ if $0 < d < 1/2$

This is indeed so if $0 < d < 1/2$ (Fig.1), since the total length of all the sides opposing the base BC at the n'th iteration—say l_n—yields a Cauchy sequence whose limiting value is, indeed, the length s of the base BC.

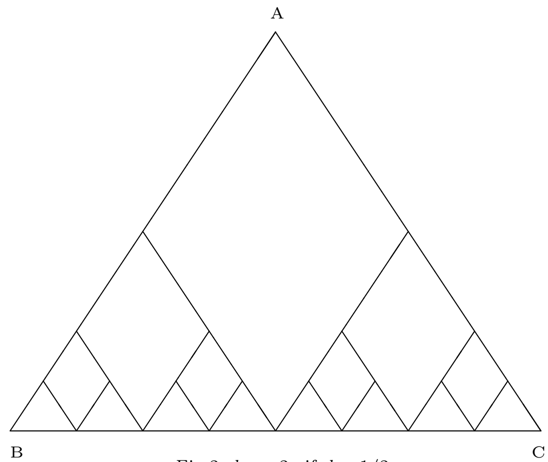

Fig.2: $l_n = 2s$ if $d = 1/2$

However, if $d = 1/2$ (Fig.2), the total length of all the sides opposing their base on BC is always $2s$; which, by definition, also yields a Cauchy sequence whose limiting value is 2!

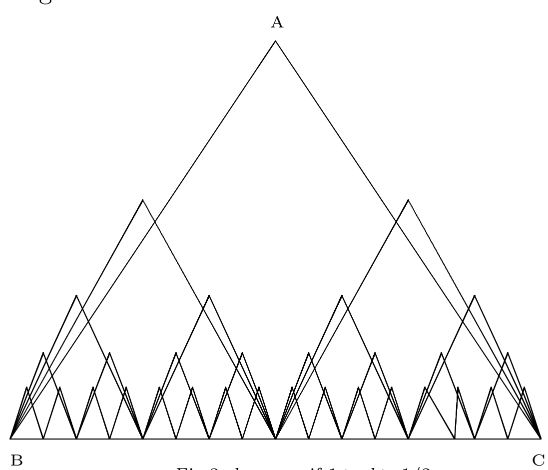

Fig.3: $l_n \to \infty$ if $1 > d > 1/2$

Finally, if $1 > d > 1/2$ (Fig. 3), the total length of all the sides opposing their base on BC is a monotonically increasing value.

Consider now:

24.4. Interpretation as a virus cluster

Case 1: Let the area BAC denote the population size of a virus cluster, where each virus cell has a 'virulence' measure h/s.

Let each triangle at the n'th iteration denote a virus cluster—with a virulence factor $h.d^n/(s/2^n)$—that reacts to the next generation anti-virus by splitting into two smaller clusters with inherited virulence $h.d^{n+1}/(s/2^{n+1})$.

We then have that:

(a) If $d < 1/2$, the effects of the virus can—in a sense—be contained and eventually 'eliminated', since both the total population of the virus, and its virulence in each cluster, decrease monotonically;

(b) If $d = 1/2$, the effects of the virus can be 'contained', but never 'eliminated' since, even though the total population of the virus decreases monotonically, its virulence in each cluster remains constant, albeit at a containable level, until the virus suffers a sudden, dinosaur-type, extinction at the 'limiting' point as $n \to \infty$;

(c) However, if $d > 1/2$, the effects of the virus can neither be 'contained' nor 'eliminated' since, even though the total population of the virus decreases monotonically, its virulence in each cluster resists containment by increasing monotonically until, again, the virus suffers a sudden, dinosaur-type, extinction at the 'limiting' point as $n \to \infty$.

24.5. Interpretation as an elastic string

Case 2: Let the base BC denote an elastic string, stretched iteratively into the above configurations.

We then have that:

(a) If $d < 1/2$, the elastic will, in principle, eventually return to its original state;

(b) If $d > 1/2$, then the elastic must break at some point, in a phase change that is apparently normal, and invites no untoward curiosity, since it forms part of our everyday experience;

(c) However, what if $d = 1/2$?

24.6. Phase change: Zeno's argument in 2-dimensions

We then arrive at a two-dimensional version of Zeno's arguments ([**Rus37**], pp.347-353); one way of resolving which is by admitting the possibility that such an elastic 'length' undergoes a 'steam-to-water-like' phase change in the 'limit' that need not correspond (see §19.4) to the putative limit of its associated Cauchy sequence[6]!

We note that Theorem 19.4 shows that Cauchy sequences which are defined as algorithmically *verifiable*, but not algorithmically *computable*, can correspond to 'essentially incompletable' real numbers whose Cauchy sequences cannot, in a sense, be known 'completely' even to Laplace's 'intellect' (such as, for instance, the fundamental dimensionless constants considered in §29.6).

The above example now show further that—and why—the numerical values of some algorithmically computable Cauchy sequences may also need to be treated as formally specifiable, first-order, non-terminating processes:

- which are 'eternal work-in-progress' in the sense of Theorem 19.4, and
- which cannot be uniquely identified by a putative 'Cauchy limit' without limiting the ability of such sequences to model phase-changing physical phenomena faithfully.

---

[6]We note that, by definition, the sequence $\{a_0, a_1, a_2, \ldots\}$ where $a_0 = 1$ and $a_i = 3$ for all $i \geq 1$, is a Cauchy sequence whose mathematical limit is 3.

In view of Theorem 19.7, the gedanken in §24.4 and §24.5 highlight the disquieting issue sought to be raised, for instance, by Krajewski in [**Kr16**][7] (see §2.2), Lakoff and Núñez in [**LR00**] (p.325-333), and Simpson in [**Sim88**], which can be expressed as:

QUERY 24.1. Since the raison d'être of a mathematical language is—or ideally should be—to express our abstractions of natural phenomena precisely, and communicate them unequivocally, in what sense can we sensibly admit an interpretation of a mathematical language that constrains all the above cases by 'limiting' configurations in a putative, set-theoretical, 'completion' of Euclidean Space?

[7] "Examples of possible theological influences upon the development of mathematics are indicated. The best known connection can be found in the realm of infinite sets treated by us as known or graspable, which constitutes a divine-like approach. Also the move to treat infinite processes as if they were one finished object that can be identified with its limits is routine in mathematicians, but refers to seemingly super-human power." ... [**Kr16**].

Part 7

The significance of *evidence-based* reasoning for some grey areas in the foundations of Cosmology

CHAPTER 25

The mythical completability of metric spaces

> "Our thoughts live in natural and artificial languages the way fish swim in natural and artificial bodies of water.
>
> One of the lessons most strikingly impressed on me by my first year physics course and the mass of collateral reading I did at the time was to guard against the errors that arise from "projecting the properties and structures of any language or symbol system on the external world". This was mentioned especially often in discussions of quantum mechanics—it was a common observation that our difficulties grasping wave-particle duality might be due to our prior conditioning to see the world through the lenses of our subject-predicate languages and logics. Soon after, I learned about the Sapir-Whorf hypothesis, and today I lump all these cautionary tales under the heading of GRAM ("Grammar Recycled As Metaphysics")."
>
> ... *Awbrey:* [**Aw18**].

From the *evidence-based* perspective of Chapter 24, we can now hypothesise:

Thesis 25.1. There are no infinite processes, i.e., nothing corresponding to infinite sequences, in natural phenomena.

Thesis 25.2. If:

(a) a physical process is representable by a Cauchy sequence as in the above cases in §24.4 and in §24.5; and

(b) we accept that there can be no infinite processes, i.e., nothing corresponding to infinite sequences, in natural phenomena;

then:

(c) in the absence of an extraneous, *evidence-based*, proof of 'closure' which determines the behaviour of the physical process in the limit as corresponding to a 'Cauchy' limit;

(d) the physical process *must* tend to a discontinuity (singularity) which has not been reflected in the Cauchy sequence that seeks to describe the behaviour of the physical process.

The *significance* of such insistence on *evidence-based* reasoning for the physical sciences is that we may then be prohibited from claiming legitimacy for a mathematical theory which seeks to represent a physical process based on the assumption that the limiting behaviour of *every* physical process which can be described by a Cauchy sequence in the theory must correspond to—and so be constrained by—the behaviour of the Cauchy limit of the corresponding sequence.

For instance the existence of Hawking radiation in cosmology is posited on the assumption that '*the consistent extension of this local thermal bath has a finite temperature at infinity*':

> "Hawking radiation is required by the Unruh effect and the equivalence principle applied to black hole horizons. Close to the event horizon of a black hole, a local observer must accelerate to keep from falling in. An accelerating obsrver sees a thermal bath of particles that pop out of the local acceleration horizon, turn around, and free-fall back in. The condition of local thermal equilibrium implies that the consistent extension of of this local thermal bath has a finite temperature at infinity, which imples that some of these particles emitted by the horizon are not reabsorbed and become outgoing Hawking radiation."
>
> . . . *https://en.wikipedia.org/wiki/Hawking_radiation.* (Accessed 04/06/2018, 08:00 IST.)

As we have demonstrated in Fig. 2 (§24.3) and §24.5, Case 2(c), *the consistent extension of the state of a stretched elastic string—as defined in Fig. 2—does not have a limiting mathematical value at infinity* which can be taken to correspond to its putatively limiting physical state.

The gedanken in §25.1 further illustrates that a mathematical singularity need not constrain a physical theory from positing a well-definable value for a limiting state of a physical process, contrary to what conventional wisdom accepts in the limiting cases of Einstein's equations for General Relativity:

> "The Big Bang is probably the most famous feature of standard cosmology. But it is also an undesirable one. That's because the classical model of the universe, described by Einstein's equations, breaks down in the conditions of the Big Bang, which include an infinite density and temperature, or what physicists call a singularity."
>
> . . . *Padmanabhan:* [**Pd17**].

Moreover, we shall argue that introduction of a, normally *weak*, anti-gravitational field whose strength can, however, accept quantum states that cause a universe to explode and implode in a predictable way at their corresponding 'mathematical' singularities, yields a mathematical model of a universe:

- That recycles endlessly from Big Bang to Ultimate Implosion;
- Which is time-reversal invariant; and
- In which the existence of 'dark energy' is intuitively unobjectionable.

Whether or not such features can be made to apply to the physical universe we inhabit is a separate issue (see [**An18**]) that lies beyond the focus of the *evidence-based* perspective of this investigation.

However, it is worthwhile noting some of the barriers that mathematical 'singularities' are perceived as imposing upon our ability to faithfully comprehend, and mathematically represent, the laws of nature.

For instance, as queried by Thanu Padmanabhan in [**Pd17a**]:

> "But what if there was no singularity? Since the 1960s, physicists have been working on describing the universe without a Big Bang by attempting to unify gravitational theory and quantum theory into something called quantum gravity. Physicists John Wheeler and Bryce deWitt were the first to apply these ideas to a hypothetical pre-geometric phase of the universe,

in which notions of space and time have not yet-emerged from some as-yet unknown structure. This heralded the study of quantum cosmology, in which physicists attempted to describe the dynamics of simple toy models of the universe in quantum language. Needless to say, several different, but related, ideas for the description of the pre-geometric phase mushroomed over the decades. The unifying theme of these models is that the classical universe arises, without any singularity, through a transition from a pre-geometric phase to one in which spacetime is described by Einstein's equations. The main difficulty in constructing such a description is that we do not have a complete theory of quantum gravity, which would allow us to model the pre-geometric phase in detail."

. . . Padmanabhan: [**Pd17**].

The issue is highlighted further by Padmanabhan in [**Pd17a**]:

"I will now raise a question which, at the outset, may sound somewhat strange. Why does the universe expand and, thereby, give us an arrow of time? To appreciate the significance of this question, recall that Eq. (9) is invariant under time reversal $t \to -t$. (After all, Einstein's equations themselves are time reversal invariant.) To match the observations, we have to choose a solution with $\dot{a} > 0$ at some fiducial time $t = t_{fid} > 0$ (say, at the current epoch), thereby breaking the time-reversal invariance of the system. This, by itself, is not an issue for a laboratory system. We know that a particular solution to the dynamical equations describing the system need not respect all the symmetries of the equations. But, for the universe, this is indeed an issue.

To see why, let us first discuss the case of $(\rho + 3p) > 0$ for all t. The choice $\dot{a} > 0$, at any instant of time, implies that we are postulating that the universe is expanding at that instant. Then Eq. (9) tells us that the universe will expand at all times in the past and will have a singularity ($a = 0$) at some finite time in the past (which we can take to be $t = 0$ without loss of generality). The structure of Eq. (9) prevents us from specifying the initial conditions at $t = 0$. So, if you insist on specifying the initial conditions and integrating the equations forward in time, you are forced to take $\dot{a} > 0$ at some time $t = \epsilon > 0$, thereby breaking the time reversal symmetry. The universe expands at present 'because' we chose it to expand at some instant in the past. This expansion, in turn, gives us an arrow of time [where] either t or a can be used as a time coordinate. But why do we have to choose the solution with $\dot{a} > 0$ at some instant? This is the essence of the so called expansion problem [6]. An alternative way of posing the same question is the following: How come a cosmological arrow of time emerges from the equations of motion which are time-reversal invariant?

In a laboratory, we can usually take another copy of the system we are studying and explore it with a time-reversal choice of initial conditions, because the time can be specified by degrees of freedom external to the system. We cannot do it for the universe because we do not have extra copies of it handy and—equally importantly—there is nothing external to it to specify the time. So the problem, as described, is specific to cosmology.

So far we assumed that $(\rho + 3p) > 0$, thereby leading to a singularity. Since meaningful theories must be nonsingular, we certainly expect a future theory of gravity—possibly a model for quantum gravity—to eliminate the singularity [effectively leading to $(\rho + 3p) < 0$. Can such a theory solve the problem of the arrow of time? This seems unlikely. To see this, let us ask what kind of dynamics we would expect in such a 'final' theory. The classical dynamics will certainly get modified at the Planck epoch, to govern the evolution of an (effective) expansion factor. The solutions could, for example, have a contracting phase (followed by a bounce) or could start from

> a Planck-size universe at $t = -\infty$, just to give two nn-singular possibilities. While we do not know these equations or their solutions, we can be confident that they will still be time-reversal invariant because quantum theory, as we know it, is time-reversal invariant.
>
> So except through a choice for initial conditions (now possibly at $t = -\infty$), we still cannot explain how the cosmological arrow of time emerges. Since quantum gravity is unlikely to produce an arrow of time, it is a worthwhile pursuit to try and understand this problem in the (semi) classical context."
>
> ... *Padmanabhan:* [**Pd17a**].

However, the arguments in §24.4 and §24.5 suggest that:

THESIS 25.3. The perceived barriers that inhibit mathematical modelling of a cyclic universe, which admits broken symmetries, dark energy, and an ever-expanding multiverse, in a mathematical language seeking unambiguous communication are illusory; they arise out of an attempt to ask of the language selected for such representation more than the language is designed to deliver.

25.1. Interpretation as the confinement state of the total energy in a universe that recycles

> "Both general relativity and Newtonian gravity appear to predict that negative mass would produce a repulsive gravitational field."
>
> ... *Anti-gravity: https://en.wikipedia.org/wiki/Anti-gravity; accessed 08/06/2018, 10:13:00.*

To illustrate why an *evidence-based* perspective towards interpreting the propositions of a mathematical model realistically would view such barriers as illusory, we consider the following gedanken.

Case 3: We can also treat Fig.2 in §24.3 as a mathematical representation of the 'confinement parameter' that determines the state of the total energy s, in a *finite* universe $\mathcal{U}$, which is subject to two constantly unequal and opposing, assumed additive, forces due to:

- A *strong* confinement field G (induced by matter), whose state is determined by a single discrete dimensionless constant, defined as an Einsteinian confinement, or gravitational strength, 'gravitational constant' (gsp), which is always $\frac{1}{2}$; and
- A *weak* anti-confinement field R (induced by anti-matter), whose state is determined by discrete dimensionless values, defined as the Einsteinian anti-confinement, or repulsive gravitational strength, 'cosmological constants' (asp), where:
  - $asp = 1 > gsp$ when $\mathcal{U}$ is in an *exploding* state *at* event e_0;
  - $asp = \frac{1}{3} + \frac{2}{3}(1 - \frac{1}{n+1}) > gsp$ when $\mathcal{U}$ is in an *imploding* state *at* event e_n for $n \geq 1$;
  - $asp = \frac{1}{3} < gsp$ when $\mathcal{U}$ is in a *steady* state:
    - during which events, denoted by $e'_n, e''_n, \ldots$,
    - occur *between* events e_n and e_{n+1};
    - where $e'_n < e_m$ is an abbreviation for 'event e'_n occurs causally before event e_m'.

and where the following are assumed to hold:

(a) Classical laws of nature determine the nature and behaviour of all those properties of the physical world that are both *determinate* and *predictable*, and are therefore mathematically describable at any event $e(n)$ by *algorithmically computable* functions from a given initial state at event $e(0)$ (Thesis 29.2);

(b) Neo-classical (quantum) laws of nature determine the nature and behaviour of those properties of the physical world that are *determinate* but *not predictable*, and are therefore mathematically describable at any event $e(n)$ only by functions that are *algorithmically verifiable* but not *algorithmically computable* from any given initial state at event $e(0)$ (Thesis 29.3);

(c) There can be no infinite processes, i.e., nothing corresponding to infinite sequences, in natural phenomena;

(d) All laws of nature are subject to *evidence-based* accountability as follows (Thesis 25.1):

- If a physical process is representable by a Cauchy sequence (as in the above cases in §24.4 and §24.5);
- then, in the absence of an extraneous, *evidence-based*, proof of 'closure' which determines the behaviour of the physical process in the limit as corresponding to a 'Cauchy' limit;
- the physical process *must* be taken to tend to a discontinuity (singularity) which has not been reflected in the Cauchy sequence that seeks to describe the behaviour of the physical process.

A: We then define:

(i) The total, say s, units of energy of the universe $\mathcal{U}$ is:

- in an *exploding* state *at* event e_0;
- in a *steady* state *between* events e_n and e_{n+1} for $n \geq 1$;
- in an *imploding* state *at* events e_n for $n \geq 1$.

(ii) The state of the anti-confinement field in $\mathcal{U}$ at an event is defined with reference to Fig.2 as follows:

- Initially at the Big Bang event e_0, where the energy s is in an unstable *exploding* state, the anti-confinement field strength:
  * is determined by the ratio $asp = \frac{s}{s} = 1 > gsp$ of the absolute value of the total energy s of the universe, and the absolute value of a confinement parameter represented by the length BC where, for convenience, we define the length BC as s;
  * which also corresponds to the limiting case of the confinement parameter as $n \rightarrow \infty$ in Fig.2.
- Between events e_n and e_{n+1} for $n > 0$, where the energy s is in a *steady* state, the anti-confinement field strength:
  * is determined by the ratio $asp = \frac{s}{l_n} = \frac{1}{3} < gsp$,

    * where the confinement parameter $l_n = 3s$ is represented by the cumulative perimeter lengths of all the triangles on their common base BC in Fig.2.

- At event e_n for $n \geq 1$, where the energy s is in an unstable *imploding* state, the anti-confinement field strength:
    * is determined by $asp = \frac{s}{l_n} + \frac{2}{3}(1 - \frac{1}{n+1}) > gsp > \frac{1}{3}$;
    * where $\frac{2}{3}(1 - \frac{1}{n+1}) > \frac{1}{3}$ is defined as the implosion constant at event e_n.

B: We further define:

(iii) At event e_0 the universe $\mathcal{U}$ explodes and expands 'instantaneously'—in a water-to-steam like phase change—to a *steady* state termed as event e'_0 where:

- The strength of the confinement field, $gsp = \frac{1}{2}$,

is now greater than:

- The strength of the anti-confinement field, $asp = \frac{s}{3s} = \frac{1}{3}$.

(iv) At any event e'_0 the total energy s of the universe $\mathcal{U}$—which we assume can neither be created nor destroyed—is subjected to a confinement field due to gravitational effects that gradually concentrates:

- some energy to form isolated matter;
- some isolated matter to form stars;
- some stars to form supernovas;
- some supernovas to form 'black holes';
- some 'black hole' to form the first 'critical black hole':
    * which we define as event e''_0 where $e''_0 \geq e_0$;
    * during which matter is gradually drawn into the 'black hole',
    * until, at event e_1, a 'critical' proportion of the total energy s of the parent universe corresponding to the state BAC has been drawn into the 'critical black hole':
        · which proportion, without loss of generality, we may take as $\frac{1}{2}$ in this example;
        · where we treat event e_1 as a singularity corresponding to the mid-point of BC;
        · such that this energy $(\frac{s}{2})$ has now been 'confined' into an imploding state with $asp = \frac{1}{3} + \frac{2}{3}(1 - \frac{1}{2}) = \frac{2}{3} > gsp$;
            - and is extinguished in an 'instantaneous' implosion, defined as the event $e_1 \geq e''_0$,
            - which forms an electromagnetically disconnected, independent, universe;

- which, without loss of generality, we treat as the splitting of the energy s of the parent universe $\mathcal{U}$ into two disconnected, isomorphic but not identical, twin sub-universes corresponding to the states $BAC_{1,1}$ and $BAC_{1,2}$ in Fig.2,

- that are situated in common, universal, confinement and anti-confinement fields G and R;

- and which, without loss of generality, we assume obey identical laws of nature;

- where the total energy s is now divided equally between the twin states $BAC_{1,1}$ and $BAC_{1,2}$;

- where, without loss of generality, we may assume that the distribution of particles and their anti-particles between the twin states $BAC_{1,2}$ and $BAC_{1,1}$ is not necessarily symmetrical.

(v) Whence it follows that:

– The total of any Hawking—or other, similarly putative[1]—energy radiated back into the 'observable' universe $\mathcal{U}$ corresponding to the state BAC during the period, defined as event e''_0, between the creation of the 'critical black hole' and its eventual extinction at event e_1 (corresponding to the mid-point of BC):

* is *not* $s/2$ (as conventional wisdom would expect in such a model);

* but, if at all, only a tiny fraction of the total energy—which is now $s/2$—of each sub-universe;

* although each sub-universe:

  · unaware of its isomorphic sibling,

  · and under the illusion that it is still the entire parent universe,

  · with merely 'black hole' concentrates of energy within it,

  · which it believes will gradually extinguish once all the energy has seeped back into its domain as a result of a putative Hawking, or similar, radiation,

  · continues to lay claim to the energy of its extinguished sibling as 'dark energy',

  · by an 'unknowably' misapplied appeal to the law of preservation of the total energy s of the original universe corresponding to the state BAC;

[1]'Putative' since the existence of such energy may be only on the basis of the debatable—see §24.6 and §25—mathematical assumption that the limit of the mathematical representations of a sequence of physical phenomena must necessarily correspond to the putative behaviour of the physical phenomena in the putative limiting state.

- Although the universe $\mathcal{U}$ is time-reversal invariant, each of the twin (isomorphic but not identical) sub-universes corresponding to the states $BAC_{1,1}$ and $BAC_{1,2}$ need not be time-reversal invariant, since the ratio of particles to their anti-particles in each of the twin sub-universes may no longer be symmetrical;
- Each sub-universe in turn forms the *next* 'critical black hole' singularity;
  * that implodes similarly at—assumed without loss of generality as a common—event e_2,
  * into two, isomorphic but electro-magnetically disconnected, twin sub-universes with equal, but asymmetrical, division of energy;
- The universe at event e_2 is a 'multiverse' of mutually disconnected 2^2 sub-universes corresponding to the states $\{BAC_{2,1}, BAC_{2,2}, BAC_{2,3}, BAC_{2,4}\}$;
  * and so on ad infinitum.

C: In other words, the n^{th} implosion at event e_n, for $n > 1$, is when the universe $\mathcal{U}$ is confined into the imploding state with a monotonically increased imploding anti-confinement strength $asp = \frac{1}{3} + \frac{2}{3}(1 - \frac{1}{n+1}) > \frac{1}{3}$; and its energy divides further—corresponding to each of the 2^n triangles $BAC_{n,i}$ on the base BC, where $1 \leq i \leq 2^n$, dividing further into two similar sub-triangles—where:

(vi) The total energy corresponding to each of the 2^n triangles *after* the event e_n is $s/2^{n-1}$ for $n > 0$;

(vii) The strength of the anti-confinement field within each sub-universe remains constant at $asp = 1/3$ between events e_n and e_{n+1}, which is below the minimum imploding $asp = \frac{2}{3}$ of event e_1.

D: We thus have a mathematical model of an exploding and then imploding universe:

(viii) That can be viewed as recycling endlessly in *either* direction of time;

(ix) Whose state—exploding, steady, or imploding—at any event e is determined by the strength of an anti-confinement field that—in the direction of time chosen in this example—regularly impels $\mathcal{U}$ to split itself into a monotonically increasing number of isomorphic, but electromagnetically disconnected, sub-universes, all situated in a common confinement/anti-confinement field:

- where the laws of nature remain unchanged;
- where, for $n > 0$, the total energy within each sub-universe at event e_n has decreased monotonically to $s/2^{n-1}$ due to persisting imploding effects of assumed gravitational/anti-gravitational forces;
- that will further split each sub-universe into two at event e_{n+1} as illustrated in Fig.2 if the strength of the anti-confinement field is in the state $1 > asp > \frac{1}{3}$;

(x) Where the energy within each sub-universe during the steady state between events e_n and e_{n+1} appears as 'dark' to its siblings:

- since it is disconnected from, and disappears forever beyond, their event-horizon at an implosion;
- and because each sub-universe, unaware of its siblings, assumes that—since energy can neither be created nor destroyed—the total energy s of the universe must remain constant within their illusory 'universe', either as visible or as 'dark' energy;
- where the distribution of matter outside the critical black hole within each sub-universe may be perceived at any instant by an observer within the sub-universe as accelerating away from the observer in an apparently expanding 'universe' whose boundary is quantified by an ever-increasing value which also tends to a discontinuity as $n \rightarrow \infty$, corresponding to the *virulence* of the virus cluster considered in §24.4, Case 1(c), Fig.3;
- where any two, isomorphic but electro-magnetically disconnected, twin sub-universes have equal, but asymmetrical, division of energy;

(xi) Where each sub-universe during the steady state between events e_n and e_{n+1} is expanding at an accelerating rate since the 'cosmological constant' $asp = \frac{1}{3} > 0$;

(xii) The energy within each sub-universe at the limiting Zeno-type phase-change point— describable mathematically as '$n \rightarrow \infty$'—implodes finally to a 'dark point' in BC;

(xiii) Where the energy within the universe as a whole experiences a steam-to-water phase-changing collapse into the original Big Bang configuration represented by an exploding anti-gravitational state $asp = 1$ denoted by BC;

- thus triggering the next cycle of its rebirth (in the chosen time direction of this example);

25.2. Conclusion

In this investigation we have argued for the plausibility of the thesis (Thesis 25.2) that if:

(a) a physical process is representable by a Cauchy sequence; and

(b) we accept that there can be no infinite processes, i.e., nothing corresponding to infinite sequences, in natural phenomena;

then:

(c) in the absence of an extraneous, *evidence-based*, proof of 'closure' which determines the behaviour of the physical process in the limit as corresponding to a 'Cauchy' limit;

(d) the physical process *must* tend to a discontinuity (singularity) which has not been reflected in the Cauchy sequence that seeks to describe the behaviour of the physical process.

We have highlighted the practical significance of our thesis for the physical sciences by defining an, *in principle verifiable*, mathematical model in Fig.2 that can be interpreted as describing the putative behaviour under a well-defined iteration of:

(i) a virus cluster; and
(ii) an elastic string.

where the physical process in each case can be 'seen' to tend to an 'ultimate' discontinuity (singularity) which has not been reflected in the Cauchy sequence that seeks to describe the behaviour of the process.

We have then highlighted the theoretical significance of our thesis for a realistic philosophy of science by showing that Fig.2 can also be interpreted as representing the, *essentially unverifiable*, state of the total energy of:

(iii) a finite Universe $\mathcal{U}$:
  - that recycles endlessly from Big Bang to Ultimate Implosion; and
  - in which the existence of 'dark energy' is mathematically and intuitionistically unobjectionable.

Moreover, the only assumptions we have made are that $\mathcal{U}$ obeys Einstein's equations and classical quantum theory, and that:

THESIS 25.4. The anti-matter in $\mathcal{U}$ produces a repulsive, anti-gravitational, field:

- that is consistent with both general relativity and Newtonian gravity;
- whose state at any instant is either exploding, steady, or imploding;
- whose 'energy anti-confinement' strength at any instant is determined by an anti-gravitational dimensionless 'cosmological constant' asp that can assume any of three values $asp = 1$ (exploding at the instant of the Big Bang), $asp = \frac{1}{3}$ (steady between an explosion and an implosion) or $asp = \frac{1}{3} + \frac{2}{3}(1 - \frac{1}{n+1})$ (imploding at the instant of the extinguishing of the n^{th} 'critical black hole' for all $n \geq 1$);
- which constantly opposes the 'energy confinement' strength of the Newtonian gravitational field whose state is determined at any instant by only one dimensionless gravitational constant[2] $gsp = \frac{1}{2}$.

Since it is conventional wisdom (see [**BCST**], [**Vi11**], [**Chr97**], [**NG91**]) that the existence of anti-matter which could produce a repulsive, anti-gravitational, field is admitted by both general relativty and Newtonian gravity, we conclude from Theses 25.2 and 25.4 that the commonly perceived barriers to modelling the behaviour of such a universe $\mathcal{U}$ unambiguously in a mathematical language may be illusory, and reflect merely an attempt to ask of the language selected for such representation more than it is designed to deliver unequivocally.

---

[2]Which could be viewed as corresponding to the gravitational constant, denoted by G, common to both Newton's law of universal gravitation and Einstein's general theory of relativity; whose value in Planck units is *defined* as 1, and whose measured value is expressed in the *International System of Units* as approximately 6.674 x 10^{-11} $N.kg^{-2}.m^{2}$.

More specifically, from the perspective of the *evidence-based* reasoning introduced in [**An16**], it can reasonably be argued that the commonly perceived barriers to modelling the behaviour of such a universe $\mathcal{U}$ realistically in a mathematical language may reflect the fact that:

- since the real numbers are defined by conventional wisdom in set-theoretical terms as the postulated limits of Cauchy sequences in a second-order *dichotomous*[3] arithmetic such as ACA_0,
- the prevailing language of choice for representing physical phenomena and their associated abstractions (conceptual metaphors) mathematically is generally some language of Set Theory,
- which admits axioms—such as an axiom of infinity—whose veridicality cannot be *evidence-based* (in the sense of Chapter 5) under a well-defined interpretation,
- and in which the *dichotomy* highlighted in ACA_0 could admit a contradiction under any well-defined interpretation of the theory.

25.3. Further directions suggested by this investigation

We note that Fig.2 in §24.3 is not a unique model for the 'confinement' properties of the universe $\mathcal{U}$. For instance, we could have started essentially similar iterations with a square $ABCD$ of side s.

Moreover, it is not necessary that each 'black hole' create isomorphic sub-universes; an assumption intended only to illustrate that an event such as an Ultimate Implosion is well-definable mathematically.

However, since the Ultimate Implosion is defined as corresponding to a mathematical limit as $n \rightarrow \infty$, and we postulate that there are no infinite processes in physical phenomena, it follows that the law determining such an Ultimate Implosion (as also the point of implosion of a 'black hole') may be of an essentially 'unknowable' quantum nature; in which case we cannot even assume in principle that a universe such as $\mathcal{U}$ can be shown to *actually* exist on the basis of *evidence-based* reasoning, nor whether or not it would recycle identically each time (in either direction).

It may thus be worth considering further, by the principle of Occam's razor, whether the above simple mathematical model of the properties of a universe $\mathcal{U}$—which, defined as obeying Einstein's equations and quantum theory, seems to fit our known experimental observations—can be taken to suggest that, as implicitly argued by physicist Sabine Hossenfelder, we may have reached the foundations of physics beyond which the laws of nature are essentially 'unknowable':

> "So you want to know what holds the world together, how the universe was made, and what rules our existence goes by? The closest you will get to an answer is following the trail of facts down into the basement of science. Folow it until facts get sparse and your onward journey is blocked by theoreticians arguing whose theory is prettier. That's when you know you've reached the foundations.
>
> The foundations of physics are those ingredients of our theories that cannot, for all we presently know, be derived from anything simpler. At this

[3]Since we show how—in the case of Goodstein's Theorem—such a belief leads to a dichotomous conclusion in Theorem 22.3.

> bottommost level we presently have space, time, and twenty-five particles, together with the equations that encode their behaviour. ...
>
> In the foundations of physics we deal only with particles that cannot be further decomposed; we call them "elementary particles." For all we presently know, they have no substructure. But the elementary particles can combine to make up atoms, molecules, proteins—and thereby create the enormous variety of structures we see around us. It's these twenty-five particles that you, I, and everything else in the universe are made of.
>
> But the particles themselves aren't all that interesting. What is interesting are the relations between them, the principles that determine their interaction, the structure of the laws that gave birth to the universe and enabled our existence. In our game, it's the rules we care about, not the pieces. And the most important lesson we have learned is that nature plays by the rules of mathematics."
>
> ... *Hossenfelder:* [**Hos18**], *p.6.*

From the broader, multi-disciplinary, *evidence-based* perspective of this investigation, we view Hossenfelder as essentially arguing in [**Hos18**] that committing intellectual and physical resources to seeking experimental verification for the putative existence of physical objects, or of a 'Theory', should:

- only follow if such putative objects, or the putative elements of the 'Theory', can be theoretically defined—even if only in principle—in a categorical mathematical language, such as the first-order Peano Arithmetic, which (see [**An16**]) has a finitary *evidence-based* interpretation, and admits unambiguous communication between any two intelligences—whether human or mechanistic;
- and not merely on the basis that they can be conceptualised metaphorically and represented in a set-theoretical language such as ZF which, even though first-order, has no *evidence-based* interpretation that would admit unambiguous communication.

CHAPTER 26

Is the validity of mathematics under siege?

The importance for mathematicians of an insistence on *evidence-based* reasoning highlighted by the gedanken considered in §24.4, §24.5 and §25.1 is reflected in Simpson's impassioned plea, in [**Sim88**], for justifying the increasing abstractness of mathematical reasoning—and avoiding the consequent dangers of a gradual diminishing of its utility to societal imperatives—by showing how, and insisting that, such reasoning refers to reality:

> "As to the usefulness of mathematics, opinion is divided. Some see mathematics as both a supreme achievement of human reason and, via science and industry, the benefactor of all mankind. (This is my own view.) Others believe that mathematics causes only alienation and war. Still others see mathematics as a useless but harmless pastime. The utility of mathematics can be argued only as part of a broad defense of reason, science, technology and Western civilization.
>
> What chiefly concerns us here is not utility but scientific truth. Of course the two issues are related. Pragmatists might argue that mathematics is useful and therefore valid. But such an inference can cover only applied mathematics and is anyhow a *non sequitur*. It makes much more sense to argue that mathematics is true and therefore useful. In the last analysis, the only way to demonstrate that mathematics is valid is to show that it refers to reality.
>
> And make no mistake about it—the validity of mathematics is under siege. In a widely cited article [28], Wigner declares that there is no rational explanation for the usefulness of mathematics in the physical sciences. He goes on to assert that all but the most elementary parts of mathematics are nothing but a miraculous formal game. Kline, in his influential book *Mathematics: The Loss of Certainty* [17], deploys a wide assortment of mathematical arguments and historical references to show that "there is no truth in mathematics." Klines book was published by the Oxford University Press and reviewed favorably in the New York Times. (For a much more insightful review, see Corcoran [4].) Neither Wigner nor Kline is viewed as an enemy of mathematics. But with friends like these, who needs enemies? Arguments like those of Kline and Wigner turn up with alarming frequency in coffee-room discussions and in the popular press. Russell's famous characterization of mathematics, as "the science in which we never know what we are talking about, nor whether what we say is true," is gleefully cited by every wisecracking sophist.
>
> In the face of the attack on mathematics, what defense is offered by the existing schools of the philosophy of mathematics? Consider first the logicists. They say that mathematics is logic, logic consists of analytic truths, and analytic truths are those which are independent of subject matter. In short, mathematics is a science with no subject matter. What about the formalists? According to them, mathematics is a process of manipulating symbols which need not symbolize anything. Then there are the intuitionists, who say that mathematics consists of mental constructions which have no

necessary relation to external reality, if indeed there is any such thing as external reality. Finally we come to the Platonists. They are better than the others because at least they allow mathematics to have some subject matter. But the subject matter which they postulate is a separate universe of objects and structures which bear no necessary relation to the real world of entities and processes. (They use the term "real world" referring not to the *real* real world but to their ideal universe of mathematical objects. The real real world is absent from their theory.) I submit that none of these schools is in a position to defend mathematics against the Russells and the Klines.

The four schools discussed in the previous paragraph are not very far apart. Each of them is based on some variant of Kantianism. Frequently they merge and blend. Most mathematicians and mathematical logicians lean toward an uneasy mixture of formalism and Platonism. Uneasiness flows from the implicit realization that neither formalism nor Platonism nor the mixture supports a comprehensive view of mathematics and its applications. There is urgent need for a philosophy of mathematics which would supply what Wigner lacks, *viz.* a rational explanation of the usefulness of mathematics in the physical sciences. Some form of finitistic reductionism may be relevant here.

I have argued elsewhere that the attack on mathematics is part of a general assault against reason. But this is not the burden of my remarks today. What is clear is that mathematicians and philosophers of mathematics ought to get on with the task of defending their discipline."

. . . *Simpson*: [**Sim88**], §6.1, p.12-15.

26.1. Why Trust a Theory?

The topical *relevance* of Simpson's plea—as also of the *importance* of insistence on *evidence-based* reasoning for philosophers of science too—was evidenced at a workshop in December 2015, convened by the Munich Centre for Mathematical Philosophy and the Arnold Sommerfeld Center for Theoretical Physics at the Ludwig Maximilians-Universtät, München, to address the issue:

Why Trust a Theory? Reconsidering Scientific Methodology in Light of Modern Physics

"Fundamental physics today faces increasing difficulties to find conclusive empirical confirmation of its theories. Some empirically unconfirmed or inconclusively confirmed theories in the field have nevertheless attained a high degree of trust among their exponents and are de facto treated as well established theories. This situation raises a number of questions that are of substantial importance for the future development of fundamental physics. Can a high degree of trust in an empirically unconfirmed or inconclusively confirmed theory be scientifically justified? Does the extent to which empirically unconfirmed theories are trusted today constitute a substantial change of the character of scientific reasoning? Might some important theories of contemporary fundamental physics be empirically untestable in principle?"

. . . *http://www.whytrustatheory2015.philosophie.uni-muenchen.de/index.html.*

26.2. A Fight for the Soul of Science

Reflecting the seriousness—and intensity—with which the issue was addressed by participants, senior science writer Natalie Wolchover reported on the workshop—in her blogpost [**Wol15**]—as *A Fight for the Soul of Science*, where scientists and philosophers debated to what extent they could responsibly trust string theory, the 'multiverse', and other ideas of modern physics that are potentially untestable:

> "Physicists typically think they "need philosophers and historians of science like birds need ornithologists," the Nobel laureate David Gross told a roomful of philosophers, historians and physicists last week in Munich, Germany, paraphrasing Richard Feynman.
>
> But desperate times call for desperate measures.
>
> Fundamental physics faces a problem, Gross explained—one dire enough to call for outsiders' perspectives. "I'm not sure that we don't need each other at this point in time," he said.
>
> It was the opening session of a three-day workshop, held in a Romanesque-style lecture hall at Ludwig Maximilian University (LMU Munich) one year after George Ellis and Joe Silk, two white-haired physicists now sitting in the front row, called for such a conference in an incendiary opinion piece in Nature. One hundred attendees had descended on a land with a celebrated tradition in both physics and the philosophy of science to wage what Ellis and Silk declared a "battle for the heart and soul of physics."
>
> The crisis, as Ellis and Silk tell it, is the wildly speculative nature of modern physics theories, which they say reflects a dangerous departure from the scientific method. Many of todays theorists—chief among them the proponents of string theory and the multiverse hypothesis—appear convinced of their ideas on the grounds that they are beautiful or logically compelling, despite the impossibility of testing them. Ellis and Silk accused these theorists of "moving the goalposts" of science and blurring the line between physics and pseudoscience. "The imprimatur of science should be awarded only to a theory that is testable," Ellis and Silk wrote, thereby disqualifying most of the leading theories of the past 40 years. "Only then can we defend science from attack."
>
> They were reacting, in part, to the controversial ideas of Richard Dawid, an Austrian philosopher whose 2013 book *String Theory and the Scientific Method* identified three kinds of "non-empirical" evidence that Dawid says can build trust in scientific theories absent empirical data. Dawid, a researcher at LMU Munich, answered Ellis and Silk's battle cry and assembled far-flung scholars anchoring all sides of the argument for the high-profile event last week."
>
> ... *Wolchover:* [**Wol15**].

The challenge faced by the scientists and philosophers, Wolchover reported, was that:

> "As we approach the practical limits of our ability to probe nature's underlying principles, the minds of theorists have wandered far beyond the tiniest observable distances and highest possible energies. Strong clues indicate that the truly fundamental constituents of the universe lie at a distance scale 10 million billion times smaller than the resolving power of the LHC. This is the domain of nature that string theory, a candidate "theory of everything," attempts to describe. But it's a domain that no one has the faintest idea how to access.
>
> The problem also hampers physicists' quest to understand the universe on a cosmic scale: No telescope will ever manage to peer past our universe's cosmic horizon and glimpse the other universes posited by the multiverse hypothesis. Yet modern theories of cosmology lead logically to the possibility that our universe is just one of many.
>
> Whether the fault lies with theorists for getting carried away, or with nature, for burying its best secrets, the conclusion is the same: Theory has detached itself from experiment. The objects of theoretical speculation are now too far away, too small, too energetic or too far in the past to reach or rule out with our earthly instruments. So, what is to be done? As Ellis and

> Silk wrote, "Physicists, philosophers and other scientists should hammer out a new narrative for the scientific method that can deal with the scope of modern physics."
>
> "The issue in confronting the next step," said Gross, "is not one of ideology but strategy: What is the most useful way of doing science?"
>
> Over three mild winter days, scholars grappled with the meaning of *theory, confirmation and truth*; how science works; and whether, in this day and age, philosophy should guide research in physics or the other way around.
>
> [...]
>
> The German physicist Sabine Hossenfelder, in her talk, argued that progress in fundamental physics very often comes from abandoning cherished prejudices (such as, perhaps, the assumption that the forces of nature must be unified). Echoing this point, Rovelli said "Dawid's idea of non-empirical confirmation [forms] an obstacle to this possibility of progress, because it bases our credence on our own previous credences." It "takes away one of the tools—maybe the soul itself—of scientific thinking," he continued, "which is 'do not trust your own thinking.'""
>
> ... *Wolchover:* [**Wol15**].

A dilemma for the philosophers, Wolchover notes, was that the lessons of history argue against conflating non-empirical argument with testable theory in the physical sciences[1]:

> "One concern with including non-empirical arguments in Bayesian confirmation theory, Dawid acknowledged in his talk, is "that it opens the floodgates to abandoning all scientific principles." One can come up with all kinds of non-empirical virtues when arguing in favor of a pet idea. "Clearly the risk is there, and clearly one has to be careful about this kind of reasoning," Dawid said. "But acknowledging that non-empirical confirmation is part of science, and has been part of science for quite some time, provides a better basis for having that discussion than pretending that it wasn't there, and only implicitly using it, and then saying I haven't done it. Once it's out in the open, one can discuss the pros and cons of those arguments within a specific context."
>
> The trash heap of history is littered with beautiful theories. The Danish historian of cosmology Helge Kragh, who detailed a number of these failures in his 2011 book, *Higher Speculations*, spoke in Munich about the 19th-century vortex theory of atoms. This "Victorian theory of everything," developed by the Scots Peter Tait and Lord Kelvin, postulated that atoms are microscopic vortexes in the ether, the fluid medium that was believed at the time to fill space. Hydrogen, oxygen and all other atoms were, deep down, just different types of vortical knots. At first, the theory "seemed to be highly promising," Kragh said. "People were fascinated by the richness of the mathematics, which could keep mathematicians busy for centuries, as was said at the time." Alas, atoms are not vortexes, the ether does not exist, and theoretical beauty is not always truth.
>
> Except sometimes it is. Rationalism guided Einstein toward his theory of relativity, which he believed in wholeheartedly on rational grounds before it was ever tested. "I hold it true that pure thought can grasp reality, as the ancients dreamed," Einstein said in 1933, years after his theory had been confirmed by observations of starlight bending around the sun.

[1]Which resonate with the consequences sought to be highlighted in this investigation—for the mathematical sciences—of conflating algorithmically *verifiable* reasoning with algorithmically *computable* reasonng.

> The question for the philosophers is: Without experiments, is there any way to distinguish between the non-empirical virtues of vortex theory and those of Einstein's theory? Can we ever really trust a theory on non-empirical grounds?
>
> *... Wolchover:* [**Wol15**].

Wolchover remarks that—despite serious dissent—a degree of consensus did take shape over the course of 'these pressing yet timeless discussions':

> "As for what was accomplished, one important outcome, according to Ellis, was an acknowledgment by participating string theorists that the theory is not "confirmed" in the sense of being verified. "David Gross made his position clear: Dawid's criteria are good for justifying working on the theory, not for saying the theory is validated in a non-empirical way," Ellis wrote in an email. "That seems to me a good position—and explicitly stating that is progress."
>
> In considering how theorists should proceed, many attendees expressed the view that work on string theory and other as-yet-untestable ideas should continue. "Keep speculating," Achinstein wrote in an email after the workshop, but "give your motivation for speculating, give your explanations, but admit that they are only possible explanations."
>
> "Maybe someday things will change," Achinstein added, "and the speculations will become testable; and maybe not, maybe never." We may never know for sure the way the universe works at all distances and all times, "but perhaps you can narrow the live possibilities to just a few," he said. "I think that would be some progress."
>
> *... Wolchover:* [**Wol15**].

26.3. The Downside of Group-Think

However, some of the more disquieting aspects of seeking such consensus are reflected in the perspective of one of the dissenters at the workshop, physicist Sabine Hossenfelder who, in an impassioned recent blogpost [**Hos18**], seeks to identify 'group-think' as responsible to a significant extent for the increasing disassociation between the abstractness of some currently mainstream 'unifying' theories of physical phenomena, and the sensory observations in which they claim to be rooted:

> "Science isn't immune to group-think. On the contrary: Scientific communities are ideal breeding ground for social reinforcement.
>
> Research is currently organized in a way that amplifies, rather than alleviates, peer pressure: Measuring scientific success by the number of citations encourages scientists to work on what their colleagues approve of. Since the same colleagues are the ones who judge what is and isn't sound science, there is safety in numbers. And everyone who does not play along risks losing funding.
>
> As a result, scientific communities have become echo-chambers of likeminded people who, maybe not deliberately but effectively, punish dissidents. And scientists don't feel responsible for the evils of the system. Why would they? They just do what everyone else is also doing.
>
> [...]
>
> It happens here in the foundations of physics too.
>
> In my community, it has become common to justify the publication of new theories by claiming the theories are falsifiable. But falsifiability is a weak criterion for a scientific hypothesis. It's necessary, but certainly not sufficient,

> for many hypotheses are falsifiable yet almost certainly wrong. Example: It will rain peas tomorrow. Totally falsifiable. Also totally nonsense.
>
> Of course this isn't news. Philosophers have gone on about this for at least half a century. So why do physicists do it? Because it's easy and because all their colleagues do it. And since they all do it, theories produced by such methods will usually get published, which officially marks them as "good science".
>
> In the foundations of physics, the appeal to falsifiability isn't the only flawed method that everyone uses because everyone else does.There are also those theories which are plainly unfalsifiable. And another example are arguments from beauty.
>
> In hindsight it seems perplexing, to say the least, but physicists published ten-thousands of papers with predictions for new particles at the Large Hadron Collider because they believed that the underlying theory must be natural. None of those particles were found.
>
> Similar arguments underlie the belief that the fundamental forces should be unified because that's prettier (no evidence for unification has been found) or that we should be able to measure particles that make up dark matter (we didn't). Maybe most tellingly, physicists in these community refuse to consider the possibility that their opinions are affected by the opinions of their peers.
>
> One way to address the current crises in scientific communities is to impose tighter controls on scientific standards. That's what is happening in psychology right now, and I hope it'll also happen in the foundations of physics soon. But this is curing the symptoms, not the disease. The disease is a lacking awareness for how we are affected by the opinions of those around us.
>
> The problem will reappear until everyone understands the circumstances that benefit group-think and learns to recognize the warning signs: People excusing what they do with saying everyone else does it too. People refusing to take responsibility for what they think are "evils of the system." People unwilling to even consider that they are influenced by the opinions of others. We have all the warning signs in science—had them for decades.
>
> Accusing scientists of group-think is standard practice of science deniers. The tragedy is, there's truth in what they say. And it's no secret: The problem is easy to see for everyone who has the guts to look. Sweeping the problem under the rug will only further erode trust in science."
>
> . . . *Hossenfelder:* [**Hos18**].

That Hossenfelder makes a significant point is undeniable. Whether or not group-think is to be held mainly responsible—for the persisting acceptance of untestable beliefs as reliable science for an understanding of the laws of nature that we believe underlie our observations of physical phenomena—is debatable.

From the *evidence-based* perspective of this investigation, we tend to view the increasing disassociation between the abstractness of some currently mainstream 'untestable' theories of physical phenomena, and the sensory observations in which they claim to be rooted, as reflecting more the argument that:

THESIS 26.1. It is the mathematicians who are ultimately responsible (in the sense of Chapter 23) for ensuring that the veridicality of the axiomatic propositions of the language in which such abstractions (which we view as the conceptual metaphors defined by Lakoff and Núñez in [**LR00**]) are adequately *expressed* and effectively *communicated* is *evidence-based*.

However, since theories that are 'empirically untestable in principle' are not only (compare §23.2):

> *Beliefs* that we *hold* to be 'true' in an absolute, Platonic, sense, and have in common with others holding similar *beliefs* as absolute, Platonic, 'truths'

but, by implicit definition, *beliefs* that cannot yield any productive insight on the nature of the information sought to be expressed and conveyed by the underlying theories, Hossenfelder's criticism—even if viewed as mis-directed—would appear to be as justified as her disquietitude at:

> "...the belief that the fundamental forces should be unified because that's prettier (no evidence for unification has been found) or that we should be able to measure particles that make up dark matter (we didn't)."

Moreover, from the *evidence-based* perspective of this investigation, Hossenfelder's argument—that we need to be aware of, and compensate for, the downside of 'group-think' when it discourages search for alternative explanations of challenging phenomena that may require us to step outside the comfort zone of our credences—is supported by the argument:

(i) in §25.1 that positing the existence of putative 'dark matter' particles is not mathematically necessary;

(ii) in §25.1 that positing the existence of putative 'multiverses' in which the laws of nature are substantially different is not mathematically necessary;

(iii) in §28(b) that positing putative non-locality in the EPR argument by appeal to Bell's Theorem appears necessary only because of the tacit—and unsustainable—belief of conventional wisdom[2] that the mathematical representations of all natural phenomena *must* obey a 'unified' logic.

    A more insightful interpretation of the EPR thesis follows once we recognise that any mathematical language which can adequately express and effectively communicate the laws of nature may be consistent under two, essentially different but complementary and not contradictory, logics for assigning truth values to the propositions of the language.

    It would further follow, then, that:

    (a) whereas the mathematical representations of all natural phenomena which is both determinate *and* predictable must necessarily be defined in terms of classical, algorithmically computable, functions;

    (b) the mathematical representations of quantum phenomena may be in terms of functions that are algorithmically verifiable, but not algorithmically computable—in which case such phenomena would be determinate but *not* predictable (and their mathematical representations need not be subject to Bell's Theorem).

[2]'Group-think' in Hossenfelder's lexicon! Eerily reminiscent of the pre-Einstein belief in an all-pervasive Newtonian 'aether' populating an absolute frame of reference.

26.4. Why mathematics may be viewed as merely an *amusing* game

> *"Finitistic reasoning is unique because of its clear real-world meaning and its indispensability for all scientific thought. Non-finistic reasoning can be accused of referring not to anything in reality but only to arbitrary mental constructions. Hence non-finistic mathematics can be accused of being not science but merely a mental game played for the amusement of mathematicians."*
>
> ... *Simpson*: [**Sim88**], §6.4, p.15.

The question arises:

Query 26.2. In what sense—if at all—can mathematicians be held responsible (in the sense of Chapter 23) for ensuring that the veridicality of the axiomatic propositions of the language—in which natural scientists and philosophers seek to adequately *express* and effectively *communicate* their sensory perceptions and associated abstractions—is *evidence-based*?

There are two issues involved here:

(1) Is it possible to develop an *evidence-based* language of adequate expression for the sensory perceptions of the physical sciences; and of effective communication for their associated philosophical abstractions?

(2) Do mathematicians *believe* that their primary responsibility is to develop such a language?

The first issue resolves straightforwardly in an affirmative if we accept both:

(a) Thesis 44.1 that the first-order Set Theory ZFC is sufficient for a human intelligence to express the conceptual metaphors that correspond to both:
  - the sensory perceptions observed and recorded by the physical sciences;
  - and the associated abstractions in which philosophers form consistent narratives of a commonly perceived external world that—when expressed in a symbolic language, and viewed as semiotic strings—can themselves be treated as giving rise to further, albeit artificially 'created', sensory perceptions;

  and

(b) Thesis 27.5 that the first-order Peano Arithmetic PA is categorical, and is thus both necessary and sufficient for a mechanical intelligence[3] (ergo, also for a human intelligence) to effectively communicate those conceptual metaphors of the physical and philosophical sciences that are *evidence-based*.

The second issue, too, would resolve straightforwardly in the affirmative if mathematicians could be seen as recognising, and embracing, the significance of (a).

However, it would not be unreasonable to hold that—influenced in no small measure by G. H. Hardy's impassioned defence, in *A Mathematician's Apology* [**Ha40**], of the practice of mathematics purely for its intrinsic aesthetics—an *enviable*

---

[3]The wider significance of relying on a mechanical intelligence as the standard is seen in §23.5 and Query 23.3, where we consider the question of whether a fear of actively seeking an ETI is merely paranoia, or whether it has a rational component.

illusion of a mathematician in an ivory tower, occupied in intellectually absorbing—even if not *amusing*—scribbles that need not have any relevance to the world outside, has gradually become the preferred narrative—attractive even to mathematicians.

That such narrative is not without basis follows since, from the *evidence-based* perspective of this investigation, it appears that, currently, the majority when wearing a mathematician's hat (see Chapter 23) follow, but do not seek *evidence-based* reasoning for, Hilbert's non-finitary reasoning and:

(i) explicitly accept (Aristotle's particularisation) that the formula $[(\exists x)F(x)]$ of a formal mathematical language L can be interpreted Platonically over an infinite domain D as the proposition 'There is some element a of D such that $F^*(a)$', where the proposition $F^*(a)$ is the interpretation of the L-formula $[F(a)]$ in D, and where there need not—even in principle—be *any evidence* for the existence of such an element a in the domain D;

(ii) implicitly accept (a); but

(iii) conclude from Gödel's reasoning in [**Go31**] that there are undecidable formulas in PA—a false conclusion (see Corollary 11.9) that does not admit (b);

whilst the rest, despite following Brouwer's more constructive reasoning, also do not seek to apply *evidence-based* reasoning strictly when:

(iv) denying (Aristotle's particularisation) that the formula $[(\exists x)F(x)]$ of a formal mathematical language L can be interpreted unrestrictedly over an infinite domain D as the proposition 'There is some element a of D such that $F^*(a)$';

(v) implicitly rejecting (a) by holding that there can be no intuitively unobjectionable interpretation of ZFC, thereby denying that ZFC can be interpreted in terms of Lakoff and Núñez's conceptual metaphors; and

(vi) believing that Aristotle's particularisation entails both the law of the excluded middle, and therefore the standard first-order logic FOL (in which this law is a theorem), have no finitary interpretation—a false belief (see Corollary 9.11) which does not admit (b).

Moreover:

- not only classical conventional wisdom based on Hilbert's approach to, and development of, proof theory (see, for instance, [**RS17**]; also [**Mycl**]),
- but even strictly constructive perspectives (as articulated, for instance, in [**Ba05**] or [**Shr13**]);

fail to distinguish between the multi-dimensional nature of the logic of a formal mathematical language (Definition 21.5), and the one-dimensional nature of the veridicality of its assertions, since both fail to adequately distinguish that:

(α) Whereas the goal of classical mathematics, post Peano, Dedekind and Hilbert, has been:

- to uniquely characterise each informally defined mathematical structure (e.g., the Peano Postulates and its associated classical predicate logic)
- by a corresponding formal first-order language, and a set of finitary axioms/axiom schemas and rules of inference (e.g., the first-order Peano Arithmetic PA and its associated first-order logic FOL)
- which assign unique provability values to each well-formed proposition of the language;

(β) The goal of constructive mathematics, post Brouwer and Tarski, has been:

- to assign unique, *evidence-based*, truth values to each well-formed proposition of the language
- under a constructively well-defined interpretation over the domain of the structure (when viewed as a 'conceptual metaphor' in the terminology of [**LR00**]).

The goals of the two activities ought to, thus, be viewed as necessarily complementing each other, rather than being treated as independent of, or in conflict with, each other as to which is more 'foundational'—as is, for instance, misleadingly argued[4] on the one hand in the following remarks of constructivist Errett Bishop and, on the other by classicist Penelope Maddy in [**Ma18**] and [**Ma18a**]:

> "The use of a formal mathematical system as a programming language presupposes that the system has a constructive interpretation. Since most formal systems have a classical, or nonconstructive, basis (in particular, they contain the law of the excluded middle), they cannot be used as programming languages.
>
> The role of formalisation in constructive mathematics is completely distinct from its role in classical mathematics. Unwilling—indeed unable, because of his education—to let mathematics generate its own meaning, the classical mathematician looks to formalism, with its emphasis on consistency (either relative, empirical, or absolute), rather than meaning, for philosophical relief. For the constructivist, formalism is not a philosophical out; rather it has a deeper significance, peculiar to the constructivist point of view. Informal constructive mathematics is concerned with the communication of algorithms, with enough precision to be intelligible to the mathematical community at large. Formal constructive mathematics is concerned with the communication of algorithms with enough precision to be intelligible to machines."
>
> *... Bishop:* [**Bi18**], *pp.1-2.*

One could reasonably argue, further, that it is this internal focus on debating as to which mathematical language is more 'foundational' that has obscured both their internal contradictions (see, for instance, §22.2 and Theorem 22.3), and a more responsible, external, appreciation of the very raison d'être of any mathematical language which, as highlighted in Chapter 23 (and by the issues raised in §25 relating to the three gedanken considered in §24.3), is to eliminate ambiguity in the precise expression and unambiguous communication of:

---

[4]Mistakenly in Bishop's case, since (a) Theorem 10.2 shows that the first-order Peano Arithmetic PA *can* be used as a programming language; and (b) Bishop erroneously (see Corollary 9.11) treats the law of the excluded middle—ergo the classical first-order logic FOL in which this law is a theorem—as 'nonconstructive'.

- first, a natural scientist's recording of our sensory perceptions and their associated perceptions of a 'common' external world; and
- second, a philosopher's abstractions of a coherent, holistic, perspective of the 'common' external world from our sensory perceptions and their associated perceptions.

Part 8

The significance of *evidence-based* reasoning for some grey areas in the foundations of the Physical Sciences

CHAPTER 27

The argument for Lucas' Gödelian Argument

Although the philosophical ramifications of John Lucas' original Gödelian argument against a reductionist account of the mind ([**Lu61**] deserve consideration that lie beyond the immediate implications of that paper, we draw attention to his informal defence of his Gödelian Thesis, where he concludes with the remarks:

> "Thus, though the Gödelian formula is not a very interesting formula to enunciate, the Gödelian argument argues strongly for creativity, first in ruling out any reductionist account of the mind that would show us to be, au fond, necessarily unoriginal automata, and secondly by proving that the conceptual space exists in which it intelligible to speak of someone's being creative, without having to hold that he must be either acting at random or else in accordance with an antecedently specifiable rule".
> ... *Lucas:* [**Luxx**].

We shall only seek here the significance of *evidence-based* reasoning for Lucas' Gödelian Thesis (as also for philosophy and the physical sciences), which is illuminated by viewing the—seemingly conflicting—classical and intuitionistic interpretations of quantification as yielding two, essentially different, interpretations of the first-order Peano Arithmetic PA (over the structure $\mathbb{N}$ of the natural numbers) that are complementary, and not contradictory ([**An15**]).

We note that the former yields the standard interpretation $\boldsymbol{M}$ of PA over $\mathbb{N}$, which is defined *relative* to the assignment T_M of algorithmically *verifiable* Tarskian truth values to the compound formulas of PA under $\boldsymbol{M}$ (Theorem 7.7 in §7.1), and which circumscribes the ambit of *non-finitary* human reasoning about 'true' arithmetical propositions.

The latter yields a finitary interpretation $\boldsymbol{B}$ of PA over $\mathbb{N}$, which is constructively well-defined *relative* to the assignment T_B of algorithmically *computable* Tarskian truth values to the compound formulas of PA under $\boldsymbol{B}$ (Theorem 9.7 in §9.1 The well-definedness follows from the *finitary* proof of consistency for PA detailed therein), and which circumscribes the ambit of *finitary* mechanistic reasoning about 'true' arithmetical propositions.

The complementarity can also be viewed as validating Lucas' Gödelian argument, if we treat it as the claim that:

THEOREM 27.1. *There can be no mechanist model of human reasoning if the standard interpretation $\boldsymbol{M}$ of PA can be treated as circumscribing the ambit of human reasoning about 'true' arithmetical propositions, and the finitary interpretation $\boldsymbol{B}$ of PA can be treated as circumscribing the ambit of mechanistic reasoning about 'true' arithmetical propositions.*

PROOF. Gödel has shown how to construct an arithmetical formula with a single variable—say $[R(x)]$ (Gödel refers to this formula only by its Gödel number r in [**Go31**], p.25(12))—such that:

- $[R(x)]$ is not PA-provable; but
- $[R(n)]$ is instantiationally PA-provable for any specified P numeral $[n]$.

Hence, for any specified numeral $[n]$, Gödel's primitive recursive relation $xB\lceil[R(n)]\rceil$ must hold for some natural number m:

- where xBy denotes Gödel's primitive recursive relation ([**Go31**], p. 22(45)):

  'x is the Gödel-number of a proof sequence in PA whose last term is the PA formula with Gödel-number y';
- and $\lceil[R(n)]\rceil$ denotes the Gödel-number of the PA formula $[R(n)]$;

If we assume that any mechanical witness can only reason *finitarily* then although, for any specified numeral $[n]$, a mechanical witness can give evidence under the finitary interpretation $\boldsymbol{B}$ that the PA formula $[R(n)]$ holds in $\mathbb{N}$, no mechanical witness can conclude *finitarily* under the finitary interpretation $\boldsymbol{B}$ of PA that, for any specified numeral $[n]$, the PA formula $[R(n)]$ holds in $\mathbb{N}$.

However, if we assume that a human witness can also reason *non-finitarily*, then a human witness *can* conclude under the non-finitary standard interpretation $\boldsymbol{M}$ of PA that, for any specified numeral $[n]$, the PA formula $[R(n)]$ holds in $\mathbb{N}$. □

27.1. A definitive Turing-test

"*Let us fix our attention on one particular digital computer C. Is it true that by modifying this computer to have an adequate storage, suitably increasing its speed of action, and providing it with an appropriate programme, C can be made to play satisfactorily the part of A in the imitation game, the part of B being taken by a man? ... In short, then, there might be men cleverer than any given machine, but then again there might be other machines cleverer again, and so on.*"

... A. M. Turing (1950), [**Tu50**], *§5 and Objection (3).*

Theorem 27.1 can also be viewed as a definitive Turing-test between a logician and any Turing machine TM.

In other words, we can demonstrate that the algorithmically computable architecture of *any* conceivable Universal Turing machine has inherent limitations which constrain it from answering Query 27.2 affirmatively; whereas the human brain is not constrained similarly.

Of course, such a demonstration can be considered a 'Turing-test' with respect only to *presumption* of an implicit *quantitive* element in Turing's intent in the above quote; where he ostensibly seems to query only *qualitatively* whether the mathematical reasoning ability of the brain of a human being, considered as a species (and not that of any individual human in particular), is demonstrably superior, or cleverer, than the mathematical reasoning ability of any conceivable Universal Turing machine (and not that of only some individually architectured machine).

QUERY 27.2. Can you *prove* that, for any given numeral $[n]$, Gödels arithmetic formula $[R(n)]$ is a theorem in the Peano Arithmetic PA, where $[R(x)]$ is defined by its Gödel number r in eqn.12 on p.25 of [**Go31**]; and $[(\forall x)R(x)]$ is defined by its Gödel number $17Gen\ r$? Answer only either 'Yes' or 'No'.

Logician: Yes.

(By Gödels reasoning on p.26(2) of [**Go31**], for any given numeral $[n]$ the formula $[R(n)]$ is entailed by the axioms of PA; even though the formula $[(\forall x)R(x)]$ is *not* a theorem in PA.)

TM: No.

(By Corollary 8.2 in [**An16**], the formula $[\neg(\forall x)R(x)]$ is provable in PA and so, by Theorem 7.1 in [**An16**], *no* Turing machine can *prove* that the formula with Gödel number $17Gen\ r$ is a theorem in PA and, ipso facto, conclude that, for any given numeral $[n]$, Gödels arithmetic formula $[R(n)]$ is a theorem in PA.)

A prescient appreciation of Theorem 27.1 can be read into Tarski's 'humorous interpretation' of Gödel's argument in [**Go31**] that there are arithmetical propositions which are 'true' under the *weak, verifiable*, standard interpretation of PA, but formally unprovable in PA:

> "So it turned out that the solution of the decision problem in its most general form is negative. I have no doubt that many mathematicians experienced a profound feeling of relief when they heard of this result. Perhaps sometimes in their sleepless nights they thought with horror of the moment when some wicked metamathematician would find a positive solution of the problem, and design a machine which would enable us to solve any mathematical problem in a purely mechanical way, so that any further creative mathematical thought would become a worthless hobby. The danger is now over, that such a robot will ever be created; mathematicians have regained their *raison dêtre* and can sleep quietly."
>
> *... Tarski:* [**Ta39**], *p.166.*

27.2. *Evidence-based* reasoning and the physical sciences

We note that, beyond its explicitly stated mathematical implications, Theorem 27.1 justifies the argument in [**An13**] and [**An15a**] (see also Chapter 29), that resolving seemingly paradoxical arguments such as 'EPR' or 'Schrödinger's cat' may require two, essentially different, Logics (in the sense of [**An15a**], Definition 1) since:

(i) the *weak, verifiable*, standard interpretation $I_{PA(N,SV)}$ of PA can be viewed as corresponding to the way human intelligence conceptualises, symbolically represents, and logically reasons about, those sensory perceptions that are triggered by physical processes which can be treated as representable—not necessarily *finitarily*—by algorithmically *verifiable* formulas, where a physical process is posited, or implicitly presumed, under a *weak* Church-Turing thesis as effectively computable if, and only if, it's mathematical representation is algorithmically *verifiable*; whilst:

(ii) the *strong, finitary*, interpretation $I_{PA(N,SC)}$ of PA can be viewed as corresponding to the way human intelligence conceptualises, symbolically represents, and logically reasons about, only those sensory perceptions that are triggered by physical processes which can be treated as representable—*finitarily*—by algorithmically *computable* formulas, where a physical process is posited, or implicitly presumed, under a *strong* Church-Turing thesis as effectively computable if, and only if, it's mathematical representation is algorithmically *computable*.

An insightful and penetrating perspective on the possible—and sometimes surprising—dynamics of the interactions between human and mechanical intelligences is offered by physicist Sabine Hossenfelder in her extensively researched, and thought-provoking, book intended for a multi-disciplinary audience, *Lost in Math* [**Hos18a**]:

> "Adam works on microbiological growth experiments. Adam formulates hypotheses and devises research strategies. Adam sits in the lab and handles incubators and centrifuges. But Adam isn't a "he." Adam is an "it." It's a robot designed by Ross King's team at Aberystwyth University in Wales. Adam has successfully identified yeast genes responsible for coding certain enzymes.[1]
>
> In physics too the machines are marching in. Researchers at the Creative Machines Lab at Cornell University in Ithaca, New York, have coded software that, fed with raw data, extracts the equations governing the motion of systems such as the chaotic double pendulum. It took the computer thirty hours to re-derive laws of nature that humans struggled for centuries to find.[2]
>
> In a recent work on quantum mechanics, Anton Zeilinger's group used software—dubbed "Melvin"—to devise experiments that the humans then performed.[3] Mario Krenn, the doctoral student who had the idea of automating the experimental design, is pleased with the results but says he still finds it "quite difficult to understand intuitively what exactly is going on."
>
> And this is only the beginning. Finding patterns and organizing information are tasks that are central to science, and those are the exact tasks that artificial neural networks are built to excel at. Such computers, designed to mimic the function of natural brains, now analyze data sets that no human can comprehend and search for correlations using deep-learning algorithms. There is no doubt that technological progress is changing what we mean by "doing science."
>
> I try to imagine the day when we'll just feed all cosmological data into an artificial intelligence (AI). We now wonder what dark matter and dark energy are, but this question might not even make sense to the AI. It will just make predictions. We will test them. And if the AI is consistently right, then we'll know it's succeeded at finding and extrapolating the right patterns. That thing, then, will be our new concordance model. We put in a question, out comes an answer—and that's it.
>
> If you're not a physicist, that might not be so different from reading about predictions made by a community of physicists using incomprehensible math and cryptic terminology. It's just another black box. You might even trust the AI more than us.
>
> But making predictions and using them to develop applications has always been only one side of science. The other side is understanding. We don't just want answers, we want explanations for the answers. Eventually we'll reach the limits of our mental capacity, and after that the best we can

[1][[**Sp10**]].
[2][[**SL09**]].
[3][[**KMFLZ**].]

do is hand over questions to more sophisticated thinking apparatuses. But I believe it's too early to give up understanding our theories.

"When young people join my group," Anton Zeilinger says, "you can see them tapping around in the dark and not finding their way intuitively. But then after some time, two or three months, they get in step and they get this intuitive understanding of quantum mechanics, and it's actually quite interesting to observe. It's like learning to ride a bike."

And intuition comes with exposure. You can get exposure to quantum mechanics—entirely without equations—in the video game Quantum Moves. In this game, designed by physicists at Aarhus University in Denmark, players earn points when they find efficient solutions for quantum problems, such as moving atoms from one potential to another. The simulated atoms obey the laws of quantum mechanics. They appear not like little balls but like a weird fluid that is subject to the uncertainty principle and can tunnel from one place to another. It takes some getting used to. But to the researcher's astonishment, the best solution they crowd-sourced from the players' strategies was more efficient than that found by a computer algorithm. When it comes to quantum intuition, it seems, humans beat AI. At least for now."

. . . Hossenfelder: [**Hos18a**]*, pp.132-134.*

27.3. Emergence in a Mechanical Intelligence

The question arises:

QUERY 27.3. To what extent *can* a mechanical intelligence synthesise logic?

An interesting answer emerges if we accept that a logic of a language can be precisely defined (Definition 21.5) as a finite set of rules which constructively assign unique truth values:

(a) Of provability/unprovability to the formulas of the language; and

(b) Of truth/falsity to the sentences of any theory of the language that is defined semantically by an interpretation of the language over a structure.

It would then follow that, if we are given a first-order language and a structure, and we take synthesising a logic of the structure to mean identifying both some finite set of rules as above and an interpretation under which (a) and (b) hold, then such synthesis should, in principle, be within the ambit of the reasoning ability of a Turing machine based mechanical intelligence.

In particular, it would then follow from Theorem 10.2 that any such mechanical intelligence can prove the PA formula:

$[(\forall x)\neg(\forall y)(x > y)]$,

which a human-like intelligence would interpret as the algorithmically computable true assertion that there is no largest computable natural number.

Now, if we take this assertion as corresponding to cognition of a concept of infinity, and if we consider such cognition as a sign (if not a definition) of emergence in an intelligence, the above perspective suggests that:

THESIS 27.4. The concept of infinity is an emergent feature of any Turing machine based mechanical intelligence founded on the first-order Peano Arithmetic.

Moreover, since all observations of physical phenomena—whether classical or quantum—depend upon mechanical artefacts whose logic is limited by Theorem 10.2 to that of a Turing machine, this suggests that:

Thesis 27.5. Since the reasoning underlying the formulations, and interpretations, of the verifiable laws of both classical and quantum physics based upon the observations of mechanical artefacts is in terms of functions and (Boolean) relations that are algorithmically computable as true or false, discovery and formulation of the laws of both classical and quantum physics lies within the algorithmically computable logic and reasoning of a mechanical intelligence whose logic is circumscribed by the first-order Peano Arithmetic.

The thesis is also suggested by developments in various areas where, quantitatively, the algorithmically computable reasoning ability of a mechanical intelligence appears to compare with, complement, and even arguably improve upon, the algorithmically computable reasoning ability of a human intelligence:

> "We have demonstrated the discovery of physical laws, from scratch, directly from experimentally captured data with the use of a computational search. We used the presented approach to detect nonlinear energy conservation laws, Newtonian force laws, geometric invariants, and system manifolds in various synthetic and physically implemented systems without prior knowledge about physics, kinematics, or geometry. The concise analytical expressions that we found are amenable to human interpretation and help to reveal the physics underlying the observed phenomenon. Many applications exist for this approach, in fields ranging from systems biology to cosmology, where theoretical gaps exist despite abundance in data.
>
> Might this process diminish the role of future scientists? Quite the contrary: Scientists may use processes such as this to help focus on interesting phenomena more rapidly and to interpret their meaning."
>
> *...Schmidt and Lipson:* [**SL09**]*, p.85.*

> "We review the main components of autonomous scientific discovery, and how they lead to the concept of a Robot Scientist. This is a system which uses techniques from artificial intelligence to automate all aspects of the scientific discovery process: it generates hypotheses from a computer model of the domain, designs experiments to test these hypotheses, runs the physical experiments using robotic systems, analyses and interprets the resulting data, and repeats the cycle. We describe our two prototype Robot Scientists: Adam and Eve. Adam has recently proven the potential of such systems by identifying twelve genes responsible for catalysing specific reactions in the metabolic pathways of the yeast *Saccharomyces cerevisiae*. This work has been formally recorded in great detail using logic. We argue that the reporting of science needs to become fully formalised and that Robot Scientists can help achieve this. This will make scientific information more reproducible and reusable, and promote the integration of computers in scientific reasoning. We believe the greater automation of both the physical and intellectual aspects of scientific investigations to be essential to the future of science. Greater automation improves the accuracy and reliability of experiments, increases the pace of discovery and, in common with conventional laboratory automation, removes tedious and repetitive tasks from the human scientist."
>
> *...Sparkes et al:* [**Sp10**]*, Abstract.*

> "Quantum mechanics predicts a number of, at first sight, counterintuitive phenomena. It therefore remains a question whether our intuition is the best way to find new experiments. Here, we report the development of the

> computer algorithm Melvin which is able to find new experimental implementations for the creation and manipulation of complex quantum states. Indeed, the discovered experiments extensively use unfamiliar and asymmetric techniques which are challenging to understand intuitively. The results range from the first implementation of a high-dimensional Greenberger-Horne-Zeilinger state, to a vast variety of experiments for asymmetrically entangled quantum states—a feature that can only exist when both the number of involved parties and dimensions is larger than 2. Additionally, new types of high-dimensional transformations are found that perform cyclic operations.Melvin autonomously learns from solutions for simpler systems, which significantly speeds up the discovery rate of more complex experiments. The ability to automate the design of a quantum experiment can be applied to many quantum systems and allows the physical realization of quantum states previously thought of only on paper."
>
> *... Krenn, Malik, Fickler, Lapkiewicz and Zeilinger:* [**KMFLZ**], *Abstract.*

27.4. Constraints on the cognition of a mechanical intelligence

However, we shall now argue that, whereas a human-like intelligence could conceive of algorithmically *verifiable*, but not algorithmically *computable*, functions and relations that would admit the *EPR* phenomena—as considered in Chapter §29—without violating the relativistic constraints noted therein, such conception is not possible by the logical constraints of a Turing machine based mechanical intelligence whose logic is circumscribed by the Provability Theorem 10.2 of the first-order Peano Arithmetic.

Any such a mechanical intelligence would, perforce, have to accept the existence of the non-locality that lies at the heart of the putative *EPR* paradox (§29.1) as indicating the existence of a physical phenomena that is not subject to relativistic constraints.

That human 'intuition' may—as remarked by Hossenfelder in [**Hos18**] (pp.132-134)—lie demonstrably beyond the algorithmically computable reasoning ability of a mechanical intelligence is also suggested by the following observations of a team of researchers at the Department of Physics and Astronomy, Aarhus University, Denmark:

> "Humans routinely solve problems of immense computational complexity by intuitively forming simple, low-dimensional heuristic strategies [1, 2, 3]. Citizen science exploits this intuition by presenting scientific research problems to non-experts. Gamification is an effective tool for attracting citizen scientists and allowing them to provide novel solutions to the research problems. Citizen science games have been used successfully in Foldit [4], EteRNA [5] and EyeWire [6] to study protein and RNA folding and neuron mapping. However, gamification has never been applied in quantum physics. Everyday experiences of non-experts are based on classical physics and it is a priori not clear that they should have an intuition for quantum dynamics. Does this premise hinder the use of citizen scientists in the realm of quantum mechanics? Here we report on Quantum Moves, an online platform gamifying optimization problems in quantum physics. Quantum Moves aims to use human players to find solutions to a class of problems associated with quantum computing. Players discover novel solution strategies which numerical optimizations fail to find. Guided by player strategies, a new low-dimensional heuristic optimization method is formed, efficiently outperforming the most prominent established methods. We have developed a low-dimensional rendering of the optimization landscape showing a growing complexity when the player solutions get fast. These fast results offer new

insight into the nature of the so-called Quantum Speed Limit. We believe that an increased focus on heuristics and landscape topology will be pivotal for general quantum optimization problems beyond the type presented here."

. . . Sørensen et al: [**Srn16**], *Abstract.*

CHAPTER 28

Can a deterministic universe be unpredictable?

We have argued (in Chapter 23) that the raison d'être of mathematical activity is the elimination of ambiguity in critical cases, such as the unambiguous representation and unequivocal communication of our observations of physical phenomena.

We shall further speculate:

(a) First (in §28.1), how constructive mathematics could model a deterministic universe that is irreducibly probabilistic, since our above investigation of the limitations of standard interpretations of classical mathematical logic suggests that, prima facie, the same foundational issues—logical and mathematical—may be reflected, albeit obliquely, in the dialogue between Albert Einstein and the adherents of the Copenhagen Interpretation of quantum mechanics spear-headed by Neils Bohr.

(b) Second (in §29.14), that the paradoxical element which surfaced as a result of the *EPR* argument (due to the perceived conflict implied by Bell's inequality between the, seemingly essential, non-locality required by current interpretations of Quantum Mechanics, and the essential locality required by current interpretations of Classical Mechanics) may reflect merely lack of recognition that any mathematical language which can adequately express and effectively communicate the laws of nature may be consistent under two, essentially different but complementary and not contradictory, logics for assigning truth values to the propositions of the language, such that the latter are capable of representing—as deterministic—the unpredictable characteristics of quantum behaviour.

28.1. The Bohr-Einstein debate

We speculate first on whether constructive mathematics could, in principle, model a deterministic universe that is irreducibly probabilistic; and suggest a possible resolution of the Einstein-Bohr debate on the essential nature—and on our mathematical representation—of the underlying laws of nature that seem to be reflected in our observations of physical phenomena.

We note that Bohr's perspective echoes, in a sense, that of Gödel ([**Go51**])—and of set-theorists such as Shelah ([**She91**])—who hold Platonically that the truth of the formal propositions, or even axioms, of a mathematical language, under a given interpretation, need not be evidence-based—and may even be unverifiable effectively.

28.2. Bohr excludes detailed analysis of atomic phenomena

Thus, Bohr remarks that:

> "I advocated a point of view, conveniently termed 'complementarity', suited to embrace the characteristic features of individuality of quantum phenomena, and at the same time to clarify the peculiar aspects of the observational problem in this field of experience. For this purpose, it is decisive to recognise that, however far the phenomena transcend the scope of classical physical explanation, the account of all evidence must be expressed in classical terms. The argument is simply that by the word 'experiment' we refer to a situation where we can tell others what we have done and what we have learned and that, therefore, the account of the experimental arrangement and of the results of the observations must be expressed in unambiguous language with suitable application of the terminology of classical physics.
>
> ... in quantum mechanics, we are not dealing with an arbitrary renunciation of a more detailed analysis of atomic phenomena, but with a recognition that such an analysis is in principle excluded. The peculiar individuality of the quantum effects presents us, as regards the comprehension of well-defined evidence, with a novel situation unforeseen in classical physics and irreconcilable with conventional ideas suited for our orientation and adjustment to ordinary experience. It is in this respect that quantum theory has called for a renewed revision of the foundation for the unambiguous use of elementary concepts, as a further step in the development which, since the advent of relativity theory, has been so characteristic of modern science."
>
> ... *Bohr:* [**Boh49**].

Although Bohr appears to express the need for, and appreciation of, intuitively unobjectionable foundations for quantum mechanics, his concerns seem, however, to address only one half of human intellectual endeavour.

- This half would (in the sense of §23.1 (1)):
  - — first, be the attempt to individually express, within a symbolic language:
    - - an instantaneous state (say, for instance, a hypothetical brain scan corresponding to the instantaneous tape description of a Turing machine as detailed in §12.3),
    - - of the synaptic elements, of the dynamically evolving, neuronic, activity,
    - - that can be taken to faithfully represent the physical state of an individual brain at any instant of time; and
  - — second, be the subsequent attempt, to individually interpret, and relate, such symbols of a language to:
    - - the instantaneous state (hypothetical scan) of the synaptic elements, of the dynamically evolving, neuronic, activity of the individual's brain that can be taken to correspond to a 'reading' of the symbols; and
    - - the cognition of a faithful correspondence with the memory (hypothetical scan) of a past experience in an individual's brain.

We may, reasonably, conjecture that what exercised Einstein was:

- The other half of human intellectual activity, which would be that of determining which, of the concepts that are represented by such expressions, can be communicated uniformly in an unambiguous, and effective, manner that is independent of individual interpretations (in the sense of §23.1 (2)).

As Bohr notes further, Einstein argues that:

> "...the quantum-mechanical description is to be considered merely as a means of accounting for the average behaviour of a large number of atomic systems and his attitude to the belief that it should offer an exhaustive description of the individual phenomena is expressed in the following words: 'To believe this is logically possible without contradiction; but it is so very contrary to my scientific instinct that I cannot forego the search for a more complete conception'."
>
> ... *Bohr:* [**Boh49**].

28.3. Einstein admits complete description

In response, Einstein held that:

> "I am, in fact, firmly convinced that the essentially statistical character of contemporary quantum theory is solely to be ascribed to the fact that this [theory] operates with an incomplete description of physical systems. ...
>
> What does not satisfy me in that theory, from the standpoint of principle, is its attitude towards that which appears to me to be the programmatic aim of all physics: the complete description of any (individual) real situation (as it supposedly exists irrespective of any act of observation or substantiation). ...
>
> Now we raise the question: Can this theoretical description be taken as the complete description of the disintegration of a single individual atom? The immediately plausible answer is: No. For one is, first of all, inclined to assume that the individual atom decays at a definite time; however, such a definite time-value is not implied in the description by the Ψ-function. If, therefore, the individual atom has a definite disintegration time, then as regards the individual atom its description by means of the Ψ-function must be interpreted as an incomplete description. In this case the Ψ-function is to be taken as the description, not of a singular system, but of an ideal ensemble of systems. In this case one is driven to the conviction that a complete description of a single system should, after all, be possible, but for such complete description there is no room in the conceptual world of statistical quantum theory. ...
>
> One may not merely ask: 'Does a definite time instant for the transformation of a single atom exist?' but rather: 'Is it, within the framework of our theoretical total construction, reasonable to posit the existence of a definite point of time for the transformation of a single atom?' One may not even ask what this assertion means. One can only ask whether such a proposition, within the framework of the chosen conceptual system—with a view to its ability to grasp theoretically what is empirically given—is reasonable or not. ...
>
> Roughly stated the conclusion is this: Within the framework of statistical quantum theory there is no such thing as a complete description of the individual system. More cautiously it might be put as follows: The attempt to conceive the quantum-theoretical description as the complete description of the individual system leads to unnatural theoretical interpretations, which become immediately unnecessary if one accepts the interpretation that the description refers to ensembles of systems and not to individual systems. In that case the whole 'egg-walking' performed in order to avoid the 'physically real' becomes superfluous. There exists, however, a simple psychological reason for the fact that this most nearly obvious interpretation is being

shunned. For if the statistical quantum theory does not pretend to describe the individual system (and its development in time) completely, it appears unavoidable to look elsewhere for a complete description of the individual system; in doing so it would be clear from the very beginning that the elements of such a description are not contained within the conceptual scheme of the statistical quantum theory. With this one would admit that, in principle, this scheme could not serve as the basis of theoretical physics. Assuming the success of efforts to accomplish a complete physical description, the statistical quantum theory would, within the framework of future physics, take an approximately analogous position to the statistical mechanics within the framework of classical mechanics. I am rather firmly convinced that the development of theoretical physics will be of this type; but the path will be lengthy and difficult."

... *Einstein:* [**Ei49**].

28.4. Do Ψ-functions represent hidden, non-algorithmic, functions?

So, a reasonable view would be that Einstein's objections were not so much against a probabilistic interpretation of quantum mechanics—since it is unarguably effective as a scientific theory—but:

- First, against the absence of suitably intuitive interpretations of such probabilities; and
- Second, against the denial of a need for intuitively unobjectionable axiomatic foundations for the theory since, without these, its formal assertions cannot be treated as being capable of unambiguous, and effective, communication under interpretation by an intelligence—organic or mechanical.

Now, a thesis of this investigation is that the acceptance of non-standard interpretations of Peano Arithmetic and, implicitly, of counter-intuitive interpretations of quantum mechanics, are both aesthetically unappealing consequences of a failure to define *evidence-based* mathematical satisfaction and truth; this would, reasonably, prevent the postulation of unique values for the outcome of gedanken experiments that are based on standard interpretations of classical mathematics.

In this investigation we have shown that if we eliminate this lacuna, and define *evidence-based* mathematical satisfaction and truth we can, indeed, arrive at constructive interpretations of Peano Arithmetic which are intuitive, isomorphic, and verifiably complete.

Hence, it is not unreasonable to conjecture that intuitive, isomorphic, interpretations of quantum mechanical concepts may also follow, in which the functions (which would include relations treated as Boolean functions) that are represented by the Ψ-function are algorithmically *verifiable*, but not algorithmically *computable*.

A feature of such functions would be that, first, they cannot be introduced explicitly as primitive symbols into any recursively definable axiomatic theory without inviting inconsistency; and, second, that although they are algorithmically uncomputable, there is always some effective method for determining their value for any given set of values of their free variables—which would correspond to a measurement, or collapse of the Ψ-function, for that particular set of values.

A consequence of the first is that such, algorithmically *uncomputable* but algorithmically *verifiable*, functions can only be represented in a recursively definable axiomatic theory through their values, and that, given any finite set of such values, based on a sequence of measurements, there are denumerable arithmetic functions that could generate the measured set in the theory.

The question, thus, as to which particular non-algorithmic function gave rise to a particular set of values, and so prediction of the value at a subsequent measurement, cannot, therefore, be determined uniquely within the theory, although algorithmically computable probabilities associated with a particular determination may be possible, in the interpretation, if such probabilities refer to events that are predestined, but the measurement of whose outcomes depend on algorithmically *verifiable*, but not algorithmically *computable*, inter-actions that are yet to unfold, and which involve the entire universe of particles—a not unreasonable assumption if we posit a Big Bang where the universe emerges from a single point of discontinuity (such as, for instance, in the the gedanken considered in §25.1) beyond which nothing can be 'known', and must, therefore, always remain inter-connected everywhere in some—essentially 'unknowable'—sense as conjectured in this June 22, 2017, Nautilus article 'What Is Space' by Jorge Cham and Daniel Whiteson.

Such functions could, thus, effectively be treated as the 'hidden functions' of quantum mechanics. In other words, Ψ-functions, like the Gödel β-functions that can represent a recursive function within a Peano Arithmetic (cf. [Me64], p131, Propositions 3.21-3.23), may simply, then, be formal manifestations of such 'hidden functions'; speculated upon, for instance, by physicist Diederik Aerts in [**Ae98**] as 'hidden measurements' (see also [**AABG**]):

> "In the hidden measurement formalism that we develop in Brussels we explain the quantum structure as due to the presence of two effects, (a) a real change of state of the system under influence of the measurement and, (b) a lack of knowledge about a deeper deterministic reality of the measurement process. We show that the presence of these two effects leads to the major part of the quantum mechanical structure of a theory describing a physical system where the measurements to test the properties of this physical system contain the two mentioned effects. We present a quantum machine, where we can illustrate in a simple way how the quantum structure arises as a consequence of the two effects. We introduce a parameter ϵ that measures the amount of the lack of knowledge on the measurement process, and by varying this parameter, we describe a continuous evolution from a quantum structure (maximal lack of knowledge) to a classical structure (zero lack of knowledge). We show that for intermediate values of ϵ we find a new type of structure that is neither quantum nor classical. We analyze the quantum paradoxes in the light of these findings and show that they can be divided into two groups: (1) The group (measurement problem and Schrödinger's cat paradox) where the paradoxical aspects arise mainly from the application of standard quantum theory as a general theory (e.g. also describing the measurement apparatus). This type of paradox disappears in the hidden measurement formalism. (2) A second group collecting the paradoxes connected to the effect of non-locality (the Einstein-Podolsky-Rosen paradox and the violation of Bell inequalities). We show that these paradoxes are internally resolved because the effect of non-locality turns out to be a fundamental property of the hidden measurement formalism itself."
> *... Aerts:* [**Ae98**]*, Abstract.*

28.5. Is a deterministic but not predictable universe consistent?

The question arises: Are our current theories of physics consistent with the concept of a universe that is completely deterministic, yet not predictable?

In other words, can the initial conditions and all physical laws at any instant, say, for instance, at the time of a projected Big Bang, be knowable completely in a manner that is consistent with our current theories of physics?

28.6. Is our universe deterministic?

As mathematician Ian Stewart observes ([**St97**], p329), the post-quantum belief that our universe may be deterministic in a yet unknown, but fundamental, way (which may not necessarily be predestined) is reflected in Einstein's remarks, in the following excerpts from letters to Max Born:

> "Quantum mechanics is certainly imposing. But an inner voice tells me that it is not yet the real thing. The theory says a lot, but does not really bring us any closer to the secret of the 'old one'. I, at any rate, am convinced that *He* is not playing at dice."
>
> *. . . Einstein:* [**Bor71**]*, Letter #50 (4^{th} December 1926), p.90.*

> "You believe in the God who plays dice, and I in complete law and order in a world which objectively exists, and which I, in a wildly speculative way, am trying to capture. I firmly *believe*, but I hope that someone will discover a more realistic way, or rather a more tangible basis than it has been my lot to do. Even the great initial success of the quantum theory does not make me believe in the fundamental dice game ... ."
>
> *. . . Einstein:* [**Bor71**]*, Letter #71 (7^{th} September 1944), p.149.*

28.7. Is quantum mechanics 'irreducibly probabilistic'?

Whilst noting the prevalent view that, despite Einstein's predilections, the universe, or at least our present quantum mechanical description of it, is of an "irreducibly probabilistic character", Stewart suggests we may need to seriously consider the:

". . . possibility of changing the theoretical framework of physics altogether, replacing quantum uncertainty by deterministic chaos, as Einstein would have liked".

> "Chaos was unknown in Einstein's days, but it was the kind of concept he was seeking. Ironically, the very image of chance as a rolling cube is deterministic and classical, not quantum. And chaos is primarily a concept of classical mechanics. How does the discovery of chaos affect quantum mechanics, and what support—or otherwise—does it offer for Einstein's philosophy? Answers to these questions are, for the moment at least, highly speculative. There is some interest among physicists in what they call 'quantum chaos', but quantum chaos is about the relations between non-chaotic quantum systems and chaotic classical approximations—not chaos as a mechanism for quantum indeterminacy. Quantum chaos . . . is the possibility of changing the theoretical framework of quantum mechanics altogether, replacing quantum uncertainty by deterministic chaos, as Einstein would have liked.
>
> It must be admitted at the outset that the vast majority of physicists see no reason to make changes to the current framework of quantum mechanics, in which quantum events have an irreducibly probabilistic character. Their view is: 'If it ain't broke, don't fix it.' However hardly any philosophers of science are at ease with the conventional interpretation of quantum mechanics, on the grounds that that it is philosophically incoherent, especially regarding the key concept of an observation. Moreover, some of the world's foremost physicists agree with the philosophers. They think that something *is* broke, and therefore needs fixing. It may not be necessary to tinker with quantum mechanics itself: it may be that all we need is a deeper kind of background mathematics that explains why the probabilistic point of view works, much as Einstein's concept of curved space explained Newtonian gravitation."

... Stewart: [**St97**], *p.330.*

We shall now argue that such a 'deeper kind of background mathematics' could lie in recognising that the mathematical expression of classical and quantum mechanics may need two complementary Logics.

CHAPTER 29

Could resolving ***EPR*** need two complementary Logics?

We presume some familiarity with the *EPR* paradox and other perceived contradictions between classical and quantum mechanical descriptions of physical phenomena, and show how these might dissolve if a physicist could cogently argue that:

(i) All properties of physical reality are deterministic, but not necessarily mathematically predetermined—in the sense that any physical property could have one, and only one, value at any time $t(n)$, where the value is completely determined by some natural law which need not, however, be representable by algorithmically computable expressions (and therefore be mathematically predictable);

(ii) There are elements of such a physical reality whose properties at any time $t(n)$ are determined completely in terms of their putative properties at some earlier time $t(0)$.

Such properties are predictable mathematically since they are representable by algorithmically computable functions. The values of any two such functions with respect to their variables are, by definition, independent of each other and must, therefore, obey Bell's inequality.

The Laws of Classical Mechanics determine the nature and behaviour of such physical reality only, and circumscribe the limits of reasoning and cognition in any emergent mechanical intelligence;

(iii) There could be elements of such a physical reality whose properties at any time $t(n)$ cannot be theoretically determined completely from their putative properties at some earlier time $t(0)$.

Such properties are unpredictable mathematically since they are only representable mathematically by algorithmically *verifiable*, but not algorithmically *computable*, functions. The values of any two such functions with respect to their variables may, by definition, be dependent on each other and need not, therefore, obey Bell's inequality.

The Laws of Quantum Mechanics determine the nature and behaviour of such physical reality, and circumscribe the limits of reasoning and cognition in any emergent humanlike intelligence.

In other words, we shall argue that the finitary, agnostic, perspective developed in §3.3 to §27.3 may be the appropriate one from which to view the anomalous philosophical issues underlying some current concepts of quantum phenomena, such as:

- Indeterminacy;
- Bell's inequalities;
- The *EPR* paradox;
- Fundamental dimensionless constants;
- Conjugate properties;
- Entanglement;
- Schrödinger's cat paradox.

29.1. The Copenhagen interpretation of Quantum Theory

We begin by briefly reviewing that, amongst the philosophically disturbing features of the standard Copenhagen interpretation of Quantum Theory, are:

— its essential indeterminateness;

> "It is a general principle of orthodox formulations of quantum theory that measurements of physical quantities do not simply reveal pre-existing or predetermined values, the way they do in classical theories. Instead, the particular outcome of the measurement somehow "emerges" from the dynamical interaction of the system being measured with the measuring device, so that even someone who was omniscient about the states of the system and device prior to the interaction couldn't have predicted in advance which outcome would be realized".
>
> ... *Goldstein et al:* [**Sh+11**].

— which was illustrated dramatically by Erwin Schrödinger's caustic observation regarding the philosophical consequences of the proposed mathematical interpretation of the ψ-function if taken to imply that the objective state of nature is essentially probabilistic;

> "One can even set up quite ridiculous cases. A cat is penned up in a steel chamber, along with the following device (which must be secured against direct interference by the cat): in a Geiger counter there is a tiny bit of radioactive substance, so small, that perhaps in the course of the hour one of the atoms decays, but also, with equal probability, perhaps none; if it happens, the counter tube discharges and through a relay releases a hammer which shatters a small flask of hydrocyanic acid. If one has left this entire system to itself for an hour, one would say that the cat still lives if meanwhile no atom has decayed. The ψ-function of the entire system would express this by having in it the living and dead cat (pardon the expression) mixed or smeared out in equal parts".
>
> ... *Schrödinger:* [**Sc35**], *§5.*

— and its separation of the world into 'system' and 'observer' (cf. [**Sh+11**]).

In 1935 Albert Einstein, Boris Podolsky and Nathan Rosen noted ([**EPR35**]) that accepting Quantum Theory, but denying these features of the Copenhagen interpretation, logically entails accepting:

— either that the world is non-local (thus contradicting Special Relativity);

> "'Non-local' ... means that there exist interactions between events that are too far apart in space and too close together in time for the events to be connected even by signals moving at the speed of light".
>
> ... *Goldstein et al:* [**Sh+11**].

— or that there are hidden variables which would eliminate the need for accepting these features as necesary to any sound interpretation of Quantum Theory.

> "Traditionally, the phrase 'hidden variables' is used to characterize any elements supplementing the wave function of orthodox quantum theory. ...
>
> This terminology is, however, particularly unfortunate in the case of the de Broglie-Bohm theory, where it is in the supplementary variables—definite particle positions—that one finds an image of the manifest world of ordinary experience".
>
> ... *Goldstein et al:* [**Sh+11**].

In 1952 David Bohm proposed ([**Bo52**]) an alternative mathematical development of the existing Quantum Theory, which was essentially equivalent to it but based on Louis de Broglie's pilot wave theory.

However, even though Bohm's interpretation eliminated the need for indeterminism and the separation of the world into 'system' and 'observer', it appealed unappealingly to hidden variables and, presumably, hidden natural laws that—we may reasonably presume further—were implicitly assumed by Bohm to be representable in principle by well-defined classically computable mathematical functions (which could be considered as having pre-existing or predetermined mathematical values over the domain over which the functions are well-defined).

Moreover, experiments designed to test whether John Stewart Bell's mathematical inequalities (in [**Bl64**]) are consistent with observational data, showed conclusively that any interpretation of Quantum Theory which appeals to (presumably classically computable) hidden variables and functions in the above sense must necessarily be non-local.

> "... In the seventies, a sequence of experiments was carried out to test for the presence of nonlocality in the microworld described by quantum mechanics (Clauser 1976; Faraci at al. 1974; Freeman and Clauser 1972; Holt and Pipkin 1973; Kasday, Ullmann and Wu 1970) culminating in decisive experiments by Aspect and his team in Paris (Aspect, Grangier and Roger, 1981, 1982). They were inspired by three important theoretical results: the EPR Paradox (Einstein, Podolsky and Rosen, 1935), Bohms thought experiment (Bohm, 1951), and Bells theorem (Bell 1964).
>
> Einstein, Podolsky, and Rosen believed to have shown that quantum mechanics is incomplete, in that there exist elements of reality that cannot be described by it (Einstein, Podolsky and Rosen, 1935; Aerts 1984, 2000). Bohm took their insight further with a simple example: the 'coupled spin-$\frac{1}{2}$ entity' consisting of two particles with spin $\frac{1}{2}$, of which the spins are coupled such that the quantum spin vector is a nonproduct vector representing a singlet spin state (Bohm 1951). It was Bohm's example that inspired Bell to formulate a condition that would test experimentally for incompleteness. The result of his efforts are the infamous Bell inequalities (Bell 1964). The fact that Bell took the EPR result literally is evident from the abstract of his 1964 paper:
>
>> "The paradox of Einstein, Podolsky and Rosen was advanced as an argument that quantum theory could not be a complete theory but should be supplemented by additional variables. These additional variables were to restore to the theory causality and locality. In this note that idea will be formulated mathematically and shown to be incompatible with the statistical predictions of quantum mechanics. It is the requirement of locality, or more

> precisely that the result of a measurement on one system be unaffected by operations on a distant system with which it has interacted in the past, that creates the essential difficulty."

> Bell's theorem states that statistical results of experiments performed on a certain physical entity satisfy his inequalities if and only if the reality in which this physical entity is embedded is local. He believed that if experiments were performed to test for the presence of nonlocality as predicted by quantum mechanics, they would show quantum mechanics to be wrong, and locality to hold. Therefore, he believed that he had discovered a way of showing experimentally that quantum mechanics is wrong. The physics community awaited the outcome of these experiments. Today, as we know, all of them agreed with quantum predictions, and as a consequence, it is commonly accepted that the micro-physical world is incompatible with local realism."
>
> *. . . Aerts, Aerts, Broekaert and Gabora:* [**AABG**]*, Introduction.*

However, our above investigations into the (apparently unrelated) area of evidence-based and finitary interpretations of the first order Peano Arithmetic PA now suggest that:

— If our above presumption concerning an implicit appeal by Bohm and Bell to functions that are implicitly assumed to be classically computable is correct,

— then the hidden variables in the Bohm-de Broglie interpretation of Quantum Theory could as well be presumed to involve natural laws which are mathematically representable only by functions that are algorithmically verifiable, but not algorithmically computable (hence mathematically determinate but unpredictable),

— in which case Bohm's interpretation need not obey Bell's inequalities and might, therefore, avoid being held as admitting 'non-locality' by Bell's reasoning.

29.2. The underlying perspective of this thesis

The underlying perspective of this thesis is that:

(1) Classical physics assumes that all the observable laws of nature can be mathematically represented in terms of well-defined functions that are algorithmically computable.

- Since the functions are well-defined, their values are pre-existing and predetermined as mappings that are capable of being known in their infinite totalities to an omniscient intelligence, such as Laplace's *intellect Li*, even *before* the events that the functions describe unfold.

> "We may regard the present state of the universe as the effect of its past and the cause of its future. An intellect which at any given moment knew all the forces that animate Nature and the mutual positions of the beings that comprise it, if this intellect were vast enough to submit its data to analysis, could condense into a single formula the movement of the greatest bodies of the universe and that of the lightest atom: for such an intellect nothing could be uncertain; and the future just like the past would be present before its eyes."
>
> *. . . Laplace: A Philosophical Essay on Probabilities.*

(2) However, the overwhelming experimental verification of the mathematical predictions of Quantum Theory suggests that the actual behavior of the real world cannot be assumed as pre-existing and predetermined in this Laplacian sense.

  – In other words, the values of functions that describe the consequences of some experimental interactions are theoretically incapable of being completely known in advance even to an omniscient intelligence, such as Laplace's *intellect Li*, until *after* the events that the functions describe unfold.

  So all the observable laws of nature cannot be represented mathematically in terms of functions that are algorithmically computable.

(3) It follows that:

  (a) Either there is no way of representing all the observable laws of nature mathematically in a deterministic model;

  (b) Or all the observable laws of nature *can* be represented mathematically in a deterministic model—but in terms of functions that, minimally, need only be algorithmically *verifiable*.

(4) The Copenhagen interpretation appears to opt for option (3)(a), and hold that there is no way of representing all the observable laws of nature mathematically in a deterministic model.

  – In other words, the interpretation is not overly concerned with the seemingly essential non-locality of Quantum Theory, and its conflict with the deterministic mathematical representation of the laws of Special Relativity.

(5) The Bohm-de Broglie interpretation appears to reject option (3)(a), and to propose a way of representing all the observable laws of nature mathematically in a deterministic model and, presumably, in terms of functions that are taken implicitly to be algorithmically *computable*.

  – However, the Bohm-de Broglie interpretation has not so far been viewed as being capable of mathematically avoiding the seemingly essential non-local feature of Quantum Theory implied by Bell's inequalities.

(6) In this investigation we therefore propose option (3)(b); i.e., that the apparently non-local feature of Quantum Theory may actually be indicative of a non-constructive and 'counter intuitive-to-human-intelligence' phenomena in nature that could, however, be mathematically represented by functions that:

  — are algorithmically *verifiable* (Definition 5.2);

  — but not algorithmically *computable* (Definition 5.3).

29.3. The *EPR* paradox

We shall now argue that the *EPR* paradox is essentially a mathematical argument whose paradoxical conclusion merely reflects the implicit mathematical ambiguity in interpreting quantification (highlighted in Chapter 21 and §4.3), and whose roots lie in the assumption of conventional Gödelian wisdom that (cf. Tarski's Theorem in §4.3):

— The 'true' sentences of a theory $T(\mathcal{U})$ cannot be defined algorithmically by any logic of the formal language $\mathcal{L}$ of the theory $T(\mathcal{U})$,

— but are an essential feature of the structure $\mathcal{U} =< A, \alpha >$,

— which is defined by a non-empty domain A, and an algebra α defined over A.

However, we hold that such a non-constructive perspective implicitly implies that the concept of 'truth' must then 'exist' Platonically, in the sense of needing to be discovered by some witness-dependent means—eerily akin to a 'revelation'—if the domain A is infinite.

29.4. Truth-values must be a computational convention

We therefore adopt the constructive perspective of §21.2 that:

— The 'true' sentences of a theory $T(\mathcal{U})$ must be defined as objective assignments,

— by a computational convention that is witness-independent,

— in terms of the Tarskian 'satisfaction' and 'truth' of the corresponding formulas, over the structure $\mathcal{U}$,

— of the formal language $\mathcal{L}$ of $T(\mathcal{U})$ under a constructive interpretation.

29.5. Chaitin's constants

We then note that:

(i) All the mathematically defined functions known to, and used by, the applied sciences are algorithmically *computable*, including those that define transcendental numbers such as π, e, etc. They can be computed algorithmically as they are all definable as the limit of some well-defined infinite series of rationals.

(ii) The existence of mathematical constants that are defined by functions which are algorithmically *verifiable* but not algorithmically *computable*—suggested most famously by Georg Cantor's diagonal argument—has been a philosophically debatable deduction.

Such existential deductions have been viewed with both suspicion and scepticism by scientists such as Henri Poincaré, L. E. J. Brouwer, etc., and disputed most vociferously on philosophical grounds by Ludwig Wittgenstein ([**Wi78**]).

(iii) A constructive definition of an arithmetical Boolean function $[R(x)]$ that is true—hence algorithmically *verifiable*—but not provable in Peano Arithmetic—hence algorithmically *uncomputable* (Corollary 11.5)—was given by Kurt Gödel in his 1931 paper on formally undecidable arithmetical propositions ([**Go31**]).

(iv) The definition of a number-theoretic function that is algorithmically verifiable but not algorithmically computable was also given by Alan Turing in his 1936 paper on computable numbers ([**Tu36**]).

He defined a halting function, say $H(n)$, that is 0 if, and only if, the Turing machine with code number n halts on input n. Such a function is mathematically well-defined, but assuming that it defines an algorithmically *computable* real number leads to a contradiction, Turing concluded the mathematical existence of algorithmically *uncomputable* real numbers.

(v) A definition of a number-theoretic function that is algorithmically *verifiable* but not algorithmically *computable* was given by Gregory Chaitin ([**Ct82**]); he defined a class of constants—denoted by Ω—which is such that if $C(n)$ is the n^{th} digit in the decimal expression of an Ω constant, then the function $C(x)$ is algorithmically *verifiable* but not algorithmically *computable*.

29.6. Physical constants

Similarly, since a consequence of the Provability Theorem for PA (Theorem 10.2) is that a PA formula can denote only algorithmically computable constants (Theorem 11.10), some physical constants may be representable by real numbers which are definable only by algorithmically *verifiable* but not algorithmically *computable* functions (compare with §5.3, which addresses Brouwer's perspective of such functions).

This is suggested by the following perspective of one of the challenging issues in physics, which seeks to theoretically determine the magnitude of some fundamental dimensionless constants:

> "... the numerical values of dimensionless physical constants are independent of the units used. These constants cannot be eliminated by any choice of a system of units. Such constants include:
>
> - α, the fine structure constant, the coupling constant for the electromagnetic interaction ($\approx 1/137.036$). Also the square of the electron charge, expressed in Planck units. This defines the scale of charge of elementary particles with charge.
> - μ or β, the proton-to-electron mass ratio, the rest mass of the proton divided by that of the electron (≈ 1836.15). More generally, the rest masses of all elementary particles relative to that of the electron.
> - α_s, the coupling constant for the strong force (≈ 1)
> - αG, the gravitational coupling constant ($\approx 10^{-38}$) which is the square of the electron mass, expressed in Planck units. This defines the scale of the mass of elementary particles.
>
> At the present time, the values of the dimensionless physical constants cannot be calculated; they are determined only by physical measurement. This is one of the unsolved problems of physics. ...
>
> The list of fundamental dimensionless constants decreases when advances in physics show how some previously known constant can be computed in terms of others. A long-sought goal of theoretical physics is to find first principles from which all of the fundamental dimensionless constants can be calculated and compared to the measured values. A successful 'Theory of Everything' would allow such a calculation, but so far, this goal has remained elusive."
>
> ... *Dimensionless physical constant - Wikipedia*

From the perspective of Theorem 11.10 we could thus suggest that:

THESIS 29.1. Some of the dimensionless physical constants are only representable in a mathematical language as real numbers that are defined by functions which are algorithmically *verifiable*, but not algorithmically *computable*.

In other words, we cannot treat such constants as denoting—even in principle—a measurable limit, as we could a constant that is representable mathematically by a real number that is definable by algorithmically *computable* functions.

29.7. Completed Infinities

From the point of view of mathematical philosophy, this distinction would be intuitively expressed by the assertion that:

- Whilst a symbol for an 'unmeasurable' physical constant may be introduced into a physical theory as a primitive term without inviting inconsistency in the theory (a consequence of Theorem 19.4), the sequence of digits in the decimal representation of the 'measure' of an 'unmeasurable' physical constant *cannot* be treated in the mathematical language of the theory as a 'completed' infinite sequence;
- Whereas the corresponding sequence in the decimal representation of the 'measure' of a 'measurable' physical constant, when introduced as a primitive term into a physical theory, *can* be treated as a 'completed' infinite sequence in the mathematical language of the theory without inviting inconsistency.

Of interest—particularly in view of Theorem 19.4—is the following perspective on the difficulties of addressing *unfinished* infinities encountered in the mathematical representation of physical phenomena:

> "... we propose that Laplacian determinism be seen in the light of constructive mathematics and Church's Thesis. This means amongst other things that infinite sequences (of natural numbers; a real number is then given by such an infinite sequence) are never 'finished', instead we see them developing in the course of time. Now a very consequent, therefore elegant interpretation of Laplacian determinism runs as follows. Suppose that there is in the real world a developing-infinite sequence of natural numbers, say α. Then how to interpret the statement that this sequence is 'uniquely determined' by the state of the world at time zero? At time zero we can have at most finite information since, according to our constructive viewpoint, infinity is never attained. So this finite information about α supposedly enables us to 'uniquely determine' α in its course of time. It is now hard to see another interpretation of this last statement, than the one given by Church's Thesis, namely that this finite information must be a (Turing-)algorithm that we can use to compute $\alpha(n)$ for any $n \in (N)$.
>
> With classical logic and omniscience, the previous can be stated thus: 'for every (potentially infinite) sequence of numbers $(a_n)_{n\in\mathbb{N}}$ taken from reality there is a recursive algorithm α such that $\alpha(n) = a_n$ for each $n \in \mathbb{N}$. This statement is sometimes denoted as '$\mathbf{CT}_{phys}$', ... this classical omniscient interpretation is easily seen to fail in real life. Therefore we adopt the constructive viewpoint. The statement 'the real world is deterministic' can then best be interpreted as: 'a (potentially infinite) sequence of numbers $(a_n)_{n\in\mathbb{N}}$ taken from reality cannot be apart from every recursive algorithm α (in symbols: $\neg\forall\alpha \in \sigma_{\omega REC}\exists n \in \mathbb{N}\ [\alpha(n) \neq a_n]$)'."
>
> ... *Waaldijk:* [**Wl03**], *§7.2, p.24.*

29.8. Zeno's argument

We note that Zeno's paradoxical arguments ([**Rus37**], pp.347-353) highlight the philosophical and theological dichotomy (addressed in another context in §24.5 and §25) between our essentially 'continuous' perception of the physical reality that we seek to capture with our measurements, and the essential 'discreteness' of any mathematical language of Arithmetic in which we seek to express such measurements.

The distinction between *algorithmic verifiability* and *algorithmic computability* of Arithmetical functions could be seen as reflecting the dichotomy mathematically.

29.9. Classical laws of nature

For instance, the distinction suggests that classical mechanics *could* be held as complete, and certain in the sense of being predictable, with respect to the *algorithmically computable* representations of physical phenomena:

THESIS 29.2. Classical laws of nature determine the nature and behaviour of all those properties of the physical world which are mathematically describable completely at any moment of time $t(n)$ by *algorithmically computable* functions from a given initial state at time $t(0)$.

29.10. Neo-classical laws of nature

On the other hand, the distinction also suggests that quantum mechanics *could* be held as *essentially* incompletable, and uncertain in the sense of being *essentially* unpredictable, with respect to the *algorithmically verifiable* representation of physical phenomena:

THESIS 29.3. Neo-classical laws of nature determine the nature and behaviour of those properties of the physical world which are describable completely at any moment of time $t(n)$ by *algorithmically verifiable* functions; however such properties are not completely describable by *algorithmically computable* functions from any given initial state at time $t(0)$.

A putative model for such behaviour is speculated upon by Waaldijk:

> "The second way to model our real world is to assume that it is deterministic. ... It would be worthwhile to explore the consequences of a deterministic world with incomplete information (since under the assumption of determinancy in the author's eyes this comes closest to real life). That is a world in which each infinite sequence is given by an algorithm, which in most cases is completely unknown. We can model such a world by introducing two players, where player I picks algorithms and hands out the computed values of these algorithms to player II, one at a time. Sometimes player I discloses (partial) information about the algorithms themselves. Player II can of course construct her or his own algorithms, but still is confronted with recursive elements of player I about which she/he has incomplete information".
>
> ... *Waaldijk:* [**Wl03**], *§1.5, p.5.*

Since such behaviour follows fixed laws and is determinate (even if not algorithmically predictable by classical laws), Albert Einstein could have been justified in the belief oft ascribed to him as 'God doesn't play dice with the world':

> "Einstein was not prepared to let us do what, to him, amounted to pulling the ground from under his feet. Later in life, also, when quantum theory had long since become an integral part of modern physics, Einstein was unable to change his attitude-at best, he was prepared to accept the existence of quantum theory as a temporary expedient. 'God does not throw dice' was his unshakable principle, one that he would not allow anybody to challenge. To which Bohr could only counter with: 'Nor is it our business to prescribe to God how He should run the world'."
>
> ... *Heisenberg:* [**Hei71**]

29.11. Incompleteness: Arithmetical analogy

The distinction also suggests that neither classical mechanics nor neo-classical quantum mechanics could be described as 'mathematically complete' with respect to the algorithmically verifiable behaviour of the physical world.

The analogy here is that Gödel showed in 1931 ([**Go31**]) that any formal arithmetic is not mathematically complete with respect to the algorithmically *verifiable* nature and behaviour of the natural numbers (which—as shown in Chapter §7—is the behaviour sought to be captured by the standard interpretation of PA).

We have, of course, shown that the first-order Peano Arithmetic PA *is* categorical (Corollary 11.1)—hence complete—with respect to the algorithmically *computable* nature and behaviour of the natural numbers.

In this sense, the *EPR* paper may not be entirely wrong in holding that:

> "We are thus forced to conclude that the quantum-mechanical description of physical reality given by wave functions is not complete."
>
> ... *Einstein, Podolsky and Rosen:* [**EPR35**]

29.12. Conjugate properties

The above also suggests that:

THESIS 29.4. The nature and behaviour of two conjugate properties F_1 and F_2 of a particle P that are determined by neo-classical laws are described mathematically at any time $t(n)$ by two algorithmically verifiable, but not algorithmically computable, functions f_1 and f_2.

In other words, it is the very essence of the neo-classical laws determining the nature and behaviour of the particle that—at any time $t(n)$—we can only determine either $f_1(n)$ or $f_2(n)$, but not both.

Hence measuring either one makes the other indeterminate as we cannot go back in time. This does not contradict the assumption that any property of an object must obey some deterministic natural law for any possible measurement that is made at any time.

29.13. Entangled particles

The above similarly suggests that:

THESIS 29.5. The nature and behaviour of an entangled property of two particles P and Q is determined by neo-classical laws, and are describable mathematically at any time $t(n)$ by two algorithmically verifiable—but not algorithmically computable—functions f_1 and g_1.

In other words, it is the very essence of the neo-classical laws determining the nature and behaviour of the entangled properties of two particles that—at any time $t(n)$—determining the state of one immediately gives the state of the other without measurement if the properties are entangled in a known manner.

This does not contradict the assumption that any property of an object must obey some deterministic natural law for any possible measurement that is made at any time. Nor does it require any information to travel from one particle to another consequent to a measurement.

29.14. Schrödinger's cat

If $[F(x)]$ is an algorithmically *verifiable* but not algorithmically *computable* Boolean function, we can take the query:

Query 29.6. Is $F(n) = 0$ for all natural numbers?

as corresponding to the Schrödinger question:

Query 29.7. If a live cat and a radioactive atom are locked in a steel chamber at time t_0, where the cat's life or death depended on whether or not the radioactive atom had decayed and emitted radiation, then can we categorically state that the cat must be either dead or alive at any given time $t > t_0$ without opening the chamber?

We can then argue that there is no mathematical paradox involved in Schrödinger's assertion that the cat is both dead and alive (in the sense of [**Pa08**], §2, Inconsistent beliefs) at any time $t_1 > t > t_0$, where t_1 is the time when the chamber is first opened, if we take this to mean that:

> I may either assume the cat to be alive until a given time t_1 (in the future, when the state of the cat is physically determined for the first time), or assume the cat to be dead until the time t_1, without arriving at any logical contradiction in my existing Quantum description of nature.

In other words:

> Once we accept Quantum Theory as a valid description of nature, then there is no paradox in stating that the theory essentially cannot predict the state of the cat at any moment of future time.
>
> The inability to predict such a state does not arise out of a lack of sufficient information about the laws of the system that Quantum theory is describing, but stems from the very nature of these laws.

The mathematical analogy for the above would be (compare with the concept of '*proximity spaces*' in [**SRP17**], §2):

> Once we accept that Peano Arithmetic is consistent (Theorem 9.10) and categorical (Corollary 11.1)—which means that any two models of the Arithmetic are isomorphic—then we cannot deduce from the axioms and

rules of inference of PA alone (see Theorem 11.7 in §11.2) whether $F(n) = 0$ for all natural numbers, or whether $F(n) = 1$ for some natural number, if $[F(x)]$ is an algorithmically *verifiable* but not algorithmically *computable* Boolean function.

Part 9

The significance of *evidence-based* reasoning for Computational Complexity

CHAPTER 30

A brief review

In a paper: 'The truth assignments that differentiate human reasoning from mechanistic reasoning: The evidence-based argument for Lucas' Gödelian thesis', which appeared in the December 2016 issue of *Cognitive Systems Research* [**An16**], we briefly addressed the philosophical challenge that arises when an intelligence—whether human or mechanistic—accepts arithmetical propositions as true under an interpretation—either axiomatically or on the basis of subjective *self-evidence*—without any specified methodology for objectively *evidencing* such acceptance in the sense of Chetan Murthy and Martin Löb:

> "It is by now folklore ... that one can view the *values* of a simple functional language as specifying *evidence* for propositions in a constructive logic ...".
> *... Chetan. R. Murthy:* [**Mu91**], *§1 Introduction.*
>
> "Intuitively we require that for each event-describing sentence, $\phi_{o^{\iota}} n_{\iota}$ say (i.e. the concrete object denoted by n_{ι} exhibits the property expressed by $\phi_{o^{\iota}}$), there shall be an algorithm (depending on **I**, i.e. M^*) to decide the truth or falsity of that sentence."
> *... Martin H Löb:* [**Lob59**], *p.165.*

Definition 30.1 (Evidence-based reasoning in Arithmetic). Evidence-based reasoning accepts arithmetical propositions as true under an interpretation if, and only if, there is some specified methodology for objectively *evidencing* such acceptance.

The significance of introducing *evidence-based* reasoning for assigning truth values to the formulas of a first-order Peano Arithmetic, such as PA, under a well-defined interpretation (see §3 in [**An16**]), is that it admits the distinction:

(1) algorithmically *verifiable* 'truth' (Definition 30.3); and

(2) algorithmically *computable* 'truth' (Definition 30.4).

Definition 30.2. A deterministic algorithm computes a mathematical function which has a unique value for any input in its domain, and the algorithm is a process that produces this particular value as output[1].

For instance, under *evidence-based* reasoning the formula $[(\forall x)F(x)]$ of the first-order Peano Arithmetic PA must always be interpreted *weakly* under the classical, standard, interpretation of PA (see [**An16**], Theorem 5.6) in terms of algorithmic *verifiability* (see [**An16**], Definition 1); where, if the PA-formula $[F(x)]$ interprets as an arithmetical relation $F^*(x)$ over N:

Definition 30.3. The number-theoretical relation $F^*(x)$ is algorithmically *verifiable* if, and only if, for any natural number n, there is a deterministic algorithm

[1]Note that a deterministic algorithm can be suitably defined as a '*realizer*' in the sense of the *Brouwer-Heyting-Kolmogorov* rules (see [**Ba16**], p.5).

$AL_{(F,\ n)}$ which can provide evidence for deciding the truth/falsity of each proposition in the finite sequence $\{F^*(1), F^*(2), \ldots, F^*(n)\}$.

Whereas $[(\forall x)F(x)]$ must always be interpreted *strongly* under the finitary interpretation of PA (see [**An16**], Theorem 6.7) in terms of algorithmic *computability* ([**An16**], Definition 2), where:

DEFINITION 30.4. The number theoretical relation $F^*(x)$ is algorithmically *computable* if, and only if, there is a deterministic algorithm AL_F that can provide evidence for deciding the truth/falsity of each proposition in the denumerable sequence $\{F^*(1), F^*(2), \ldots\}$.

The significance of the distinction between algorithmically *computable* reasoning based on algorithmically *computable* truth, and algorithmically *verifiable* reasoning based on algorithmically *verifiable* truth, is that it admits the following, hitherto unsuspected, consequences:

(i) PA has two well-defined interpretations over the domain N of the natural numbers (including 0):
   (a) the *weak* non-finitary standard interpretation $I_{PA(N,SV)}$ ([**An16**], Theorem 5.6), and
   (b) a *strong* finitary interpretation $I_{PA(N,SC)}$ ([**An16**], Theorem 6.7);

(ii) PA is *non-finitarily* consistent under $I_{PA(N,SV)}$ ([**An16**], Theorem 5.7);

(iii) PA is *finitarily* consistent under $I_{PA(N,SC)}$ ([**An16**], Theorem 6.8).

The relevance, for this investigation, of distinguishing between algorithmically *verifiable* and algorithmically *computable* number-theoretic functions, as in Definitions 30.3 and 30.4, is that it assures us a formal foundation for placing in perspective, and complementing, an uncomfortably counter-intuitive entailment in number theory—Theorem 30.11—which has been treated by conventional wisdom (see §30.3) as sufficient for concluding that the prime divisors of an integer *cannot* be proven to be mutually independent.

However, we shall show that such informally perceived barriers are, in this instance, illusory (§30.4); and that admitting the above distinction illustrates:

(a) *Why* the prime divisors of an integer *are* mutually independent (Theorem 31.9);

(b) *Why* determining whether the *signature* (Definition 30.5) of a given integer n—coded as the key in a modified Bazeries-cylinder (Definition 31.1) based combination lock—is that of a prime, or not, can be done in polynomial time $O(log_e n)$ (Theorem 32.2); as compared to the time $\ddot{O}(log_e^{15/2} n)$ given by Agrawal et al in [**AKS04**], and improved to $\ddot{O}(log_e^{6} n)$ by Lenstra and Pomerance in [**LP11**], for determining whether the *value* of a given integer n is that of a prime or not.

(c) *Why* it can be cogently argued that determining a factor of a given integer cannot be polynomial time (Hypothesis 32.5).

DEFINITION 30.5. The[2] *signature* of a given integer n is the sequence of residues $< a_{n,i} >$ where $n + a_{n,i} \equiv 0 \ mod \ (p_i)$ for all primes p_i such that $1 \leq i \leq \pi(\sqrt{n})$.

DEFINITION 30.6. The *value* of a given integer n is any well-defined interpretation—over the domain of the natural numbers—of the (unique) numeral $[n]$ that represents n in the first-order Peano Arithmetic PA.

We note that Theorem 32.2 establishes a lower limit for [**AKS04**] and [**LP11**], because determining the *signature* (Definition 30.5) of a given integer n does not require knowledge of the *value* (Definition 30.6) of the integer as defined by the Fundamental Theorem of Arithmetic (Theorem 30.9).

30.1. Are the prime divisors of an integer mutually independent?

We begin by addressing the query:

QUERY 30.7. Are the prime divisors of an integer n mutually independent?

DEFINITION 30.8. Two events are independent if the occurrence of one event does not influence (and is not influenced by) the occurrence of the other.

Prima facie, the prime divisors of an integer intuitively *seem* to be mutually independent by virtue of the Fundamental Theorem of Arithmetic:

THEOREM 30.9. *Every positive integer $n > 1$ can be represented in exactly one way as a product of prime powers:*

$$n = p_1^{n_1} p_2^{n_2} \cdots p_k^{n_k} = \prod_{i=1}^{k} p_i^{n_i}$$

where $p_1 < p_2 < \ldots < p_k$ are primes and the n_i are positive integers.

Moreover, the prime divisors of n can also be *seen* to be mutually independent in the usual, linearly displayed, Sieve of Eratosthenes (see also Chapter 33, Tables 1 and 2), where whether an integer n is crossed out as a multiple of a prime p is *obviously* independent (in the sense of Definition 30.8) of whether it is also crossed out as a multiple of a prime $q \neq p$:

$$E(\cancel{1}),\ E(2),\ E(3),\ E(\cancel{4}),\ E(5),\ E(\cancel{6}),\ E(7),\ E(\cancel{8}),\ E(\cancel{9}),\ E(\cancel{10}),\ E(11),\ \ldots$$

Despite such compelling evidence—which, admittedly, does fall short of the criteria of 'information that we *agree* to *define* as *true* on the basis of a *convention*' in §23.2—conventional wisdom appears to *unreasonably* accept as definitive the counter-intuitive conclusion (addressed in §30.3) that although we can *see* it as true, we cannot mathematically prove the following proposition as *true*:

PROPOSITION 30.10. Whether or not a prime p divides an integer n is independent of whether or not a prime $q \neq p$ divides the integer n.

We note that such an *unprovable-but-intuitively-true* conclusion is *unreasonable* because it makes a *stronger* assumption than that in Gödel's similar claim for his arithmetical formula $[(\forall x)R(x)]$—whose Gödel-number is $17Gen\ r$—in [**Go31**], p.26(2). *Stronger*, since Gödel does not assume his proposition to be *intuitively*

[2]Unique since, if $p^2_{\pi(\sqrt{m})+1} > m \geq p^2_{\pi(\sqrt{m})}$ and $p^2_{\pi(\sqrt{n})+1} > n \geq p^2_{\pi(\sqrt{n})}$ have the same signature, then $|m - n| = c_1 . \prod_{i=1}^{\pi(\sqrt{m})} p_i = c_2 . \prod_{i=1}^{\pi(\sqrt{n})} p_i$; whence $c_1 = c_2 = 0$ since $\prod_{i=1}^{k} p_i > (\prod_{i=2}^{k-2} p_i).p_k^2 > p_{k+1}^2$ for $k > 4$ by appeal to Bertrand's Postulate $2.p_k > p_{k+1}$; and the uniqueness is easily verified for $k \leq 4$.

true, but shows that though the arithmetical formula with Gödel-number $17Gen\ r$ is not provable in his Peano Arithmetic P yet, for any P-numeral $[n]$, the formula $[R(n)]$ whose Gödel-number is $Sb\begin{pmatrix} r & 17 \\ & Z(n) \end{pmatrix}$ *is* P-provable, and therefore *meta-mathematically true* under any well-defined Tarskian interpretation of P (cf., [**An16**], §3.).

Expressed in computational terms (see [**An16**], Corollary 8.3), under any well-defined interpretation of P, Gödel's formula $[R(x)]$ translates as an arithmetical relation, say $R'(x)$, such that $R'(n)$ is algorithmically *verifiable*, but not algorithmically *computable*, as always true over N, since $[\neg(\forall x)R(x)]$ is P-provable ([**An16**], Corollary 8.2).

We thus argue that a perspective which denies Proposition 30.10 is based on perceived barriers that reflect, and are peculiar to, *only* the argument that:

THEOREM 30.11. *There is no deterministic algorithm*[3] *that, for any given n, and any given prime $p \geq 2$, will evidence that the probability $\mathbb{P}(p \mid n)$ that p divides n is $\frac{1}{p}$, and the probability $\mathbb{P}(p \not\mid n)$ that p does not divide n is $1 - \frac{1}{p}$.*

PROOF. By a standard result in the Theory of Numbers ([**Ste02**], Chapter 2, p.9, Theorem 2.1[4]), we cannot define a probability function for the probability that a random n is prime over the probability space $(1, 2, 3, \ldots,)$.

The theorem follows. □

However, such a perspective does not consider the possibility—which we show has significant consequences for the resolution of outstanding problems in both Computational Complexity *and* the Theory of Numbers—that there can be algorithmically *verifiable* number-theoretic functions which are not algorithmically *computable*; and that:

THEOREM 30.12. *For any given n, there is a deterministic algorithm that, given any prime $p \geq 2$, will evidence that the probability $\mathbb{P}(p \mid n)$ that p divides n is $\frac{1}{p}$, and the probability $\mathbb{P}(p \not\mid n)$ that p does not divide n is $1 - \frac{1}{p}$.*

PROOF. The proof follows immediately if we take i as p in Corollary 31.4 and Corollary 31.5. □

30.2. The informal argument for Theorem 30.11

The informal argument that we cannot define a probability function for the probability that a random n is prime over the probability space $(1, 2, 3, \ldots,)$ (Theorem 30.11)—as also for the *belief*[5] that whether or not a prime p divides an integer n is

[3] We note that a deterministic algorithm computes a mathematical function which has a unique value for any input in its domain, and the algorithm is a process that produces this particular value as output. It can be suitably defined as a '*realizer*' in the sense of the *Brouwer-Heyting-Kolmogorov* rules (see [**Ba16**], p.5).

[4] Compare with the informal argument in [**HL23**], pp.36-37; also with that in §30.11.

[5] Which, arguably, falls within the criteria of 'information that we *hold* to be *true*—short of Platonic *belief*—since it *can* be treated as *self-evident*' (see §23.2).

not independent of whether or not a prime $q \neq p$ divides the integer n—is expressed at length in a referee's critique of the author's contrary contention:

> "My objection is quite simply that I don't know what you mean by a randomly given positive integer n. If you want to make sense of it, then you need to assign to each positive integer n a probability $p(n)$. These probabilities must have two properties: that they are non-negative, and that their sum should be 1. If you do that, then you can talk about things like the probability that $m|n$. It will be $\sum_{d=1}^{\infty} p(dm)$.
>
> As an example, setting $p(n) = 2^{-n}$ for $n = 1, 2, 3, \ldots$ would satisfy the conditions for a probability distribution, though obviously this would be an unsuitable choice for your purposes. But the problem is that *every* possible way of choosing the $p(n)$ is unsuitable for your purposes. There does not exist a way of choosing the $p(n)$ such that for every m the equation $\sum_{d=1}^{\infty} p(dm) = 1/m$ holds.
>
> ... Consider first the probability of an unspecified integer n being divisible by an unspecified prime p. Given an arbitrary probability distribution on the positive integers, there will always be some prime p for which the above statement is false.
>
> To see this, suppose that the probability that n is chosen is not zero. Let's write this probability as $q(n)$. Now choose p so large that $1/p$ is less than $q(n)$. Then the probability that the remainder on division by p is n is at least $c(n)$ (since there is a probability $c(n)$ of choosing the integer n) and that is greater than $1/p$.
>
> ... A typical way that number theorists deal with a difficulty like this is to choose a random integer n in the range from N to $2N$ for some large integer N. But then you cannot say that the probability that n is a multiple of p is exactly $1/p$—it is only *approximately* $1/p$. And the various events are not exactly independent but only *approximately* independent. So there are error terms involved. And the entire difficulty of the subject is that these error terms accumulate and it becomes hard to say what the final answer is to any accuracy.
>
> ... Let me explain why what I did say is true. We pick an integer n uniformly at random from the set $\{N, N+1, N+2, , 2N\}$. What is the probability that n is even? If N is odd, then exactly half those integers are odd and half are even.
>
> If N is even, then we can write $N = 2M$, and in that case of the $N+1$ elements of the set, $M+1$ are even and M are odd, so the probability that n is even is $(M+1)/2M$. So that's already an example where the probability is only approximately equal to $1/p$ (which in this case is $1/2$). In general, the number of multiples of p in a set of R consecutive integers will be R/p if p happens to be a factor of R, and otherwise it will be one of the integers on either side of R/p.
>
> In the second case, which has to happen for several p (since R cannot be divisible by every prime less than R, or even than the square root of R), the best we can say is that the probability that an integer chosen uniformly at random from the R consecutive integers is a multiple of p is approximately equal to $1/p$.
>
> ... It is possible to define a notion of "density" for sets of integers in such a way that the density of the set of all integers congruent to a mod p is $1/p$ for every a and every p.
>
> ... It is not possible to define a probability distribution on the integers in such a way that every integer is chosen with equal probability.
>
> .... If you want to claim that you can make sense of the statement:

> 'The probability that an unspecified integer n is divisible by p is $1/p$',

you will need to develop some kind of probability theory that allows you to do something that conventional probability theory (where you would need to specify a probability distribution on the positive integers) does not."

30.3. Conventional wisdom

However, we note that the basis for the conventional wisdom—that whether or not a prime p divides an integer n is *not* independent of whether or not a prime $q \neq p$ divides the integer n—generally appears more *faith-based* than *evidence-based* since, as the following examples show, it is expressed:

(i) either explicitly, but without formal proof:

- "Here is the code of the algorithm. ...the input x is a product of two prime numbers, ϕ is a polynomial in just one variable, and *gcd* refers to the greatest-common-divisor algorithm expounded by Euclid around 300 B.C.

    * Repeat until exit:
    * $a :=$ a random number in $1, \ldots, x-1$;
    * if $gcd(b, x) > 1$ then exit.

  Exiting enables carrying out the two prime factors of x...

  How many iterations must one expect to make through this maze before exit? How and when can the choice of the polynomialϕ speed up the exploration? ...

  Note that we cannot consider the events $b \equiv 0 \ mod(p)$ and $b \equiv 0 \ mod(q)$ to be independent, even though p and q are prime, because $b = \phi a$ and ϕ may introduce bias."

  ... *Regan:* [**Re16**].

- "...the probabilities are not independent. ...The probability that a number n is divisible by a prime p is $1/p$, if concerning n we know only that it is large compared with p. If we know that n is *near* N^2 and *not divisible by any prime smaller than* p, then the probability that n is divisible by p is not $1/p$, but f/p."

  ... *Furry:* [**Fu42**].

- "Prof. E. M. Wright, some months ago, sent me privately a proof on somewhat similar lines that that the probabilities could not be independent for primes greater than $n^{0.76}$."

  ... *Cherwell:* [**Che42**].

- "*Find the probability that x, a large integer chosen at random, is a prime number.* ...If the integer x is not divisible by any prime p which does not exceed $x^{1/2}$, x itself must be a prime—and so divisibility by primes exceeding $x^{1/2}$ is, in fact, *not* independent of the smaller primes."[6]

. . . *Pólya:* [**Pol59**].

(ii) or implicitly, by arguing—as, for instance, in [**Ste02**], Chapter 2, p.9, Theorem 2.1—that a proof to the contrary *must* imply that, if $P(n \textit{ is a prime})$ is the probability that an integer n has the property of being a prime, then $\sum_{i=1}^{\infty} P(i \textit{ is a prime}) = 1$.

30.4. Illusory barriers

However, we shall show in Chapter 31 that the barriers faced by conventional wisdom in addressing Query 30.7 unequivocally are illusory; they dissolve if we differentiate between the following probabilities:

(i) The probability $P_1(n \in \phi)$ of *selecting* an integer that has the property ϕ from a *given* set S of integers;

Example 1: If N is the set of natural numbers, what is the probability of selecting an integer $n \in N$ that has the property of being a prime?

We note that since we cannot define a precise ratio of primes to composites in N, but only an order of magnitude such as $O(\frac{1}{log_e n})$, the probability $P_1(p) \equiv P_1(n \in N \textit{ is a prime})$ of *selecting* an integer that has the property of being a prime obviously cannot be defined in N.

(ii) The probability $P_2(n \in \phi)$ that an *unspecified* integer, in a *given* set S of integers, *has* the property ϕ;

Example 2: If N^+ is the set of positive integers, what is the probability that an unspecified integer $n \in N^+$ secreted in a black box is even?

We note that since any $n \in N^+$ is either odd or even, the probability $P_2(p) \equiv P_2(n \in N^+ \textit{ is even})$ that the *unspecified* integer $n \in N^+$ secreted in the black box *has* the property of being even must be $\frac{1}{2}$.

We note that the probability $P_2(p) \equiv P_2(n \in N^+ \textit{ is even})$ cannot *depend* upon the probability $P_1(p) \equiv P_1(n \in N^+ \textit{ is even})$ of *selecting* an integer $n \in N^+$ that has the property of being even, as the latter would require[7] that $\sum_{i=1}^{\infty} P_2(i \in N^+ \textit{ is even}) = 1$, which is not the case in this example.

Such *dependence* would also appear to eerily echo the *curious* argument—preferred by the Copenhagen interpretation of quantum theory (but shown as violating the principle of Occam's razor in §29.14)—that whether or not the putative cat *is* alive—and not just *known* to be alive—at any moment in Schrödinger's famous *gedanken*, would depend ultimately open whether or not we were to open the *box* at *that* moment!

---

[6]It is not obvious whether Pólya's—rather curious—perspective is unconsidered, or whether it falls within the criteria of 'information that we *hold* to be *true*—short of Platonic *belief*—since it *can* be treated as *self-evident*' (see §23.2).

[7]See Steuding [**Ste02**], Chapter 2, p.9, Theorem 2.1.

(iii) The probability $P_3(n \in \phi)$ of *determining* that a *given* integer n has the property ϕ.

Example 3: I give you a 5*-digit combination lock along with a* 10*-digit integer* n*. The lock only opens if you set the combination to a proper factor of* n *which is greater than* 1*. What is the probability that a given combination will open the lock.*

We note that this is the basis for RSA encryption, which provides the cryptosystem used by many banks for securing their communications.

It is the basis we shall use to illustrate that the probability $P_3(p|n)$ of *determining* that a prime p divides a *given* integer n is $\frac{1}{p}$, and is independent of whether or not a prime $q \neq p$ divides n.

CHAPTER 31

Why the prime divisors of an integer are mutually independent

We define the probability $P_3(p|n)$ of *determining* (in the sense detailed in §30.4(iii)), by the spin of a modified Bazeries Cylinder[1], that a prime p divides a *given* integer n, and show that it is independent of whether or not a prime $q \neq p$ divides n.

Definition 31.1. A modified Bazeries Cylinder is a set of polygonal wheels—not necessarily identical (such as B_i and B_j in Fig. 1 below)—mounted on a common spindle, whose faces are coded with symbols, where the event $B_i(u)$ (Fig 2 below) is the value $0 \leq u \leq i-1$ yielded by a spin of a single i-faced Bazeries wheel B_i, and the event $B_{ij}(u, v)$ (Fig, 3 below) is the value (u, v)—where $0 \leq u \leq i-1$ and $0 \leq v \leq j-1$—yielded by simultaneous, but independent, spins of an i-faced Bazeries wheel B_i and a j-faced Bazeries wheel B_j. □

Fig. 1. An i-faced Bazeries wheel B_i and a j-faced Bazeries wheel B_j.

Hypothesis 31.2. The event yielded by the simultaneous spins of a set of Bazeries wheels is random. □

(1) We consider first, for any *given* $n > i > 1$, the probability $P_3(B_i(u))$—over the probability space $(0, 1, 2, \ldots, i-1)$—of *determining* that the spin of the Bazeries wheel B_i—with faces numbered $0, 1, 2, \ldots, i-1$—yields the event $B_i(u)$.

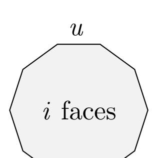

Fig. 2. The event $B_i(u)$ for a single i-faced Bazeries wheel B_i

We conclude by Hypothesis 31.2 that, for any $0 \leq u \leq i-1$:

Lemma 31.3. $P_3(B_i(u)) = \frac{1}{i}$. □

[1]Compare *Bazeries cylinder: https://en.wikipedia.org/wiki/Jefferson_disk*.

Now, if $n \equiv u \ (mod\ i)$ where $i > u \geq 0$, then i divides n if, and only if, $u = 0$. The probability $P_3(i|n)$ of *determining* by the spin of a Bazeries wheel whether i divides n is thus:

COROLLARY 31.4. $P_3(i|n) = P_3(B_i(0)) = \frac{1}{i}$. □

Hence the probability $P_3(i \not| n)$ of similarly *determining* that i does not divide n is:

COROLLARY 31.5. $P_3(i \not| n) = 1 - \frac{1}{i}$. □

(2) We consider next, for any *given* $n > i,\ j > 1$ where $i \neq j$, the compound probability $P_3(B_{ij}(u, v))$ of *determining* whether the simultaneous, but independent, spins of the pair of Bazerian wheels B_i—with faces numbered $0, 1, 2, \ldots, i-1$—and B_j—with faces numbered $0, 1, 2, \ldots, j-1$—yields the event $B_{ij}(u, v)$.

Fig. 3. The event $B_{ij}(u, v)$ for a set of two Bazeries wheels B_i and B_j.

Since the two events $B_i(u)$ and $B_j(v)$ are mutually independent by definition, we conclude by Hypothesis 31.2 that[2]:

LEMMA 31.6. $P_3(B_{ij}(u, v)) = P_3(B_i(u)).P_3(B_j(v)) = \frac{1}{ij}$. □

(3) We conclude further by Hypothesis 31.2, Lemma 31.3, Corollary 31.4, and Lemma 31.6, that:

LEMMA 31.7. $P_3(i|n\ \&\ j|n) = P_3(i|n).P_3(j|n)$ *if, and only if,* $n > i, j > 1$ *and* i, j *are co-prime.* □

PROOF. We note that:

(a) The assumption that i, j be co-prime is sufficient. Thus, if i, j are co-prime, and:

$$n \equiv u \ (mod\ i),\ \ n \equiv v \ (mod\ j),\ \ n \equiv w \ (mod\ ij)$$

where $i > u \geq 0,\ j > v \geq 0,\ ij > w \geq 0$, then the ij integers $v.i + u.j$ are all incongruent and form a complete system of residues[3].

Hence $i|n$ and $j|n$ if, and only if, $u = v = 0$.

It follows that $P_3(i|n\ \&\ j|n) = P_3(B_{ij}(0, 0))$.

By Corollary 31.4, $P_3(i|n) = P_3(B_i(0)) = \frac{1}{i}$ and $P_3(j|n) = P_3(B_j(0)) = \frac{1}{j}$.

By Lemma 31.6, $P_3(B_{ij}(0, 0)) = \frac{1}{ij}$.

Hence, if i, j are co-prime, then $P_3(i|n\ \&\ j|n) = P_3(i|n).P_3(j|n)$.

---

[2]Grinstead and Snell [**GS97**], Chapter 4, §4.1, Definition 4.2, p.141.

[3]Hardy and Wright [**HW60**], p.52, Theorem 59.

(b) The assumption that i, j be co-prime is necessary.

For instance, if $j = 2i$, then $i|n$ and $j|n$ if, and only if, $v = 0$.

Hence $P_3(i|n \;\&\; j|n) = P_3(B_j(0))$

By Corollary 31.4, $P_3(i|n) = P_3(B_i(0)) = \frac{1}{i}$ and $P_3(j|n) = P_3(B_j(0)) = \frac{1}{j}$.

Hence $P_3(i|n \;\&\; j|n) \neq P_3(i|n).P_3(j|n)$.

The lemma follows. □

(4) We thus conclude from Lemma 31.7 that:

COROLLARY 31.8. *If p and q are two unequal primes, $P_3(p|n \;\&\; q|n) = P_3(p|n).P_3(q|n)$.* □

THEOREM 31.9. *The prime divisors of an integer are mutually independent.* □

CHAPTER 32

Why *Integer Factorising* cannot be polynomial-time

32.1. The probability of *determining* that a *given* integer n is a prime

We consider the compound event where $B_i(0)$ does not occur for any of a set of $\pi(\sqrt{n})$ Bazeries wheels.

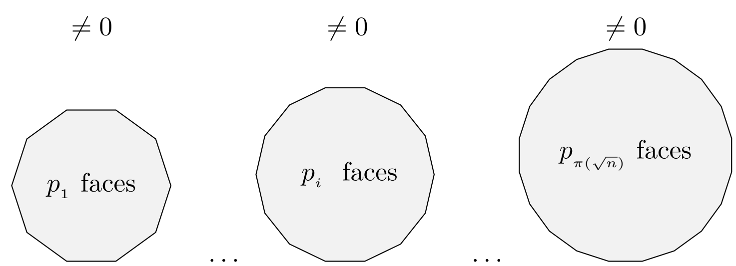

Fig. 4. The event where $B_i(0)$ does not occur for any of a set of $\pi(\sqrt{n})$ Bazeries wheels.

Now, even though we cannot define the probability $P_1(n\ is\ a\ prime)$ of *selecting* an integer n from the set N of all natural numbers that has the property of being prime[1], since we have by Corollary 31.5 that the probability $P_3(i \not| n)$ of *determining* by the spin of a Bazeries wheel that a prime $p < n$ does not divide a *given* n is $1 - \frac{1}{p}$, it follows from Theorem 31.9 that:

THEOREM 32.1. *The probability* $P_3(n\ is\ a\ prime)^2$ *of determining that a given integer* n *is prime is* $\prod_{i=1}^{\pi(\sqrt{n})}(1 - \frac{1}{p_i})$. □

PROOF. By Definition 31.1, Hypothesis 31.2, and Lemma 31.6, the probability that $B_i(0)$ does not occur for any i in a simultaneous spin of the $\pi(\sqrt{n})$ Bazeries wheels—where p_i is the i'th prime and B_i has p_i faces (Fig. 4)—is $\prod_{i=1}^{\pi(\sqrt{n})}(1 - \frac{1}{p_i})$.

If k is such that $k \not\equiv 0\ (mod\ p)$ for any prime $p \leq \sqrt{n}$, then the probability $P_3(k\ is\ co-prime\ to\ p \leq \sqrt{n})$ of *determining* by the simultaneous spin of the above $\pi(\sqrt{n})$ Bazeries wheels that k is not divisible by any prime $p \leq \sqrt{n}$ is $\prod_{i=1}^{\pi(\sqrt{n})}(1 - \frac{1}{p_i})$.

In the particular case where n is such that $n \not\equiv 0\ (mod\ p)$ for any prime $p \leq \sqrt{n}$, the probability $P_3(n\ is\ co-prime\ to\ p \leq \sqrt{n})$ of *determining* by the simultaneous spin of the above $\pi(\sqrt{n})$ Bazeries wheels that n is not divisible by any prime $p \leq \sqrt{n}$ is $\prod_{i=1}^{\pi(\sqrt{n})}(1 - \frac{1}{p_i})$.

[1]See §30.3 (2)(i).
[2]See §30.3 (2)(iii).

Since an integer n is a prime if, and only if, it is not divisible by any prime $p \leq \sqrt{n}$, the theorem follows. $\square$

32.2. Why determining primality is polynomial time

We now have that:

LEMMA 32.2. *The minimum number of events needed for determining that the signature of a given integer n—coded as the key of a Bazeries combination lock—is that of a prime is of order $O(log_e n)$.*

PROOF. By Theorem 32.1, the expected number of events which *determine* that a *given* n is prime in a set of k simultaneous spins of the $\pi(\sqrt{n})$ Bazeries wheels[3]—where p_i is the i'th prime and B_i has p_i faces (Fig.4)—is $k.\prod_{i=1}^{\pi(\sqrt{n})}(1-\frac{1}{p_i})$; which—by Mertens' Theorem[4] $\prod_{p\leq x}(1-\frac{1}{p}) \sim \frac{e^{-\lambda}}{log_e x}$—is ≥ 1 if $k \geq \frac{e^{\lambda}}{2}.log_e\ n$. The lemma follows by Definition 30.5 for minimum k. $\square$

We note the standard definition:

DEFINITION 32.3. A deterministic algorithm computes a number-theoretical function $f(n)$ in polynomial-time[5] if there exists k such that, for all inputs n, the algorithm computes $f(n)$ in $\leq (log_e\ n)^k + k$ steps.

By Definition 32.3, we further conclude that:

THEOREM 32.4. *Determining whether the signature of a given integer n—coded as the key in a modified Bazeries-cylinder (Definition 31.1) based combination lock—is that of a prime, or not, can be simulated by a deterministic algorithm in polynomial time $O(log_e n)$.*[6]

32.3. *Integer Factorising* cannot be polynomial-time

Given that n is composite, Theorem 31.9 and Theorem 32.1 now yield the computational complexity consequence that no deterministic algorithm can further compute a factor of n in polynomial time since:

COROLLARY 32.5. *Any deterministic algorithm that always computes a prime factor of n cannot be polynomial-time.* $\square$

---

[3]We note that this is *not* equivalent to the throws of a $\prod_{i=1}^{\pi(\sqrt{n})} p_{\pi(\sqrt{i})}$-sided die, each of whose faces is equally possible as a key to the code in question, since such throws do not use the fact—Theorem 31.9—that the prime divisors of n are mutually independent.

[4]Hardy and Wright [**HW60**], p. 351, Theorem 22.8; where $\lambda = 0.57722\ldots$ is the Euler-Mascheroni constant and $\frac{e^{\lambda}}{2} = 0.89053\ldots$.

[5]cf. Cook [**Cook**], p.1; also Brent [**Brn00**], p.1, fn.1: "For a polynomial-time algorithm the expected running time should be a polynomial in the length of the input, i.e. $O((logN)^c)$ for some constant c".

[6]We note that, in a seminal paper 'PRIMES is in P', Agrawal et al [**AKS04**] have shown that deciding whether the given *value* of an integer n is that of a prime or not can be done in polynomial time $\ddot{O}(log_e^{15/2} n)$; improved to $\ddot{O}(log_e^6 n)$ by Lenstra and Pomerance in [**LP11**].

Proof. By Theorem 32.1 and Mertens' Theorem, the expected number of primes $\leq \sqrt{n}$ is $O(\frac{\sqrt{n}}{log_e \sqrt{n}})$. Moreover, any computational process that successfully identifies a prime divisor of n must necessarily appeal to at least one logical operation for identifying such a factor.

Since n is a prime if, and only if, it is not divisible by any prime $p \leq \sqrt{n}$, it follows that, if $n = p^k$ for some prime p and $k > 1$, then determining p may require at least one logical operation for algorithmically testing each prime $\leq \sqrt{n}$ deterministically if, for some n, the prime p is the one that is tested last in the particular method of testing the primes $\leq \sqrt{n}$.

Since any algorithmically deterministic method of testing the primes $\leq \sqrt{n}$ must be independent of n, and always have some prime p that is tested last for any *given* n, the algorithm cannot always determine in polynomial time that p is a prime factor of n if $n = p^k$ for some $k > 1$.

In other words, since the number of primes to be tested if p is tested last, and $n = p^k$ for some $k > 1$, is of the order $O(\sqrt{n}/log_e\ n)$, the number of computations required by any deterministic algorithm that always computes a prime factor of n cannot be polynomial-time—i.e. of order $O((log_e\ n)^c)$ for any c—in the length of the input n.

The corollary follows. $\square$

Part 10

The significance of *evidence-based* reasoning for the Theory of Numbers

CHAPTER 33

The structure of divisibility and primality

> "Prime numbers are the most basic objects in mathematics. They also are among the most mysterious, for after centuries of study, the structure of the set of prime numbers is still not well understood. Describing the distribution of primes is at the heart of much mathematics ...".[1]

The significance of *evidence-based* reasoning (Chapter 5)—and of the differentiation between algorithmically *verifiable* and algorithmically *computable* number-theoretic functions (as detailed in Definitions 5.2 and 5.3)—for the Theory of Numbers is seen in the following identification of the natural number n with a corresponding set of residues $\{r_i(n)\}$, which shows how the usual, linearly displayed, Eratosthenes sieve argument reveals the structure of divisibility (and, ipso facto, of primality) more transparently when displayed as a 2-dimensional matrix representation of the residues $r_i(n)^2$, defined for all $n \geq 2$ and all $i \geq 2$ by:

$n + r_i(n) \equiv 0\ (mod\ i)$, where $i > r_i(n) \geq 0$.

Table 1: Eratosthenes sieve I

Sequence:	R_1	R_2	R_3	R_4	R_5	R_6	R_7	R_8	R_9	R_{10}	R_{11}	...	R_n
$n=1$	0	1	2	3	4	5	6	7	8	9	10	...	n-1
$n=2$	0	0	1	2	3	4	5	6	7	8	9	...	n-2
$n=3$	0	1	0	1	2	3	4	5	6	7	8	...	n-3
$n=4$	0	0	2	0	1	2	3	4	5	6	7	...	n-4
$n=5$	0	1	1	3	0	1	2	3	4	5	6	...	n-5
$n=6$	0	0	0	2	4	0	1	2	3	4	5	...	n-6
$n=7$	0	1	2	1	3	5	0	1	2	3	4	...	n-7
$n=8$	0	0	1	0	2	4	6	0	1	2	3	...	n-8
$n=9$	0	1	0	3	1	3	5	7	0	1	2	...	n-9
$n=10$	0	0	2	2	0	2	4	6	8	0	1	...	n-10
$n=11$	0	1	1	1	4	1	3	5	7	9	0	...	n-11
n	r_1	r_2	r_3	r_4	r_5	r_6	r_7	r_8	r_9	r_{10}	r_{11}	...	0

Density: For instance, the residues $r_i(n)$ can be defined for all $n \geq 1$ as the values of the non-terminating sequences $R_i(n) = \{i-1,\ i-2,\ \ldots,\ 0,\ i-1,\ i-2,\ \ldots,\ 0,\ \ldots\}$, defined for all $i \geq 1$ (as illustrated in Table 1[3]).

- For any given $i \geq 2$, each non-terminating sequence $R_i(n)$ can be viewed as generated by the incremental face-by-face movement of a Bazeries

[1]Andrew Granville: from this AMS press release of 5 December 1997.
[2]See §41, Appendix I(A), Fig.7 and II(B), Fig.8.
[3]For r_i read $r_i(n)$; for R_i read $R_i(n)$.

wheel (Definition 31.1), with i faces, which cycles through the values $(i-1,\ i-2,\ \ldots,\ 0)$ with period i;

- For any $i \geq 2$ the asymptotic density[4]—over the set of natural numbers—of the set $\{n\}$ of integers that are divisible by i is $\frac{1}{i}$; and the asymptotic density of integers that are not divisible by i is $\frac{i-1}{i}$.

Table 2: Eratosthenes sieve II

Sequence:	R_1	R_2	R_3	R_4	R_5	R_6	R_7	R_8	R_9	R_{10}	R_{11}	...	R_n
$E(1)$:	0	1	2	3	4	5	6	7	8	9	10	...	n-1
$E(2)$:	0	0	1	2	3	4	5	6	7	8	9	...	n-2
$E(3)$:	0	1	0	1	2	3	4	5	6	7	8	...	n-3
$E(4)$:	0	0	2	0	1	2	3	4	5	6	7	...	n-4
$E(5)$:	0	1	1	3	0	1	2	3	4	5	6	...	n-5
$E(6)$:	0	0	0	2	4	0	1	2	3	4	5	...	n-6
$E(7)$:	0	1	2	1	3	5	0	1	2	3	4	...	n-7
$E(8)$:	0	0	1	0	2	4	6	0	1	2	3	...	n-8
$E(9)$:	0	1	0	3	1	3	5	7	0	1	2	...	n-9
$E(10)$:	0	0	2	2	0	2	4	6	8	0	1	...	n-10
$E(11)$:	0	1	1	1	4	1	3	5	7	9	0	...	n-11
...													
$E(n)$:	r_1	r_2	r_3	r_4	r_5	r_6	r_7	r_8	r_9	r_{10}	r_{11}	...	0

Primality: The residues $r_i(n)$ can alternatively be defined for all $i \geq 1$ as values of the non-terminating sequences, $E(n) = \{r_i(n) : i \geq 1\}$, defined for all $n \geq 1$ (as illustrated in Table 2).

- The non-terminating sequences $E(n)$ highlighted in red correspond to a prime[5] p (since $r_i(p) \neq 0$ for $1 < i < p$) in the usual, linearly displayed, Eratosthenes sieve:

  $E(\cancel{1})$, $E(2)$, $E(3)$, $E(\cancel{4})$, $E(5)$, $E(\cancel{6})$, $E(7)$, $E(\cancel{8})$, $E(\cancel{9})$, $E(\cancel{10})$, $E(11)$, $\ldots$

- The non-terminating sequences highlighted in cyan identify a crossed out composite n (since $r_i(n) = 0$ for some $1 < i < n$) in the usual, linearly displayed, Eratosthenes sieve.

From an *evidence-based* perspective, it immediately follows from Theorem 32.1 that—as illustrated by the 2-dimensional representation of Eratosthenes sieve—the probability of determining that a number is prime is algorithmically *verifiable*, but not algorithmically *computable*, in the sense that:

LEMMA 33.1. *For any given n, there is a set of Bazeries wheels that can generate the sequence $E(n) = \{r_i(n) : n \geq i \geq 1\}$ and allow us to conclude that the probability $P_3(n$ is a prime$)$[6] of determining that a given integer n is prime is* $\prod_{i=1}^{\pi(\sqrt{n})}(1 - \frac{1}{p_i})$. □

[4]See §33.1(a); see also [**Ste02**], Chapter 2, p.10; [**El79a**], Notation, p.xxi; [**GS97**], Chapter 5, pp.183-186.

[5]Conventionally defined as integers that are not divisible by any smaller integer other than 1.

[6]See §30.3 (2)(iii).

LEMMA 33.2. *There is no set of Bazeries wheels that, for any given n, can generate the sequence $E(n) = \{r_i(n) : n \geq i \geq 1\}$ and allow us to conclude that the probability $P_3(n$ is a prime$)$ of determining that a given integer n is prime is $\prod_{i=1}^{\pi(\sqrt{n})}(1 - \frac{1}{p_i})$.* □

33.1. The residues $r_i(n)$ can be viewed in two different ways

The residues $r_i(n)$ can thus be viewed in two essentially different ways.

(a) *First* as the values, for any given i, of a function $R_i(n)$ over the domain N of the natural numbers.

Classically, since we cannot define a probability function for the probability that a random n is prime over the probability space $(1, 2, 3, \ldots,)$ ([**Ste02**], Chapter 2, p.9, Theorem 2.1), this definition does not admit an argument which will allow us to conclude that the prime divisors of any given integer n are independent.

(b) *Second* as the values, for any given n, of the sequence $E(n) = \{r_i(n) : i \geq 1\}$.

This now allows us to define a probability model from which we may conclude for any given $n > 1$, and any given prime $p > 1$, that the probability of the event $r_p(n) = 0$—whence p divides n—is $\frac{1}{p}$; and that the probability of the event $r_p(n) \neq 0$—whence p does not divide n—is $1 - \frac{1}{p}$.

This further allows us to argue (§36.2; see also Theorem 31.9) that, given $p, q > 1$ are two unequal primes, the compound probability that $r_p(n) = 0$ and $r_q(n) = 0$—whence both p and q divide n—is $\frac{1}{pq}$; and so the prime divisors of any given integer n are mutually independent.

CHAPTER 34

Heuristic approximations of prime counting functions

We next show how differentiating between algorithmic *verifiabilty* and algorithmic *computability* in Lemmas 33.1 and 33.2 admits *evidence-based* solutions to the query (where $\pi(n)$ denotes the number of primes $\leq n$):

Query 34.1. Can we estimate $\pi(n)$ *non-heuristically* for all finite values of n?

34.1. *Heuristically* estimated behaviour of the primes

To place the significance of Query 34.1 in an appropriate historical perspective, we note that Adrien-Marie Legendre and Carl Friedrich Gauss are reported[1] to have independently conjectured in 1796 that, if $\pi(x)$ denotes the number of primes less than or equal to x, then $\pi(x)$ is asymptotically equivalent to $\frac{x}{log_e x}$.

Fig.1: *Heuristic* behaviour of the primes

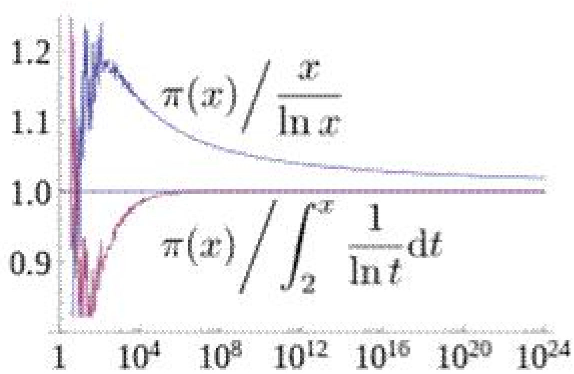

Fig.1: Graph showing ratio of the prime-counting function $\pi(x)$ to two of its approximations, $\frac{x}{ln\ x}$ and $Li(x)$. As x increases (note x axis is logarithmic), both ratios tend towards 1. The ratio for $\frac{x}{ln\ x}$ converges from above very slowly, while the ratio for $Li(x)$ converges more quickly from below.[2]

Around 1848/1850, Pafnuty Lvovich Chebyshev proved that $\pi(x) \asymp \frac{x}{log_e x}$, and confirmed that if $\pi(x)/\frac{x}{log_e x}$ has a limit, then it must be 1[3].

[1]cf. Prime Number Theorem. (2014, June 10). In Wikipedia, The Free Encyclopedia. Retrieved 09:53, July 9, 2014, from http://en.wikipedia.org/w/index.php?titleP̄rime_number_theorem&oldid=612391868; see also [**Gr95**].

[2]cf. Prime Number Theorem. (2014, June 10). In Wikipedia, The Free Encyclopedia. Retrieved 09:53, July 9, 2014, from http://en.wikipedia.org/w/index.php?titleP̄rime_number_theorem&oldid=612391868.

[3][**Dic52**], p.439; see also [**HW60**], p.9, Theorem 7 and p.345, §22.4 for a proof of Chebychev's Theorem.

The question of whether $\pi(x)/\frac{x}{log_e x}$ has a limit at all, or whether it oscillates, was purportedly answered—it has a limit—first by Jacques Hadamard and Charles Jean de la Vallée Poussin independently in 1896, using advanced argumentation involving functions of a complex variable[4]; and again independently by Paul Erdös and Atle Selberg[5] in 1949/1950, using only elementary—but still abstruse—methods without involving functions of a complex variable.

34.2. *Heuristic* approximations to $\pi(x)$

We also note that, reportedly[6]:

> "In a handwritten note on a reprint of his 1838 paper 'Sur l'usage des séries infinies dans la théorie des nombres', which he mailed to Carl Friedrich Gauss, Peter Gustav Lejeune Dirichlet conjectured (under a slightly different form appealing to a series rather than an integral) that an even better approximation to $\pi(x)$ is given by the offset logarithmic integral $Li(x)$ defined by:
> $Li(x) = \int_2^x \frac{1}{log_e t}.dt = li(x) - li(2)$."[7]

Fig.2: *Heuristic* distributions of the primes

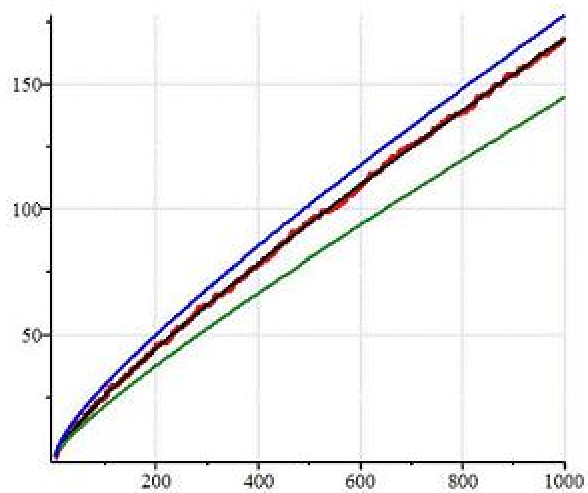

Fig.2: The above graph compares the actual number $\pi(x)$ (red) of primes $\leq x$ with the distribution of primes as estimated variously by the functions $Li(x)$ (blue), $R(x)$ (black), and $\frac{x}{log_e x}$ (green), where $R(x)$ is Riemann's function $\sum_{n=1}^{\infty} \frac{\mu(n)}{(n)} li(x^{1/n})$.[8]

We further note that in 1889 Jean de la Vallée Poussin proved[9] (cf. Fig.1):

[4][**Dic52**], p.439; see also [**Ti51**], Chapter III, p.8 for details of Hadamard's and de la Vallée Poussin's proofs of the Prime Number Theorem.

[5]See [**HW60**], p.360, Theorem 433 for a proof of Selberg's Theorem.

[6]cf. Prime Number Theorem. (2014, June 10). In Wikipedia, The Free Encyclopedia. Retrieved 09:53, July 9, 2014, from: http://en.wikipedia.org/w/index.php?titlePrime_number_theorem&oldid=612391868.

[7]Where $li(x) = \int_0^x \frac{1}{log_e t}.dt$.

[8]cf. How Many Primes Are There? In *The Prime Pages.* Retrieved 10:29, September 27, 2015, from:
https://primes.utm.edu/howmany.html.

[9][**Dic52**], p.440.

"...that $Li(x)$ represents $\pi(x)$ more exactly than $\frac{x}{log_e x}$ and its remaining approximations $\frac{x}{log_e x} + \frac{x}{log_e^2 x} + \ldots + \frac{(m-1)!x}{log_e^m x}$."

Moreover, all the known approximations of $\pi(n)$ for finite values of n are derived from real-valued functions that are asymptotic to $\pi(x)$, such as $\frac{x}{log_e x}$, $Li(x)$ and Riemann's function $R(x) = \sum_{n=1}^{\infty} \frac{\mu(n)}{(n)} li(x^{1/n})$.

Historically, however, the degree of approximation for finite values of n has been—and apparently continues to be—determined only *heuristically*, by conjecturing upon an error term in the asymptotic relation that can be seen to yield a closer approximation than others to the actual values of $\pi(n)$ (eg., Fig.2, where $n = 1000$).

For instance, the Riemann Hypothesis is that (compare [**Bomb**], p.4):

Riemann Hypothesis: For all $k > 2$, there is some constant $c_{_k} > 0$ such that:

$$|Li(x) - \pi(x)| \leq c_{_k}.\sqrt{(x)}.log_{_e}(x) \text{ for all } x > k.$$

where $Li(x)$ is the logarithmic integral and $\pi(x)$ is the prime counting function.

34.3. Is the constant in the Riemann Hypothesis algorithmically *verifiable* but not algorithmically *computable*?

The significance of *evidence-based* reasoning for Riemann's Hypothesis is that it admits the possibility that the constant in the hypothesis may be algorithmically *verifiable* (Definition 5.2), but not algorithmically *computable* (Definition 5.2), if:

- For any given integer $n > 2$, there is always a deterministic algorithm that will compute the digits in the decimal representation of a constant $c_{_n}$ such that:

  $$|Li(x) - \pi(x)| \leq c_{_n}.\sqrt{(x)}.log_{_e}(x) \text{ for all } x > n;$$

- There is no deterministic algorithm that, for any given integer $n > 2$, will compute the digits in the decimal representation of a constant $c_{_n}$ such that:

  $$|Li(x) - \pi(x)| \leq c_{_n}.\sqrt{(x)}.log_{_e}(x) \text{ for all } x > n.$$

34.4. Conventional wisdom

We note that the focus on *only heuristic* approximations of $\pi(n)$ for finite values of n apparently reflects conventional number theory wisdom, which appears to be that the distribution of primes is such that the probability $\mathbb{P}(n \in \{p\})$ of an integer n being a prime p can *only* be *heuristically* estimated as $\frac{1}{log_e n}$[10]—as suggested by the limiting value for $\pi(n)$ in the Prime Number Theorem, $\pi(n) \sim \frac{n}{log_e n}$[11]—and, further, that such probability is *not capable* of being estimated or well-defined statistically[12] independently of the Theorem.

[10]"The chance of a random integer x being prime is about $1/log\ x$" ... Chris K. Caldwell, *How Many Primes Are There?* In *The Prime Pages*. Retrieved 10:29, September 27, 2015, from: https://primes.utm.edu/howmany.html.

[11][**HW60**], Theorem 6, p.9.

[12]See, for instance, [**Ste02**], Chapter 2, p.9, Theorem (*sic*) 2.1!

Thus—whilst conceding[13] that the *heuristic* probability of an integer n being prime *could* also be naïvely assumed as $\prod_{i=1}^{\sqrt{n}}(1-\frac{1}{p_i})$—such a perspective seems to argue against undue reliance upon such naïvety, by concluding (*erroneously*, as we show in §37.1, Lemma 37.5) that the number $\pi(n)$ of primes less than or equal to n suggested by such probability would then be approximated *erroneously* by the prime counting function:

$$\pi_H(n) = \sum_{j=1}^{n}\prod_{i=1}^{\pi(\sqrt{n})}(1-\frac{1}{p_i}) = n.\prod_{i=1}^{\pi(\sqrt{n})}(1-\frac{1}{p_i}) \sim \frac{2.e^{-\gamma}n}{log_e n}.$$

For instance, Hardy and Littlewood argue curiously that:

> "In the first place we observe that any formula in the theory of primes, deduced from considerations of probability, is likely to be erroneous in just this way. Consider, for example, the problem 'what is the chance that a large number n should be prime?' We know that the answer is that the chance is approximately $\frac{1}{log\ n}$.
>
> Now the chance that n should not be divisible by any prime less than a *fixed* x is asymptotically equivalent to
>
> $$\prod_{\varpi<x}(1-\frac{1}{\varpi})$$
>
> and it would be natural to infer[1] that the chance required is asymptotically equivalent to
>
> $$\prod_{\varpi<\sqrt{x}}(1-\frac{1}{\varpi})$$
>
> But
>
> $$\prod_{\varpi<\sqrt{x}}(1-\frac{1}{\varpi}) \sim \frac{2e^{-C}}{log\ n}$$
>
> and our inference is incorrect, to the extent of a factor $2e^{-C}$.
>
> [1] One might well replace $\varpi < \sqrt{x}$ by $\varpi < x$, in which case we should obtain a probability half as large. This remark is in itself enough to show the unsatisfactory character of the argument."
>
> ... pp.36-37, G.H Hardy and J.E. Littlewood, *Some problems of 'partitio numerorum:' III: On the expression of a number as a sum of primes*, Acta Mathematica, December 1923, Volume 44, pp.1-70.

34.5. An *illusory* barrier

From an *evidence-based* perspective, however, such perspectives may need to admit the possibility that—as we show in the examples of 'discontinuous' Cauchy sequences (in §24.3) that represent physical processes which do not obey Cauchy convergence—functions involving *non-heuristic* estimates of the prime counting function $\pi(n)$ may also involve a distinct discontinuity as $n \to \infty$, as is suggested by Fig.4 in §35.1.

Moreover, such reasoning could raise an *illusory* barrier in seeking *non-heuristic* estimations of $\pi(n)$—and possibly of $|Li(x) - \pi(x)|$—if, as in the case of Lemma 33.2, the following theorem too is accepted as unsurpassable:

[13][**Gr95**], p.13.

THEOREM 34.2. *There is no algorithm which, for any given n, will allow us to conclude that the probability $P_3(n$ is a prime$)$ of determining that n is prime is $\prod_{i=1}^{\pi(\sqrt{n})}(1-\frac{1}{p_i})$.*

PROOF. The theorem follows immediately from Lemma 33.2 that there is no set of Bazeries wheels that, for any given n, can generate the sequence $E(n) = \{r_i(n) : n \geq i \geq 1\}$ and allow us to conclude that the probability $P_3(n$ *is a prime*$)$ of determining that a *given* integer n is prime is $\prod_{i=1}^{\pi(\sqrt{n})}(1-\frac{1}{p_i})$. □

Illusory, because it follows immediately from Theorem 32.1 that:

THEOREM 34.3. *For any given n, there is an algorithm which will allow us to conclude that the probability $P_3(n$ is a prime$)$ of determining that n is prime is $\prod_{i=1}^{\pi(\sqrt{n})}(1-\frac{1}{p_i})$.* □

34.6. *Non-heuristic* estimations of prime counting functions

The significance of Theorem 34.3 is that, by considering the asymptotic density of the set of all integers that are not divisible by the first k primes $p_1, p_2, \ldots, p_k$ we shall show that the expected number of such integers in any interval of length $(p^2_{\pi(\sqrt{n})+1} - p^2_{\pi(\sqrt{n})})$ is:

$\{(p^2_{\pi(\sqrt{n})+1} - p^2_{\pi(\sqrt{n})})\prod_{i=1}^{k}(1-\frac{1}{p_i})\}$.

This then allows us to define and estimate various prime counting functions *non-heuristically*, such as:

(a) For each n, the expected number of primes in the interval $(1,\ n)$ is (as illustrated in §35, Fig.1):

$\pi_H(n) = n\prod_{i=1}^{\pi(\sqrt{n})}(1-\frac{1}{p_i})$.

- The number $\pi(n)$ of primes $\leq n$ is thus approximated *non-heuristically* (Lemma 37.5 and Corollary 37.14) by:

  $\pi(n) \approx \pi_H(n) = n\prod_{i=1}^{\pi(\sqrt{n})}(1-\frac{1}{p_i}) \sim 2.e^{-\gamma}.\frac{n}{log_e n} \rightarrow \infty$.

(b) For each n, the expected number of primes in the interval $(p^2_{\pi(\sqrt{n})},\ p^2_{\pi(\sqrt{n})+1})$ is (as illustrated in §35, Fig.2):

$\pi_L(p^2_{\pi(\sqrt{n})+1}) - \pi_L(p^2_{\pi(\sqrt{n})}) = \{(p^2_{\pi(\sqrt{n})+1} - p^2_{\pi(\sqrt{n})})\prod_{i=1}^{\pi(\sqrt{n})}(1-\frac{1}{p_i})\}$.

- The number $\pi(n)$ of primes $\leq n$ is also thus approximated *non-heuristically* (Lemma 37.8 and Corollary 37.13) for $n \geq 4$ by the cumulative sum:

  $\pi(n) \approx \pi_L(n) = \sum_{j=1}^{n}\prod_{i=1}^{\pi(\sqrt{j})}(1-\frac{1}{p_i}) \sim a.\frac{n}{log_e n} \rightarrow \infty$ for some constant $a > 2.e^{-\gamma}$.

(c) For each n, the expected number of Dirichlet primes—of the form $a + m.d$ for some natural number $m \geq 1$—in the interval $(p^2_{\pi(\sqrt{n})},\ p^2_{\pi(\sqrt{n})+1})$ is:

$\{(p^2_{\pi(\sqrt{n})+1} - p^2_{\pi(\sqrt{n})})\prod_{i=1}^{k}\frac{1}{q_i^{\alpha_i}}.\prod_{i=1}^{k}(1-\frac{1}{q_i})^{-1}.\prod_{j=1}^{\pi(\sqrt{n})}(1-\frac{1}{p_j})\}$

where $1 \leq a < d = q_1^{\alpha_1}.q_2^{\alpha_2}\ldots q_k^{\alpha_k}$ and $(a,d) = 1$.

– The number $\pi_{(a,d)}(n)$ of Dirichlet primes $\leq n$ is thus approximated *non-heuristically* (Lemma 38.10) for all $n \geq q_k^2$ by the cumulative sum:

$\pi_{(a,d)}(n) \approx \prod_{i=1}^{k} \frac{1}{q_i^{\alpha_i}} \cdot \prod_{i=1}^{k}(1 - \frac{1}{q_i})^{-1} \cdot \sum_{l=1}^{n} \prod_{j=1}^{\pi(\sqrt{l})}(1 - \frac{1}{p_j}) \rightarrow \infty.$

(d) For each n, the expected number of $\mathbb{TW}$ primes—such that n is a prime and $n+2$ is either a prime or $p^2_{\pi(\sqrt{n})+1}$—in the interval $(p^2_{\pi(\sqrt{n})},\ p^2_{\pi(\sqrt{n})+1})$ is:

$\{(p^2_{\pi(\sqrt{n})+1} - p^2_{\pi(\sqrt{n})}) \prod_{i=2}^{\pi(\sqrt{n})}(1 - \frac{2}{p_i})\}.$

– The number $\pi_2(p^2_{k+1})$ of twin primes $\leq p^2_{k+1}$ is thus approximated *non-heuristically* (Lemma 39.8) for all $k \geq 1$ by the cumulative sum:

$\pi_2(p^2_{k+1}) \approx \sum_{j=9}^{p^2_{k+1}} \prod_{i=2}^{\pi(\sqrt{j})-1}(1 - \frac{2}{p_i}) \rightarrow \infty.$

CHAPTER 35

Non-heuristic approximations of $\pi(n)$ for ***all*** values of n

Now, it follows from Theorem 34.3 that the asymptotic density[1] of integers co-prime to the first k primes, $p_1, p_2, \ldots, p_k$, over the set of natural numbers, is:

$\prod_{i=1}^{k}(1 - \frac{1}{p_i})$;

and that the expected number of such integers in the interval (a, b) is thus:

$(b - a)\prod_{i=1}^{k}(1 - \frac{1}{p_i})$,

where the binomial standard deviation of the expected number of integers co-prime to $p_1, p_2, \ldots, p_k$ in *any* interval of length $(b - a)$ is:

$\sqrt{(b - a)\prod_{i=1}^{k}(1 - \frac{1}{p_i})(1 - \prod_{i=1}^{k}(1 - \frac{1}{p_i}))}$.

Fig.1: Graph of $y = \prod_{i=1}^{\pi(\sqrt{x})}(1 - \frac{1}{p_i})$ for estimating $\pi_H(n)$

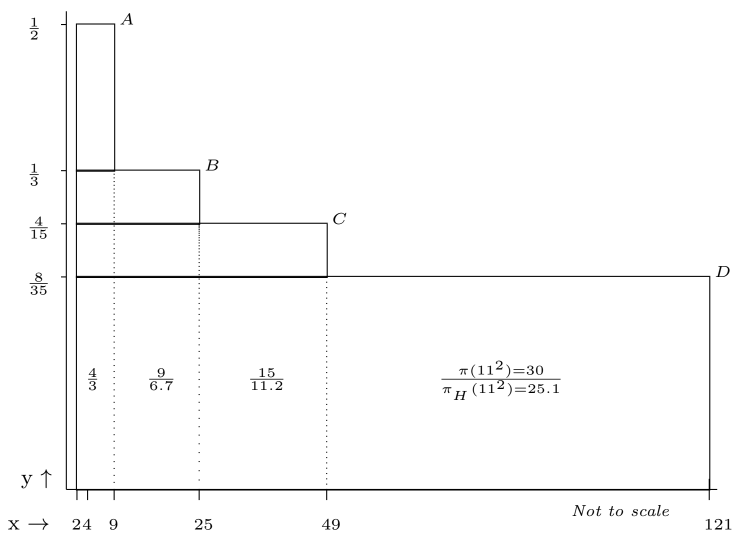

Fig.1: Graph of $y = \prod_{i=1}^{\pi(\sqrt{x})}(1 - \frac{1}{p_i})$ for estimating $\pi_H(n)$. The overlapping rectangles $A, B, C, D, \ldots$ in fig. $\pi_H(n)$ represent $\pi_H(p_{j+1}^2) = p_{j+1}^2 . \prod_{i=1}^{j}(1 - \frac{1}{p_i})$ for $j \geq 1$. Figures within each rectangle are the primes and estimated primes corresponding to the functions $\pi(n)$ and $\pi_H(n)$, respectively, within the interval $(1,\ p_{j+1}^2)$ for $j \geq 2$.

[1] cf. [**Ste02**], Chapter 2, p.10.

Taking (a, b) as the intervals $(p_1^2, p_2^2), (p_2^2, p_3^2), \ldots, (p^2_{\pi(\sqrt{n})},\ p^2_{\pi(\sqrt{n})+1})$, we note that (as illustrated in Fig.1):

(i) For any given n:

$\pi_H(p^2_{\pi(\sqrt{n})+1}) = p^2_{\pi(\sqrt{n})+1} \prod_{i=1}^{\pi(\sqrt{n})}(1-\frac{1}{p_i})$ is (contrary to conventional wisdom in §34.4) a *non-heuristic* estimate of $\pi(p^2_{\pi(\sqrt{n})+1})$, with standard deviation:

$p_{\pi(\sqrt{n})+1}\sqrt{\prod_{i=1}^{\pi(\sqrt{n})}(1-\frac{1}{p_i})(1-\prod_{i=1}^{\pi(\sqrt{n})}(1-\frac{1}{p_i}))}$.

Fig.2: Graph of $y = \prod_{i=1}^{\pi(\sqrt{x})}(1-\frac{1}{p_i})$ for estimating $\pi_L(n)$

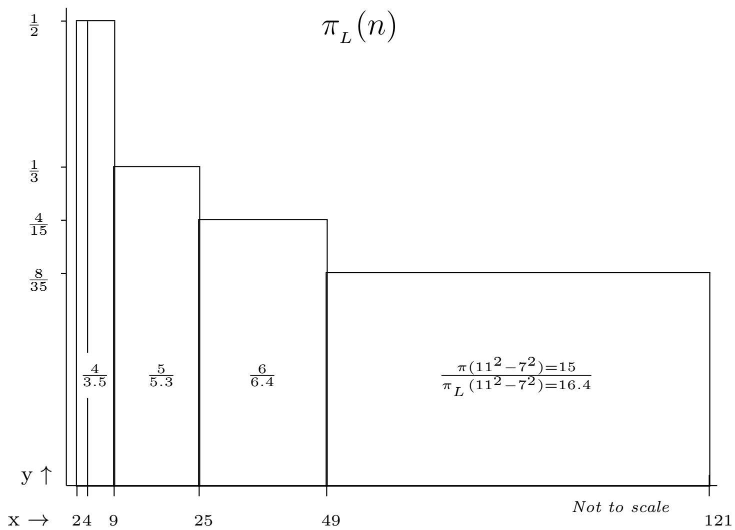

Fig.2: Graph of $y = \prod_{i=1}^{\pi(\sqrt{x})}(1-\frac{1}{p_i})$. The rectangles represent $(p_{j+1}^2 - p_j^2)\prod_{i=1}^{j}(1-\frac{1}{p_i})$ for $j \geq 1$. Figures within each rectangle are the primes corresponding to the functions $\pi(n)$ and $\pi_L(n)$ within the interval $(p_j^2,\ p_{j+1}^2)$ for $j \geq 2$. The area under the curve is $\pi_L(x) = (x - p_n^2)\prod_{i=1}^{n}(1-\frac{1}{p_i}) + \sum_{j=1}^{n-1}(p_{j+1}^2 - p_j^2)\prod_{i=1}^{j}(1-\frac{1}{p_i}) + 2$.

Moreover (as illustrated in Fig.2):

(ii) For any given n:

$\pi_L(p^2_{\pi(\sqrt{n})+1}) - \pi_L(p^2_{\pi(\sqrt{n})})$ is also a *non-heuristic* estimate of the number of primes in the interval $(p^2_{\pi(\sqrt{n})},\ p^2_{\pi(\sqrt{n})+1})$.

It follows that:

$\pi_L(p^2_{\pi(\sqrt{n})+1}) = \sum_{j=1}^{\pi(\sqrt{n})}\{(p_{j+1}^2 - p_j^2)\prod_{i=1}^{j}(1-\frac{1}{p_i})\}$ is *cumulatively* a *non-heuristic* estimate of $\pi(p^2_{\pi(\sqrt{n})+1})$, with *cumulative* standard deviation:

$\sum_{j=1}^{\pi(\sqrt{n})}\sqrt{(p_{j+1}^2 - p_j^2)\prod_{i=1}^{j}(1-\frac{1}{p_i})(1-\prod_{i=1}^{j}(1-\frac{1}{p_i}))}$.

More generally (as illustrated in Fig.3):

(iii) The *non-heuristic* approximations of the number $\pi(n)$ of primes less than or equal to n are given by the prime counting functions $\pi_H(n)$ (Lemma 37.5) and $\pi_L(n)$ (Lemma 37.8)[2]:

– $\pi(n) \approx \pi_H(n) = \sum_{j=1}^{n} \prod_{i=1}^{\pi(\sqrt{n})}(1 - \frac{1}{p_i}) = n.\prod_{i=1}^{\pi(\sqrt{n})}(1 - \frac{1}{p_i}) \sim 2e^{-\lambda}\frac{n}{log_e n}$.

– $\pi(n) \approx \pi_L(n) = \sum_{j=1}^{n} \prod_{i=1}^{\pi(\sqrt{j})}(1 - \frac{1}{p_i}) \sim a.\frac{n}{log_e n} \to \infty$, $a > 2.e^{-\gamma} \approx 1.12292\ldots$;

Fig.3: The graphs of $y = \pi_H(x)$ and $y = \pi_L(x)$

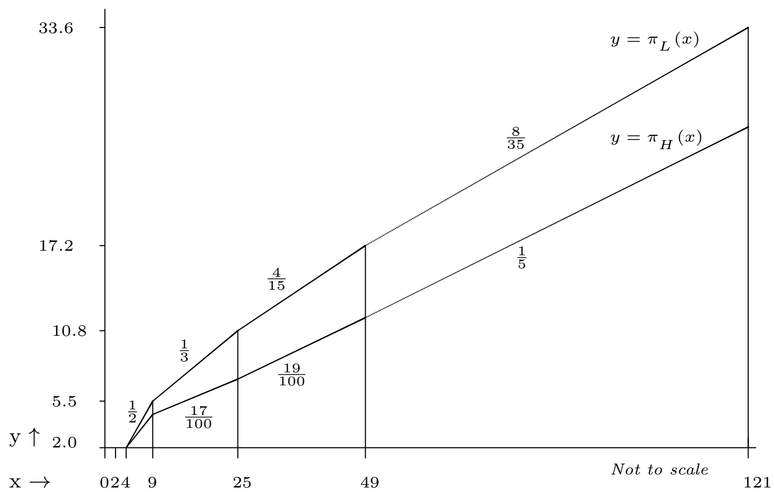

Fig.3: Graph of: (i) $y = \pi_H(x) = x.\prod_{i=1}^{\pi(\sqrt{x})}(1 - \frac{1}{p_i})$[3]; and of: (ii) $y = \pi_L(x) = (x - p_n^2)\prod_{i=1}^{n}(1 - \frac{1}{p_i}) + \sum_{j=1}^{n-1}(p_{j+1}^2 - p_j^2)\prod_{i=1}^{j}(1 - \frac{1}{p_i}) + 2$ in the interval $(p_n^2,\ p_{n+1}^2)$. Note that the gradient of $y = \pi_L(x)$ in the interval $(p_n^2,\ p_{n+1}^2)$ is $\prod_{i=1}^{n}(1 - \frac{1}{p_i}) \to 0$.

35.1. How good are the *non-heuristic* estimates of $\pi(n)$?

Based on the manual and spreadsheet calculations detailed in Chapter 42, we compare the *non-heuristically* estimated values of $\pi(n)$:

(i) $\pi_H(n) = \sum_{j=1}^{n} \prod_{i=1}^{\pi(\sqrt{n})}(1 - \frac{1}{p_i}) = n.\prod_{i=1}^{\pi(\sqrt{n})}(1 - \frac{1}{p_i})$ (green); and

(ii) $\pi_L(n) = \sum_{j=1}^{n} \prod_{i=1}^{\pi(\sqrt{j})}(1 - \frac{1}{p_i})$ (red);

with the actual values of $\pi(n)$ (blue) for $4 \leq n \leq 3000$ in Fig.4 (compare with Fig.2 in §34.2).

Now, we note that:

[2]Compare [**HL23**], pp.36-37.

[3]See §37.1

(a) $\pi(n) \sim \frac{n}{log_e(n)}$ by the Prime Number Theorem;

(b) $\pi_H(n) \sim 2e^{-\lambda}\frac{n}{log_e n}$ where $2.e^{-\gamma} \approx 1.12292\ldots$ by Corollary 37.14;

(c) $\pi_L(n) > \pi_H(n)$ for all $n \geq 9$ by Corollary 37.9;

(d) $\pi_L(n) > \pi(n) > \pi_H(n)$ for $n \leq 3000$ by observation (Fig.4).

Fig.4: *Non-heuristically* estimated distributions of the primes ≤ 3000

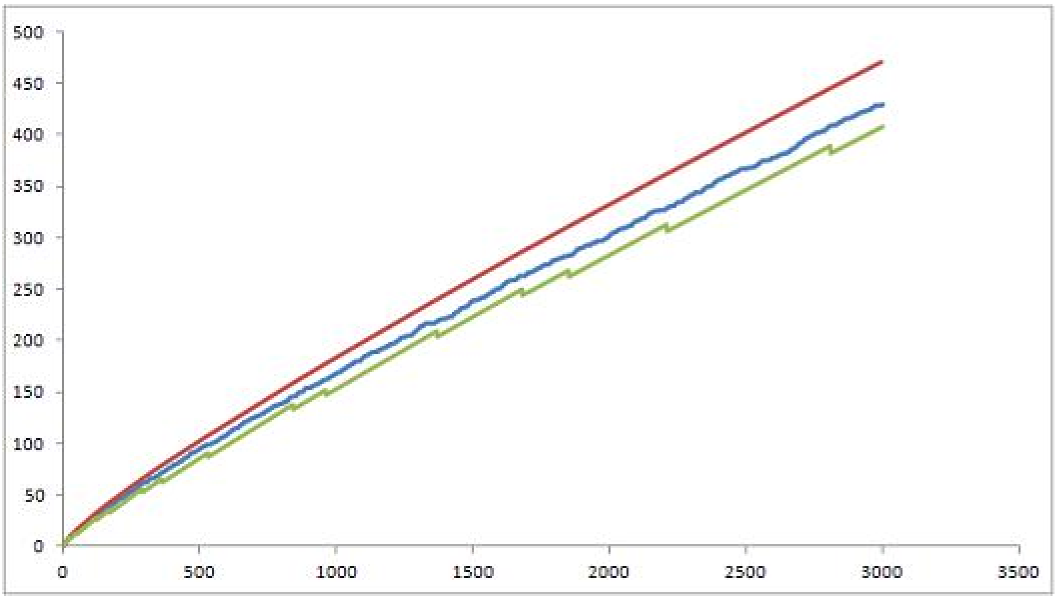

Fig.4: The above graph compares the *non-heuristically* estimated values of $\pi(n)$: $\pi_H(n) = \sum_{j=1}^{n}\prod_{i=1}^{\pi(\sqrt{n})}(1 - \frac{1}{p_i}) = n.\prod_{i=1}^{\pi(\sqrt{n})}(1 - \frac{1}{p_i})$ (green) and $\pi_L(n) = \sum_{j=1}^{n}\prod_{i=1}^{\pi(\sqrt{j})}(1 - \frac{1}{p_i})$ (red), with the actual values of $\pi(n)$ (blue) for $4 \leq n \leq 3000$.

This raises the interesting queries:

Query 35.1. Which is the least n such that $\pi_H(n) > \pi(n)$?

Query 35.2. Which is the largest n such that $\pi(n) > \pi_H(n)$?

Query 35.3. Is $\pi(n)$ an arithmetical function which tends to a discontinuity as $n \rightarrow \infty$?[4]

35.2. Three intriguing observations

The following computations[5] compare the actual values of $\pi(n)$, $n.\prod_{j=i}^{\pi(\sqrt{n})}(1 - \frac{1}{p_j})$, and $\frac{n}{log_e\ n}$ in the range $4 \leq n \leq 100$ (Fig.5), $4 \leq n \leq 1000$ (Fig.6), $4 \leq n \leq 10000$ (Fig.7), $4 \leq n \leq 100000$ (Fig.8), $4 \leq n \leq 1000000$ (Fig.9).

[4]As in the case of a virus cluster, or that of an elastic string, considered in §24.3.

[5]Courtesy *Mathematica*.

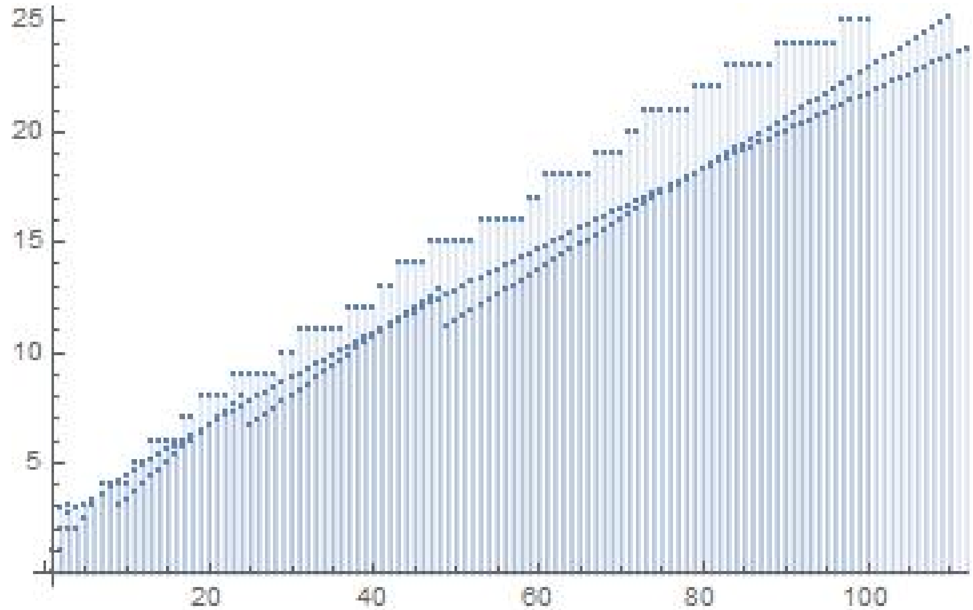

Fig.5: The above graph compares the actual values of $\pi(n)$ in the range $4 \leq n \leq 100$, with $n.\prod_{j=i}^{\pi(\sqrt{n})}(1 - \frac{1}{p_j})$ in the range $4 \leq n \leq 110$, and $\frac{n}{log_e\ n}$ in the range $4 \leq n \leq 120$.

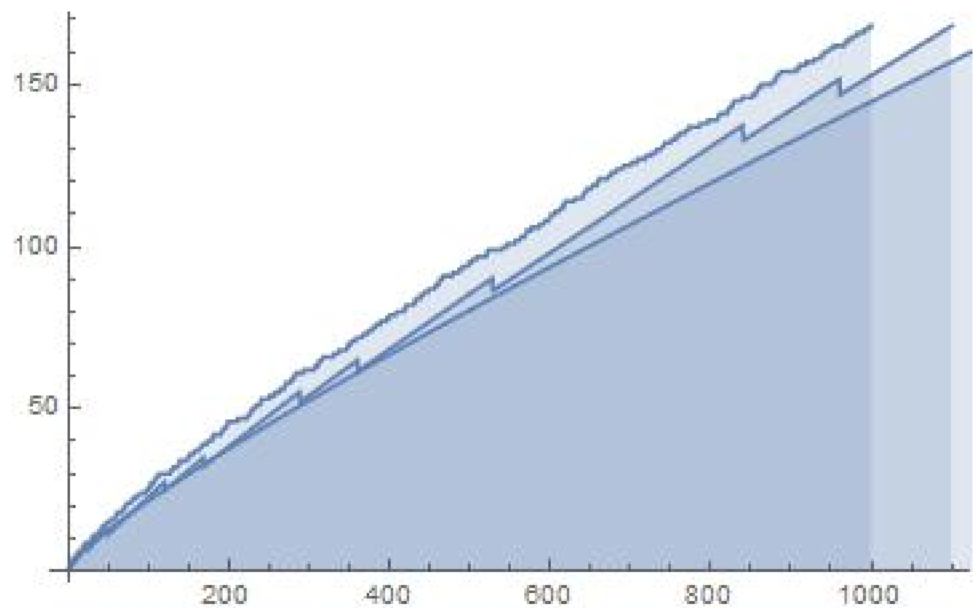

Fig.6: The The above graph compares the actual values of $\pi(n)$ in the range $4 \leq n \leq 1000$, with $n.\prod_{j=i}^{\pi(\sqrt{n})}(1 - \frac{1}{p_j})$ in the range $4 \leq n \leq 1100$, and $\frac{n}{log_e\ n}$ in the range $4 \leq n \leq 1200$.

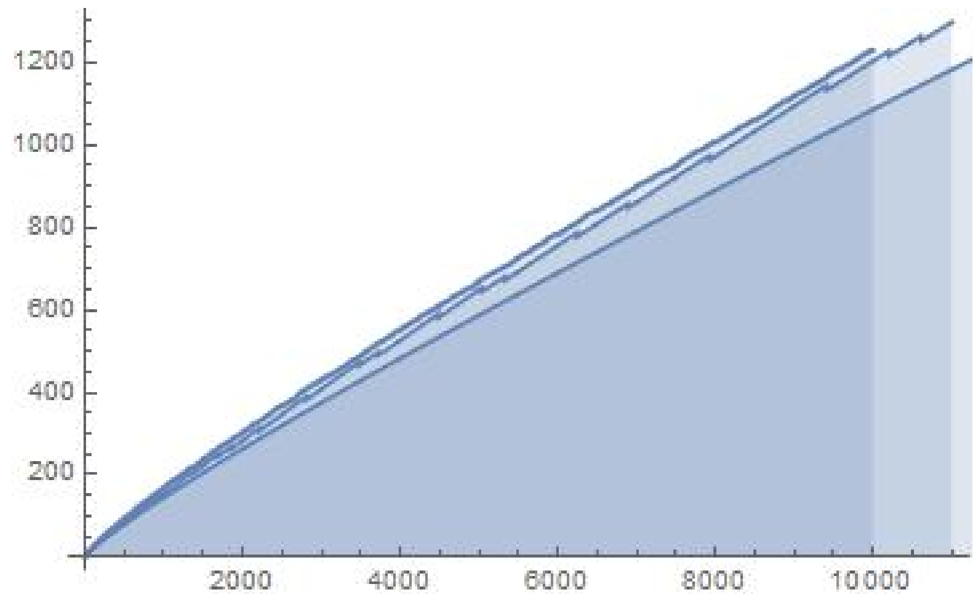

Fig.7: The above graph compares the actual values of $\pi(n)$ in the range $4 \leq n \leq 10000$, with $n.\prod_{j=i}^{\pi(\sqrt{n})}(1-\frac{1}{p_j})$ in the range $4 \leq n \leq 11000$, and $\frac{n}{log_e\ n}$ in the range $4 \leq n \leq 12000$.

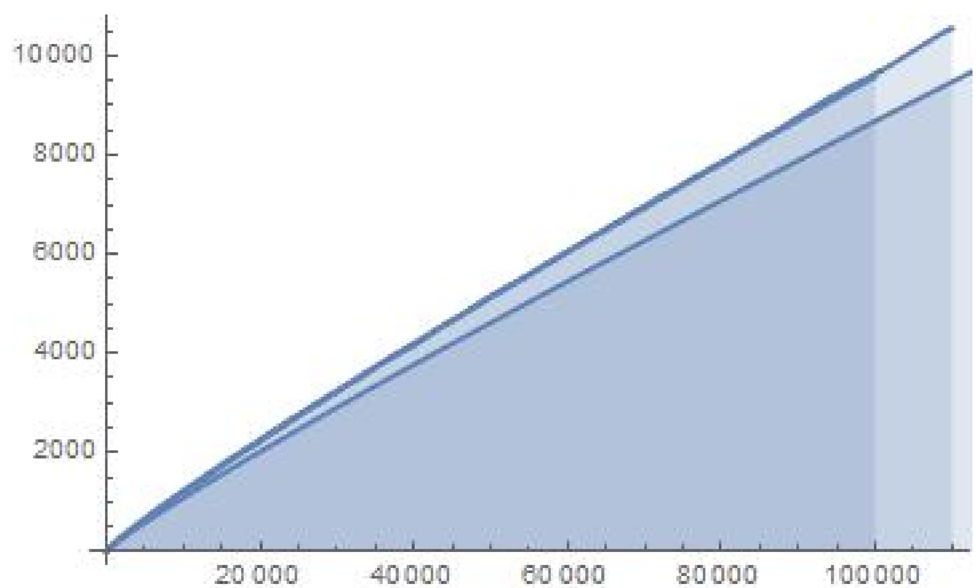

Fig.8: The above graph compares the actual values of $\pi(n)$ in the range $4 \leq n \leq 100000$, with $n.\prod_{j=i}^{\pi(\sqrt{n})}(1-\frac{1}{p_j})$ in the range $4 \leq n \leq 110000$, and $\frac{n}{log_e\ n}$ in the range $4 \leq n \leq 120000$.

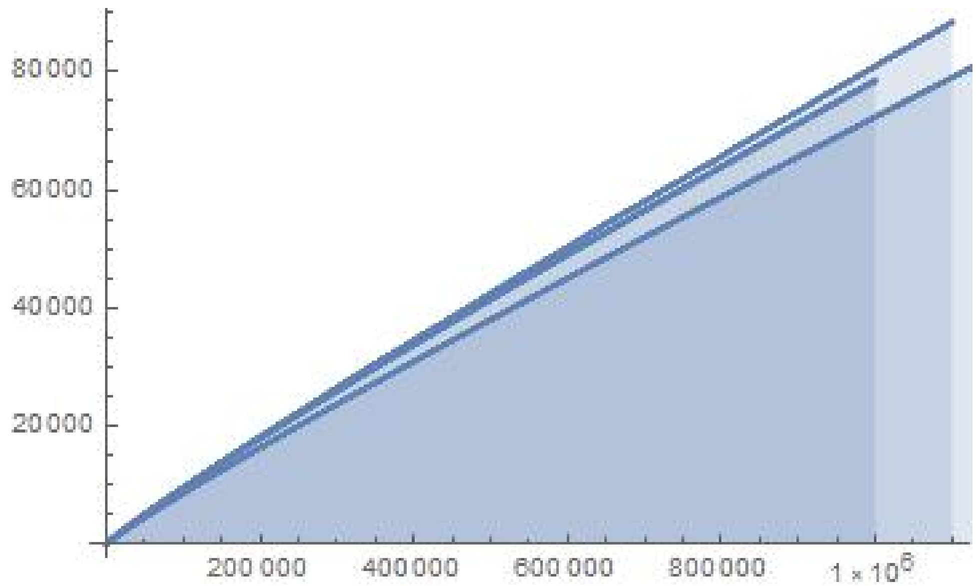

Fig.9: The above graph compares the actual values of $\pi(n)$ in the range $4 \leq n \leq 1000000$, with $n.\prod_{j=i}^{\pi(\sqrt{n})}(1 - \frac{1}{p_j})$ in the range $4 \leq n \leq 1100000$, and $\frac{n}{log_e\ n}$ in the range $4 \leq n \leq 1200000$.

We note that Query 35.1 is answered by Figs.8-9, which show that the least n such that $\pi_H(n) \geq \pi(n)$ occurs somewhere around $n = 100000$.

As regards Query 35.2 however, we conjecture Fig.9 suggests that if k is the least n such that $\pi_H(n) \geq \pi(n)$, then $k - 1$ is the largest n such that $\pi(n) > \pi_H(n)$.

Finally, we conjecture that—despite what is suggested by the Prime Number Theorem—Query 35.3 can be answered affirmatively if the three functions $\pi(n)$, $n.\prod_{j=i}^{\pi(\sqrt{n})}(1 - \frac{1}{p_j})$, and $\frac{n}{log_e\ n}$ are as well-behaved as Fig.9 suggests[6]!

35.3. Conventional estimates of $\pi(x)$ for finite $x > 2$ are heuristic

The above observations reflect the circumstance that all conventional estimates of $\pi(x)$ for finite $x > 2$ are heuristic

Moreover, we note Guy Robin ([**Rob83**]) proved that the following changes sign infinitely often:

$(log_e\ n).\prod_{p\leq n}(1 - \frac{1}{p}) - e^{-\gamma}$

Robin's result is analogous to Littlewood's curious theorem[7] that the difference $\pi(x) - Li(x)$ changes sign infinitely often. No analogue of the Skewes number (an

[6]See also computations for $4 \leq n \leq 5000000$ and $4 \leq n \leq 6000000$ here.

[7]Since there is no English translation of Littlewood's 1914 paper, which was presented in French on his behalf by Hadamard at a conference, the author has had to rely upon his own translation of Littlewood's theorem based on both his limited knowledge of French, and his limited knowledge of the substance of Littlewood's paper. Hopefully, the following remarks will not reflect seriously upon his ignorance of either!

upper bound on the first natural number x for which $\pi(x) > Li(x)$) is, however, known for Robin's result.

Littlewood's theorem is 'curious' since:

(a) There is no explicitly defined arithmetical formula that, for any $x > 2$, will yield $\pi(x)$. Hence, Littlewood's proof deduces the behaviour of $\pi(x) - Li(x)$ for *finite* values of $x > 2$ by implicitly appealing to the relation of $\pi(x)$ to $\zeta(s)$, defined as $\sum \frac{1}{n^s}$ over all integers $n \geq 1$, through the identity of the infinite summation with the Euler product $\prod(1 - \frac{1}{p^s})^{-1}$ over all primes, which is valid *only* for $\mathrm{Re}(s) > 1$; as is its consequence (which involves the re-arrangement of an infinite summation that, too, is valid only for $\mathrm{Re}(s) > 1$):

$$log_e\ \zeta(s) = s.\int_2^\infty \frac{\pi(x)}{x(x^s-1)}dx = s.\int_2^\infty \frac{\pi(x)/(1-\frac{1}{x^s})}{x^{s+1}}dx$$

(b) Littlewood's proof deduces the behaviour of $\pi(x) - Li(x)$ for *finite* values of $x > 0$ by[8] appealing to the analytically continued behaviour of $\zeta(s)$ in areas where $\pi(x)$ is not defined!

Moreover, we note that—unlike $\pi_H(n)$ and $\pi_L(n)$—conventional estimates of $\pi(x)$ for *finite* values of $x > 0$ can be treated as *heuristic*, since they appeal only to the *limiting* behaviour ([**HW60**], Theorem 420, p.345) of a formally (i.e., explicitly) *undefined* arithmetical function, $\pi(n)$, as based upon the limiting behaviours of formally *defined* arithmetical functions $\phi(n)$ and $\psi(n)$:

$$\pi(x) \sim \frac{\phi(x)}{log_e\ x} \sim \frac{\psi(x)}{log_e\ x}$$

where:

$$\phi(x) = \sum_{p \leq x} log_e\ p = log_e \prod_{p \leq x} p$$

$$\psi(x) = \sum_{p^m \leq x} log_e\ p$$

and the latter is curiously stipulated as valid only for $x > 1$, but definable in terms of a summation over the zeros of the zeta function in the critical strip $0 < \mathrm{Re}(\rho) < 1$:

> "$\psi(x)$ is given by the so-called explicit formula
>
> $$\psi(x) = x - \sum_\rho \frac{x^\rho}{\rho} - log_e\ (2.\pi) - \frac{1}{2}log_e\ (1 - x^2)$$
>
> for $x > 1$ and x not a prime or prime power, and the sum is over all nontrivial zeros ρ of the Riemann zeta function $\zeta(s)$, i.e., those in the critical strip so $0 < R(\rho) < 1$, and interprets as
>
> $$lim_{t\to\infty} \sum_{|I(\rho)|<t} \frac{x^\rho}{\rho}."$$
>
> ... http://mathworld.wolfram.com/MangoldtFunction.html

[8]Which needs further justification from an *evidence*-based perspective, as I argue in my blog-page: https://foundationalperspectives.wordpress.com/2018/09/26/michael-atiyah-on-the-riemann-hypothesis-and-the-fine-structure-constant/.

CHAPTER 36

The residues $r_i(n)$.

We begin formal proofs of the foregoing considerations by defining the residues $r_i(n)$ for all $n \geq 2$ and all $i \geq 2$ as below[1]:

DEFINITION 36.1. $n + r_i(n) \equiv 0$ $(mod\ i)$ where $i > r_i(n) \geq 0$.

Since each residue $r_i(n)$ cycles over the i values $(i-1, i-2, \ldots, 0)$, these values are all incongruent and form a complete system of residues[2] $mod\ i$.

It immediately follows that:

LEMMA 36.2. *$r_i(n) = 0$ if, and only if, i is a divisor of n.* □

36.1. The probability model $\mathbb{M}_i = \{(0, 1, 2, \ldots, i-1),\ r_i(n), \frac{1}{i}\}$

By the standard definition of the probability $\mathbb{P}(e)$ of an event e[3], we have by Lemma 36.2 that:

LEMMA 36.3. *For any $n \geq 2$, $i \geq 2$ and any given integer $i > u \geq 0$:*

- *the probability $\mathbb{P}(r_i(n) = u)$ that $r_i(n) = u$ is $\frac{1}{i}$;*
- $\sum_{u=0}^{u=i-1} \mathbb{P}(r_i(n) = u) = 1$;
- *and the probability $\mathbb{P}(r_i(n) \neq u)$ that $r_i(n) \neq u$ is $1 - \frac{1}{i}$.* □

By the standard definition of a probability model, we conclude that:

THEOREM 36.4. *For any $i \geq 2$, $\mathbb{M}_i = \{(0, 1, 2, \ldots, i-1),\ r_i(n), \frac{1}{i}\}$ yields a probability model for each of the values of $r_i(n)$.* □

COROLLARY 36.5. *For any given n, i and u such that $r_i(n) = u$, the probability that the roll of an i-sided cylindrical die will yield the value u is $\frac{1}{i}$ by the probability model defined in Theorem 36.4 over the probability space $(0, 1, 2, \ldots, i-1)$.* □

COROLLARY 36.6. *For any $n \geq 2$ and any prime $p \geq 2$, the probability $\mathbb{P}(r_p(n) = 0)$ that $r_p(n) = 0$, and that p divides n, is $\frac{1}{p}$; and the probability $\mathbb{P}(r_p(n) \neq 0)$ that $r_p(n) \neq 0$, and that p does not divide n, is $1 - \frac{1}{p}$.* □

We also note the standard definition[4]:

[1]The residues $r_i(n)$ can also be graphically displayed variously as shown in the Appendix I in §41.

[2][**HW60**], p.49.

[3]See [**Kol56**], Chapter I, §1, Axiom III, pg.2.

[4]See [**Kol56**], Chapter VI, §1, Definition 1, pg.57 and §2, pg.58.

DEFINITION 36.7. Two events e_i and e_j are mutually independent for $i \neq j$ if, and only if, $\mathbb{P}(e_i \cap e_j) = \mathbb{P}(e_i).\mathbb{P}(e_j)$.

36.2. The prime divisors of any integer n are mutually independent

We begin by formally noting first that:

LEMMA 36.8. *If $n \geq 2$ and $n > i, j > 1$, where $i \neq j$, then:*

$$\mathbb{P}((r_i(n) = u) \cap (r_j(n) = v)) = \mathbb{P}(r_i(n) = u).\mathbb{P}(r_j(n) = v)$$

where $i > u \geq 0$ and $j > v \geq 0$.

PROOF. We note that:

(i) If $n \geq 2$ and $n > i, j > 1$, where $i \neq j$, then we can always determine a unique pair of residues $r_i(n) = u$ and $r_j(n) = v$, where $i > u \geq 0$, $j > v \geq 0$, i divides $n + u$, and j divides $n + v$.

(ii) There are $i.j$ pairs (u, v) such that $i > u \geq 0$ and $j > v \geq 0$.

(iii) The compound probability that the simultaneous roll of one i-sided cylindrical die and one j-sided cylindrical die will yield the values u and v, respectively, is thus $\frac{1}{i.j}$ by the probability model for such a simultaneous event as defined over the probability space $\{(u, v) : i > u \geq 0, j > v \geq 0\}$, where we note that:
- the probability $\mathbb{P}((r_i(n) = u) \cap (r_j(n) = v))$ that $r_i(n) = u$ and $r_j(n) = v$ is $\frac{1}{i.j}$;
- $\sum_{All\ (u,v):\ i>u\geq 0,\ j>v\geq 0} \mathbb{P}((r_i(n) = u) \cap (r_j(n) = v)) = 1$;

(iv) By Lemma 36.3, the product of the probability $\frac{1}{i}$ that the roll of an i-sided cylindrical die will yield the value u, and the probability $\frac{1}{j}$ that the roll of a j-sided cylindrical die will yield the value v, is $\frac{1}{i.j}$.[5]

(v) It follows that:

$$\mathbb{P}((r_i(n) = u) \cap (r_j(n) = v)) = \frac{1}{i.j}$$

$$\mathbb{P}(r_i(n) = u).\mathbb{P}(r_j(n) = v) = (\tfrac{1}{i})(\tfrac{1}{j}).$$

The lemma follows. □

COROLLARY 36.9. $\mathbb{P}((r_i(n) = 0) \cap (r_j(n) = 0)) = \mathbb{P}(r_i(n) = 0).\mathbb{P}(r_j(n) = 0)$. □

Since, by Lemma 36.2, $r_i(n) = 0$ if, and only if, i is a divisor of n, it follows from Corollary 36.9 that:

THEOREM 36.10. *If i and j are co-prime and $i \neq j$, then whether, or not, i divides any given natural number n is independent of whether, or not, j divides n.* □

[5] In other words, the compound probability of determining u and v correctly from the simultaneous roll of one i-sided cylindrical die and one j-sided cylindrical die, is the product of the probability of determining u correctly from the roll of an i-sided cylindrical die, and the probability of determining v correctly from the roll of a j-sided cylindrical die.

PROOF. We note that

(i) By Corollary 36.8, we have that:

$$\mathbb{P}((r_i(n) = 0) \cap (r_j(n) = 0)) = \frac{1}{i.j}$$

$$\mathbb{P}(r_i(n) = 0).\mathbb{P}(r_j(n) = 0) = (\tfrac{1}{i})(\tfrac{1}{j}).$$

(ii) Further, if i and j are co-prime, and $n + r_{i.j}(n) \equiv 0 \ (mod \ i.j)$, then the $i.j$ integers $r_j(n).i + r_i(n).j$ are all incongruent and form a complete system of residues. It follows that $n = a.i$—whence i divides n—and also $n = b.j$—whence j divides n—if, and only if $r_i(n) = r_j(n) = r_{i.j}(n) = 0$.

The lemma follows. □

We thus have a formal proof of Theorem 31.9 that:

COROLLARY 36.11. *The prime divisors of any integer n are mutually independent.* □

CHAPTER 37

Density of integers not divisible by primes $Q = \{q_1, q_2, \ldots, q_k\}$

Continuing our consideration of prime distribution, we conclude from Lemma 36.3 and Corollary 36.11 that:

LEMMA 37.1. *The asymptotic density of the set of all integers that are not divisible by any of a given set of primes $Q = \{q_1, q_2, \ldots, q_k\}$ is:*

$\prod_{q \in Q}(1 - 1/q)$. □

It follows that:

LEMMA 37.2. *The expected number of integers in any interval (a,b) that are not divisible by any of a given set of primes $Q = \{q_1, q_2, \ldots, q_k\}$ is:*

$(b - a)\prod_{q \in Q}(1 - 1/q)$. □

37.1. The function $\pi_H(n)$

In particular, the expected number $\pi_H(n)$ of integers $\leq n$ that are not divisible by any of the primes $p_1, p_2, \ldots, p_{\pi(\sqrt{k})}$ is:

COROLLARY 37.3. $\pi_H(n) = n.\prod_{i=1}^{\pi(\sqrt{k})}(1 - \frac{1}{p_i})$.

It follows that:

COROLLARY 37.4. *The expected number of primes $\leq p^2_{\pi(\sqrt{n})+1}$ is (as illustrated in Chapter 35, Fig.1):*

$\pi_H(p^2_{\pi(\sqrt{n})+1}) = p^2_{\pi(\sqrt{n})+1}\prod_{i=1}^{\pi(\sqrt{n})}(1 - \frac{1}{p_i})$

with cumulative standard deviation:

$p_{\pi(\sqrt{n})+1}\sqrt{\prod_{i=1}^{\pi(\sqrt{n})}(1 - \frac{1}{p_i})(1 - \prod_{i=1}^{\pi(\sqrt{n})}(1 - \frac{1}{p_i}))}$. □

We conclude that $\pi_H(n)$ is a *non-heuristic* approximation of the number of primes $\leq n$:

LEMMA 37.5. $\pi(n) \approx \pi_H(n) = n.\prod_{i=1}^{\pi(\sqrt{n})}(1 - \frac{1}{p_i})$.

37.2. The function $\pi_L(n)$

It also follows immediately from Theorem 37.2 that:

COROLLARY 37.6. *The expected number of primes in the interval* $(p^2_{\pi(\sqrt{n})},\ p^2_{\pi(\sqrt{n})+1})$ *is (as illustrated in §35, Fig.2):*

$$(p^2_{\pi(\sqrt{n})+1} - p^2_{\pi(\sqrt{n})}) \prod_{i=1}^{\pi(\sqrt{n})} (1 - \tfrac{1}{p_i})$$

with standard binomial deviation:

$$\sqrt{(p^2_{\pi(\sqrt{n})+1} - p^2_{\pi(\sqrt{n})}) \prod_{i=1}^{\pi(\sqrt{n})} (1 - \tfrac{1}{p_i})(1 - \prod_{i=1}^{\pi(\sqrt{n})} (1 - \tfrac{1}{p_i}))}. \qquad \Box$$

It further follows from Lemma 37.2 and Corollary 37.6 that:

COROLLARY 37.7. *The number* $\pi(p^2_{\pi(\sqrt{n})+1})$ *of primes less than* $p^2_{\pi(\sqrt{n})+1}$ *is also approximated by the cumulative sum:*

$$\pi_L(p^2_{\pi(\sqrt{n})+1}) = \sum_{j=1}^{\pi(\sqrt{n})} \{(p^2_{j+1} - p^2_j) \prod_{i=1}^{j} (1 - \tfrac{1}{p_i})\}$$

with cumulative standard deviation:

$$\sum_{j=1}^{\pi(\sqrt{n})} \sqrt{(p^2_{j+1} - p^2_j) \prod_{i=1}^{j} (1 - \tfrac{1}{p_i})(1 - \prod_{i=1}^{j} (1 - \tfrac{1}{p_i}))}. \qquad \Box$$

We conclude that $\pi_L(n)$ is a cumulative *non-heuristic* approximation of the number of primes $\leq n$[1]:

LEMMA 37.8. $\pi(n) \approx \pi_L(n) = \sum_{j=1}^{n} \prod_{i=1}^{\pi(\sqrt{j})} (1 - \frac{1}{p_i})$.

It immediately follows from Lemma 37.5 and Lemma 37.8 that:

COROLLARY 37.9. $\pi_L(n) > \pi_H(n)$ *for all* $n \geq 9$.

37.3. The interval $(p^2_n,\ p^2_{n+1})$

It follows immediately from the definition of $\pi(x)$ as the number of primes less than or equal to x that:

LEMMA 37.10. $\prod_{i=1}^{\pi(\sqrt{x})} (1 - \frac{1}{p_i}) = \prod_{i=1}^{\pi(\sqrt{x+1})} (1 - \frac{1}{p_i})$ *for* $p^2_n \leq x < p^2_{n+1}$. $\Box$

We can thus generalise the number-theoretic function of Lemma 37.8 as the real-valued function:

DEFINITION 37.11. $\pi_L(x) = \pi_L(p^2_n) + (x - p^2_n) \prod_{i=1}^{n} (1 - \frac{1}{p_i})$ for $p^2_n \leq x < p^2_{n+1}$.

We note that the graph of $\pi_L(x)$ in the interval $(p^2_n,\ p^2_{n+1})$ for $n \geq 1$ is now a straight line with gradient $\prod_{i=1}^{n} (1 - \frac{1}{p_i})$, as illustrated in §35, Fig.3 where we defined $\pi_L(x)$ equivalently by:

$$\pi_L(x) = (x - p^2_n) \prod_{i=1}^{n} (1 - \tfrac{1}{p_i}) + \sum_{j=1}^{n-1} (p^2_{j+1} - p^2_j) \prod_{i=1}^{j} (1 - \tfrac{1}{p_i}) + 2$$

[1]Fig.12 in §42, and Fig.15 in §42.1, comparatively analyse the values of $\pi(n)$ and $\pi_L(n)$ for $4 \leq n \leq 1500$.

37.4. The functions $\pi_L(x)/\frac{x}{log_e x}$ and $\pi_H(x)/\frac{x}{log_e x}$

We consider next the function $\pi_L(x)/\frac{x}{log_e x}$ in the interval $(p_n^2,\ p_{n+1}^2)$:

$$\pi_L(x)/\frac{x}{log_e x} = (\pi_L(p_n^2) + (x - p_n^2)\prod_{i=1}^{n}(1 - \frac{1}{p_i}))/\frac{x}{log_e x}$$

This now yields the derivative $(\pi_L(x).\frac{log_e x}{x})'$ in the interval $(p_n^2,\ p_{n+1}^2)$ as:

$$\pi_L(x).(\frac{log_e x}{x})' + (\pi_L(x))'.\frac{log_e x}{x}$$

$$(\pi_L(p_n^2)+(x-p_n^2)\prod_{i=1}^{n}(1-\frac{1}{p_i})).(\frac{log_e x}{x})'+(\pi_L(p_n^2)+(x-p_n^2)\prod_{i=1}^{n}(1-\frac{1}{p_i}))'.\frac{log_e x}{x}$$

$$(\pi_L(p_n^2) + (x - p_n^2)\prod_{i=1}^{n}(1 - \frac{1}{p_i})).(\frac{1}{x^2} - \frac{log_e x}{x^2}) + (\prod_{i=1}^{n}(1 - \frac{1}{p_i})).\frac{log_e x}{x}$$

Since $p_n^2 \leq x < p_{n+1}^2$, by Mertens'[2] and Chebyshev's Theorems we can express the above as:

$$\sim (\pi_L(p_n^2) + \frac{e^{-\gamma}(x-p_n^2)}{log_e n}).(\frac{1}{x^2} - \frac{log_e x}{x^2}) + \frac{e^{-\gamma}.log_e x}{x.log_e n}$$

$$\sim (\frac{\pi_L(p_n^2)}{x} + \frac{e^{-\gamma}}{log_e n}(1 - \frac{p_n^2}{x})).\frac{(1-log_e x)}{x} + \frac{e^{-\gamma}.log_e x}{x.log_e n}$$

$$\sim (\frac{\pi_L(p_n^2)}{p_n^2}.\frac{p_n^2}{x} + \frac{e^{-\gamma}}{log_e n}(1 - \frac{p_n^2}{x})).\frac{(1-2.log_e p_n)}{p_n^2} + \frac{2.e^{-\gamma}.log_e p_n}{p_n^2.log_e n}$$

Since each term $\rightarrow 0$ as $n \rightarrow \infty$, we conclude that the function $\pi_L(x)/\frac{x}{log_e x}$ does not oscillate but tends to a limit as $x \rightarrow \infty$ since:

LEMMA 37.12. $(\pi_L(x)/\frac{x}{log_e x})' \in o(1)$. □

We further conclude that:

COROLLARY 37.13. $\pi_L(n) = \sum_{j=1}^{n}\prod_{i=1}^{\pi(\sqrt{j})}(1 - \frac{1}{p_i}) \sim a.\frac{n}{log_e n}$ *for some constant* a. □

We note that $a > 2.e^{-\gamma}$[3], since $\prod_{i=1}^{\pi(\sqrt{j})}(1 - \frac{1}{p_i}) \geq \prod_{i=1}^{\pi(\sqrt{n})}(1 - \frac{1}{p_i})$ for all $1 \leq j \leq n$, and it follows from Definition 37.3 that:

COROLLARY 37.14. $\pi_H(n) = n.\prod_{i=1}^{\pi(\sqrt{n})}(1 - \frac{1}{p_i}) \sim 2.e^{-\gamma}.\frac{n}{log_e n}$[4]. □

---

[2][**HW60**], Theorem 429, p.351.

[3] Where $2.e^{-\lambda} \approx 1.12292\ldots$; [**Gr95**], p.13.

[4]By Mertens' Theorem; since $log_e \pi(\sqrt{n}) \sim (log_e \sqrt{n} - log_e log_e \sqrt{n})$ by the Prime Number Theorem.

CHAPTER 38

Primes in an arithmetic progression

We consider now Dirichlet's Theorem, which is the assertion that if a and d are co-prime and $1 \leq a < d$, then the arithmetic progression $a + m.d$, where $m \geq 1$, contains an infinitude of (Dirichlet) primes.

We first note that, by Lemma 36.8:

LEMMA 38.1. *If p_i and p_j are two primes where $i \neq j$ then, for any $n \geq 2$, $\alpha, \beta \geq 1$, we have:*

$$\mathbb{P}((r_{p_i^\alpha}(n) = u) \cap (r_{p_j^\beta}(n) = v)) = \mathbb{P}(r_{p_i^\alpha}(n) = u).\mathbb{P}(r_{p_j^\beta}(n) = v)$$

where $p_i^\alpha > u \geq 0$ and $p_j^\beta > v \geq 0$. □

Now, the $p_i^\alpha.p_j^\beta$ numbers $d.p_i^\alpha + c.p_j^\beta$, where $p_i^\alpha > c \geq 0$ and $p_j^\beta > d \geq 0$, are all incongruent and form a complete system of residues[1] *mod* $(p_i^\alpha.p_j^\beta)$. It follows that $n = a.p_i^\alpha$—whence p_i^α divides n—and also $n = b.p_i^\beta$—whence p_i^β divides n—if, and only if $r_{p_i^\alpha}(n) = r_{p_i^\beta}(n) = 0$.

If $u = 0$ and $v = 0$ in Lemma 38.1, so that both p_i and p_j are prime divisors of n, we immediately conclude that:

$$\mathbb{P}((r_{p_i^\alpha}(n) = 0) \cap (r_{p_j^\beta}(n) = 0)) = \frac{1}{p_i^\alpha.p_j^\beta}$$

$$\mathbb{P}(r_{p_i^\alpha}(n) = 0).\mathbb{P}(r_{p_j^\beta}(n) = 0) = (\frac{1}{p_i^\alpha})(\frac{1}{p_j^\beta}).$$

COROLLARY 38.2. $\mathbb{P}((r_{p_i^\alpha}(n) = 0) \cap (r_{p_j^\beta}(n) = 0)) = \mathbb{P}(r_{p_i^\alpha}(n) = 0).\mathbb{P}(r_{p_j\beta}(n) = 0)$. □

It also immediately follows that Corollary 36.11 can be extended to prime powers in general[2]:

THEOREM 38.3. *For any two primes $p \neq q$ and natural numbers $n, \alpha, \beta \geq 1$, whether or not p^α divides n is independent of whether or not q^β divides n.* □

38.1. The asymptotic density of Dirichlet integers

We note next that:

[1][**HW60**], p.52, Theorem 59.

[2]*Hint*: The following arguments may be easier to follow if we visualise the residues $r_{p_i^\alpha}(n)$ and $r_{p_i^\beta}(n)$ as they would occur in §41, Fig.7 and Fig.8.

Lemma 38.4. *For any co-prime natural numbers* $1 \leq a < d = q_1^{\alpha_1}.q_2^{\alpha_2} \ldots q_k^{\alpha_k}$ *where:*

> $q_1 < q_2 < \ldots < q_k$ *are primes and* $\alpha_1, \alpha_2 \ldots \alpha_k \geq 1$ *are natural numbers;*

the natural number n *is of the form* $a + m.d$ *for some natural number* $m \geq 1$ *if, and only if:*

$$a + r_{q_i^{\alpha_i}}(n) \equiv 0 \ (mod\ q_i^{\alpha_i}) \text{ for all } 1 \leq i \leq k$$

where $0 \leq r_i(n) < i$ *is defined for all* $i > 1$ *by:*

$$n + r_i(n) \equiv 0 \ (mod\ i)\ .$$

Proof. First, if n is of the form $a + m.d$ for some natural number $m \geq 1$, where $1 \leq a < d = q_1^{\alpha_1}.q_2^{\alpha_2} \ldots q_k^{\alpha_k}$, then:

$$\begin{array}{rlll} & n & \equiv & a\ (mod\ d) \\ and: & n + r_{q_i^{\alpha_i}}(n) & \equiv & 0\ (mod\ q_i^{\alpha_i}) \quad for\ all\ 1 \leq i \leq k \\ whence: & a + r_{q_i^{\alpha_i}}(n) & \equiv & 0\ (mod\ q_i^{\alpha_i}) \quad for\ all\ 1 \leq i \leq k \end{array}$$

Second:

$$\begin{array}{rlll} If: & a + r_{q_i^{\alpha_i}}(n) & \equiv & 0\ (mod\ q_i^{\alpha_i}) \quad for\ all\ 1 \leq i \leq k \\ and: & n + r_{q_i^{\alpha_i}}(n) & \equiv & 0\ (mod\ q_i^{\alpha_i}) \quad for\ all\ 1 \leq i \leq k \\ then: & n - a & \equiv & 0\ (mod\ q_i^{\alpha_i}) \quad for\ all\ 1 \leq i \leq k \\ whence: & n & \equiv & a\ (mod\ d) \end{array}$$

The Lemma follows. □

By Lemma 36.3, it follows that:

Corollary 38.5. *The probability that* $a + r_{q_i^{\alpha_i}}(n) \equiv 0\ (mod\ q_i^{\alpha_i})$ *for any* $1 \leq i \leq k$ *is* $\frac{1}{q_i^{\alpha_i}}$. □

By Lemma 38.1 and Theorem 38.3, it further follows that:

Corollary 38.6. *The joint probability that* $a + r_{q_i^{\alpha_i}}(n) \equiv 0\ (mod\ q_i^{\alpha_i})$ *for all* $1 \leq i \leq k$ *is* $\prod_{i=1}^{k} \frac{1}{q_i^{\alpha_i}}$. □

We conclude by Lemma 38.4 that:

Corollary 38.7. *The asymptotic density of Dirichlet integers, defined as numbers of the form* $a + m.d$ *for some natural number* $m \geq 1$ *which are not divisible by any given set of primes* $\mathbb{R} = \{r_1, r_2, \ldots, r_l\}$*, where* $1 \leq a < d = q_1^{\alpha_1}.q_2^{\alpha_2} \ldots q_k^{\alpha_l}$ *is:*

$$\prod_{i=1}^{k} \frac{1}{q_i^{\alpha_i}} . \prod_{r \in \mathbb{R}\ \&\ r \neq q_i} (1 - \frac{1}{r}).$$

Proof. Since a, d are co-prime, we have by Lemma 38.4 that if n is of the form $a + m.d$ for some natural number $m \geq 1$, where $1 \leq a < d = q_1^{\alpha_1}.q_2^{\alpha_2} \ldots q_k^{\alpha_k}$, we have that:

$$\begin{array}{rlcll} & n & \equiv & a \ (mod\ q_i) & for\ all\ 1 \leq i \leq k \\ whilst: & n + r_i(n) & \equiv & 0 \ (mod\ i) & for\ all\ 1 \leq i \\ whence: & a + r_{q_i}(n) & \equiv & 0 \ (mod\ q_i) & for\ all\ 1 \leq i \leq k \\ & r_{q_i}(n) & \neq & 0 & for\ all\ 1 \leq i \leq k \\ and: & q_i & \not| & n & for\ all\ 1 \leq i \leq k \end{array}$$

Hence, if n is of the form $a + m.d$ for some natural number $m \geq 1$, where $1 \leq a < d = q_1^{\alpha_1}.q_2^{\alpha_2} \ldots q_k^{\alpha_k}$ and $(a, d) = 1$, the probability that $q_i \not| n$ for all $1 \leq i \leq k$ is 1.

By Lemma 37.1, Theorem 37.2 and Theorem 38.3, the asymptotic density of Dirichlet numbers of the form $a + m.d$ which are not divisible by any given set of primes $\mathbb{R} = \{r_1, r_2, \ldots, r_l\}$ is thus:

$$\prod_{i=1}^{k} \frac{1}{q_i^{\alpha_i}} . \prod_{r \in \mathbb{R}\ \&\ r \neq q_i} (1 - \frac{1}{r})$$

The Corollary follows. □

COROLLARY 38.8. *The expected number of Dirichlet integers in any interval* (a, b) *is:*

$$(b - a) \prod_{i=1}^{k} \frac{1}{q_i^{\alpha_i}} . \prod_{i=1}^{k} (1 - \frac{1}{q_i})^{-1} . \prod_{r \in \mathbb{R}} (1 - \frac{1}{r}).$$ □

38.2. An elementary non-heuristic proof of Dirichlet's Theorem

Since n is a prime if, and only if, it is not divisible by any prime $p \leq \sqrt{n}$, it follows that the number $\pi_{(a,d)}(n)$ of Dirichlet primes, of the form $a + m.d$ for some natural number $m \geq 1$ and $1 \leq a < d = q_1^{\alpha_1}.q_2^{\alpha_2} \ldots q_k^{\alpha_k}$, that are less than or equal to any $n \geq q_k^2$ is cumulatively approximated by the non-heuristic Dirichlet prime counting function:

DEFINITION 38.9. $\pi_D(n) = \sum_{l=1}^{n} (\prod_{i=1}^{k} \frac{1}{q_i^{\alpha_i}} . \prod_{i=1}^{k} (1 - \frac{1}{q_i})^{-1} . \prod_{j=1}^{\pi(\sqrt{l})} (1 - \frac{1}{p_j})).$

We conclude that:

LEMMA 38.10. $\pi_{(a,d)}(n) \approx \pi_D(n) \to \infty$ *as* $n \to \infty$.

PROOF. If a, d are co-prime and $1 \leq a < d = q_1^{\alpha_1}.q_2^{\alpha_2} \ldots q_k^{\alpha_k}$, we have for any $n \geq q_k^2$:

$$\begin{array}{rcl} \pi_D(n) & = & \sum_{l=1}^{n} (\prod_{i=1}^{k} \frac{1}{q_i^{\alpha_i}} . \prod_{i=1}^{k} (1 - \frac{1}{q_i})^{-1} . \prod_{j=1}^{\pi(\sqrt{l})} (1 - \frac{1}{p_j})) \\ & = & \prod_{i=1}^{k} \frac{1}{q_i^{\alpha_i}} . \prod_{i=1}^{k} (1 - \frac{1}{q_i})^{-1} . \sum_{l=1}^{n} \prod_{j=1}^{\pi(\sqrt{l})} (1 - \frac{1}{p_j}) \\ & \geq & \prod_{i=1}^{k} \frac{1}{q_i^{\alpha_i}} . \prod_{i=1}^{k} (1 - \frac{1}{q_i})^{-1} .n. \prod_{j=1}^{\pi(\sqrt{n})} (1 - \frac{1}{p_j}) \end{array}$$

Since, by Mertens' Theorem, $\prod_{p \leq x} (1 - \frac{1}{p}) \sim \frac{e^{-\lambda}}{log_e x}$, we have that:

$$n. \prod_{j=1}^{\pi(\sqrt{n})} (1 - \frac{1}{p_j}) \sim \frac{2e^{-\gamma} n}{log_e(n)} \to \infty \text{ as } n \to \infty.$$

the lemma follows. □

Since $p^2_{n+1} - p^2_n \to \infty$ as $n \to \infty$, we conclude further that:

THEOREM 38.11. *There are an infinity of primes in any arithmetic progression* $a + m.d$ *where* $(a, d) = 1$[3]. □

[3]Compare [**HW60**], p.13, Theorem 15*.

CHAPTER 39

A non-heuristic proof that there are infinite twin-primes

We define $\pi_2(n)$ as the number of integers $p \leq n$ such that both p and $p+2$ are prime.

In order to estimate $\pi_2(n)$, we first define:

DEFINITION 39.1. An integer n is a $\mathbb{TW}(k)$ integer if, and only if, $r_{p_i}(n) \neq 0$ and $r_{p_i}(n) \neq 2$ for all $1 \leq i \leq k$, where $0 \leq r_i(n) < i$ is defined for all $i > 1$ by:

$$n + r_i(n) \equiv 0 \ (mod\ i)\ .$$

We note that:

LEMMA 39.2. *If n is a $\mathbb{TW}(k)$ integer, then both n and $n+2$ are not divisible by any of the first k primes $\{p_1, p_2, \ldots, p_k\}$.*

PROOF. The lemma follows immediately from Definition 39.1, Definition 6.9 and Lemma 36.2. □

Since each residue $r_i(n)$ cycles over the i values $(i-1, i-2, \ldots, 0)$, these values are all incongruent and form a complete system of residues *mod* i.

It thus follows from Definition 39.1 that the asymptotic density of $\mathbb{TW}(k)$ integers over the set of natural numbers is:

LEMMA 39.3. $\mathbb{D}(\mathbb{TW}(k)) = \prod_{i=2}^{k}(1 - \frac{2}{p_i})$. □

We also have that:

LEMMA 39.4. *If $p_k^2 \leq n \leq p_{k+1}^2$ is a $\mathbb{TW}(k)$ integer, then n is a prime and either $n+2$ is also a prime, or $n+2 = p_{k+1}^2$.*

PROOF. By Definition 39.1 and Definition 6.9:

$$\begin{array}{lcl} r_{p_i}(n) & \neq & 2 \ for\ all\ 1 \leq i \leq k \\ n+2 & \neq & \lambda.p_i \ for\ all\ 1 \leq i \leq k,\ \lambda \geq 1 \end{array}$$

Hence n is prime; and either $n+2$ is divisible by p_{k+1}, in which case $n+2 = p_{k+1}^2$, or it is a prime. □

If we define $\pi_{\mathbb{TW}(k)}(n)$ as the number of $\mathbb{TW}(k)$ integers $\leq n$, by Lemma 39.3 the expected number of $\mathbb{TW}(k)$ integers in any interval (a, b) is given—with a binomial standard deviation—by:

LEMMA 39.5. $\pi_{\mathbb{TW}(k)}(b) - \pi_{\mathbb{TW}(k)}(a) \approx (b-a)\prod_{i=2}^{k}(1 - \frac{2}{p_i})$. □

Since n is a prime if, and only if, it is not divisible by any prime $p \leq \sqrt{n}$, it follows from Lemma 39.4 that $\pi_{_{\mathbb{TW}(k)}}(p_{_{k+1}}^2) - \pi_{_{\mathbb{TW}(k)}}(p_{_k}^2)$ is at most one less than the number of twin-primes in the interval $(p_{_{k+1}}^2 - p_{_k}^2)$.

LEMMA 39.6. $\pi_{_{\mathbb{TW}(k)}}(p_{_{k+1}}^2) - \pi_{_{\mathbb{TW}(k)}}(p_{_k}^2) + 1 \geq \pi_{_2}(p_{_{k+1}}^2) - \pi_{_2}(p_{_{k)}}^2) \geq \pi_{_{\mathbb{TW}(k)}}(p_{_{k+1}}^2) - \pi_{_{\mathbb{TW}(k)}}(p_{_k}^2)$

Now, by Lemma 39.5 the expected number of $\mathbb{TW}(k)$ integers in the interval $(p_{_{k+1}}^2 - p_{_k}^2)$ is given by:

LEMMA 39.7. $\pi_{_{\mathbb{TW}(k)}}(p_{_{k+1}}^2) - \pi_{_{\mathbb{TW}(k)}}(p_{_k}^2) \approx (p_{_{k+1}}^2 - p_{_k}^2) \prod_{i=2}^{k}(1 - \frac{2}{p_{_i}})$. □

We conclude that the number $\pi_{_2}(p_{_{k+1}}^2)$ of twin primes $\leq p_{_{k+1}}^2$ is given by the cumulative non-heuristic approximation:

LEMMA 39.8. $\sum_{j=1}^{k}(\pi_{_2}(p_{_{j+1}}^2) - \pi_{_2}(p_{_j}^2)) = \pi_{_2}(p_{_{k+1}}^2) \approx \sum_{j=1}^{k}(p_{_{j+1}}^2 - p_{_j}^2) \prod_{i=2}^{j}(1 - \frac{2}{p_{_i}})$. □

We further conclude that:

THEOREM 39.9. $\pi_{_2}(n) \to \infty$ *as* $n \to \infty$.

PROOF. We have that, for $k \geq 2$:

$$\begin{aligned}
\sum_{j=1}^{k}(p_{_{j+1}}^2 - p_{_j}^2) \prod_{i=2}^{j}(1 - \tfrac{2}{p_{_i}}) &= \sum_{j=9}^{p_{_{k+1}}^2} \prod_{i=2}^{\pi(\sqrt{j})-1}(1 - \tfrac{2}{p_{_i}}) \\
&\geq (p_{_{k+1}}^2 - 9). \prod_{i=2}^{k}(1 - \tfrac{2}{p_{_i}}) \\
&\geq (p_{_{k+1}}^2 - 9). \prod_{i=2}^{k}(1 - \tfrac{1}{p_{_i}})(1 - \tfrac{1}{(p_{_i}-1)}) \\
&\geq (p_{_{k+1}}^2 - 9). \prod_{i=2}^{k}(1 - \tfrac{1}{p_{_i}})(1 - \tfrac{1}{p_{_{i-1}}}) \\
&\geq (p_{_{k+1}}^2 - 9). \prod_{i=2}^{k}(1 - \tfrac{1}{p_{_{i-1}}})^2 \\
&\geq (p_{_{k+1}}^2 - 9). \prod_{i=1}^{k}(1 - \tfrac{1}{p_{_i}})^2
\end{aligned}$$

Now, by Mertens' Theorem, we have that:

$$\begin{aligned}
(p_{_{k+1}}^2 - 9). \prod_{i=1}^{k}(1 - \tfrac{1}{p_{_i}})^2 &\sim (p_{_{k+1}}^2 - 9).(\tfrac{e^{-\gamma}}{log_e k})^2 \\
&\to \infty \textit{ as } n \to \infty
\end{aligned}$$

The theorem follows by Lemma 39.8. □

CHAPTER 40

The Generalised Prime Counting Function: $\sum_{j=1}^{n}\prod_{i=a}^{\pi(\sqrt{j})}(1-\frac{b}{p_i})$

We note that the argument of Theorem 39.9 in Chapter 39 is a special case of the behaviour as $n \rightarrow \infty$ of the Generalised Prime Counting Function $\sum_{j=1}^{n}\prod_{i=a}^{\pi(\sqrt{j})}(1-\frac{b}{p_i})$, which estimates the number of integers $\leq n$ such that there are b values that cannot occur amongst the residues $r_{p_i}(n)$ for $a \leq i \leq \pi(\sqrt{j})$[1]:

THEOREM 40.1. $\sum_{j=1}^{n}\prod_{i=a}^{\pi(\sqrt{j})}(1-\frac{b}{p_i}) \rightarrow \infty$ *as* $n \rightarrow \infty$ *if* $p_a > b \geq 1$.

PROOF. For $p_a > b \geq 1$, we have that:

$$\begin{aligned}
\sum_{j=1}^{n}\prod_{i=a}^{\pi(\sqrt{j})}(1-\tfrac{b}{p_i}) &\geq \sum_{j=p_a^2}^{n}\prod_{i=a}^{\pi(\sqrt{j})}(1-\tfrac{b}{p_i})\\
&\geq \sum_{j=p_a^2}^{n}\prod_{i=a}^{\pi(\sqrt{n})}(1-\tfrac{b}{p_i})\\
&\geq (n-p_a^2).\prod_{i=a}^{\pi(\sqrt{n})}(1-\tfrac{b}{p_i})\\
&\geq (n-p_a^2).\prod_{i=a}^{n}(1-\tfrac{b}{p_i})
\end{aligned}$$

The theorem follows if:

$$log_e(n-p_a^2)+\sum_{i=a}^{n}log_e(1-\tfrac{b}{p_i}) \rightarrow \infty$$

(i) We note first the standard result for $|x| < 1$ that:

$$log_e(1-x) = -\sum_{m=1}^{\infty}\frac{x^m}{m}$$

For any $p_i > b \geq 1$, we thus have:

$$log_e(1-\tfrac{b}{p_i}) = -\sum_{m=1}^{\infty}\frac{(b/p_i)^m}{m} = -\frac{b}{p_i}-\sum_{m=2}^{\infty}\frac{(b/p_i)^m}{m}$$

Hence:

$$\sum_{i=a}^{n}log_e(1-\tfrac{b}{p_i}) = -\sum_{i=a}^{n}(\tfrac{b}{p_i})-\sum_{i=a}^{n}(\sum_{m=2}^{\infty}\frac{(b/p_i)^m}{m})$$

(ii) We note next that, for all $i \geq a$:

$$c < (1-\tfrac{b}{p_a}) \rightarrow c < (1-\tfrac{b}{p_i})$$

It follows for any such c that:

[1]Thus $b = 1$ yields an estimate for the number of primes $\leq n$, and $b = 2$ an estimate for the number of TW primes (Definition 39.1) $\leq n$.

$$\sum_{m=2}^{\infty}\frac{(b/p_i)^m}{m} \leq \sum_{m=2}^{\infty}(\frac{b}{p_i})^m = \frac{(b/p_i)^2}{1-b/p_i} \leq \frac{b^2}{c.p_i^2}$$

Since:

$$\sum_{i=1}^{\infty}\frac{1}{p_i^2} = O(1)$$

it further follows that:

$$\sum_{i=a}^{n}(\sum_{m=2}^{\infty}\frac{(b/p_i)^m}{m}) \leq \sum_{i=a}^{n}(\frac{b^2}{c.p_i^2}) = O(1)$$

(iii) From the standard result[2]:

$$\sum_{p\leq x}\frac{1}{p} = log_e log_e x + O(1) + o(1)$$

it then follows that:

$$\begin{aligned}\sum_{i=a}^{n} log_e(1-\frac{b}{p_i}) &\geq -\sum_{i=a}^{n}(\frac{b}{p_i}) - O(1)\\ &\geq -b.(log_e log_e n + O(1) + o(1)) - O(1)\end{aligned}$$

The theorem follows since:

$$log_e(n-p_a^2) - b.(log_e log_e n + O(1) + o(1)) - O(1) \to \infty$$

and so:

$$log_e(n-p_a^2) + \sum_{i=a}^{n} log_e(1-\frac{b}{p_i}) \to \infty \qquad \square$$

[2][**HW60**], p.351, Theorem 427.

CHAPTER 41

Algorithms for generating the residue function $r_i(n)$

We graphically illustrate how the residues $r_i(n)$ occur naturally as values of:

A: The natural-number based residue sequences R_i;

B: The natural-number based residue sequences $E(n)$;

and as the output of:

C: The natural-number based algorithm $E_{\mathbb{N}}$;

D: The prime-number based algorithm $E_{\mathbb{P}}$;

E: The prime-number based algorithm $E_{\mathbb{Q}}$.

A: The natural-number based sequences $R_i(n)$

Density: For instance, the residues $r_i(n)$ can be defined for all $n \geq 1$ as the values of the sequences $R_i(n)$, defined for all $i \geq 1$, as illustrated below in Fig.1[1], where:

- For any $i \geq 2$, each sequence $R_i(n)$ cycles through the values $(i-1,\ i-2,\ \ldots,\ 0)$ with period i;
- For any $i \geq 2$ the asymptotic density—over the set of natural numbers—of the set $\{n\}$ of integers that are divisible by i is $\frac{1}{i}$; and the asymptotic density of integers that are not divisible by i is $\frac{i-1}{i}$.

Sequence:	R_1	R_2	R_3	R_4	R_5	R_6	R_7	R_8	R_9	R_{10}	R_{11}	$\ldots R_n$
$n=1$	0	1	2	3	4	5	6	7	8	9	10	...n-1
$n=2$	0	0	1	2	3	4	5	6	7	8	9	...n-2
$n=3$	0	1	0	1	2	3	4	5	6	7	8	...n-3
$n=4$	0	0	2	0	1	2	3	4	5	6	7	...n-4
$n=5$	0	1	1	3	0	1	2	3	4	5	6	...n-5
$n=6$	0	0	0	2	4	0	1	2	3	4	5	...n-6
$n=7$	0	1	2	1	3	5	0	1	2	3	4	...n-7
$n=8$	0	0	1	0	2	4	6	0	1	2	3	...n-8
$n=9$	0	1	0	3	1	3	5	7	0	1	2	...n-9
$n=10$	0	0	2	2	0	2	4	6	8	0	1	...n-10
$n=11$	0	1	1	1	4	1	3	5	7	9	0	...n-11
n	r_1	r_2	r_3	r_4	r_5	r_6	r_7	r_8	r_9	r_{10}	r_{11}	...0

Fig.1: The natural-number based residue sequences $R_i(n)$

B: The natural-number based sequences $E(n)$

Primality: The residues $r_i(n)$ can also be viewed alternatively as values of the associated sequences, $E(n) = \{r_i(n) : i \geq 1\}$, defined for all $n \geq 1$, as illustrated below in Fig.2, where:

- The sequences $E(n)$ highlighted in red correspond to a prime[2] p (since $r_i(p) \neq 0$ for $1 < i < p$) in the usual, linearly displayed, Eratosthenes sieve:

  $E(\cancel{1}),\ E(2),\ E(3),\ E(\cancel{4}),\ E(5),\ E(\cancel{6}),\ E(7),\ E(\cancel{8}),\ E(\cancel{9}),\ E(\cancel{10}),\ E(11),\ \ldots$

[1] For r_i read $r_i(n)$; for R_i read $R_i(n)$.

[2] Conventionally defined as integers that are not divisible by any smaller integer other than 1.

- The sequences highlighted in cyan identify a crossed out composite n (since $r_i(n) = 0$ for some $i < i < n$) in the usual, linearly displayed, Eratosthenes sieve.
- The 'boundary' residues $r_1(n) = 0$ and $r_n(n) = 0$ are identified in cyan.

Sequence:	R_1	R_2	R_3	R_4	R_5	R_6	R_7	R_8	R_9	R_{10}	R_{11}	$\ldots R_n$
$E(1)$:	0	1	2	3	4	5	6	7	8	9	10	. . . n-1
$E(2)$:	0	0	1	2	3	4	5	6	7	8	9	. . . n-2
$E(3)$:	0	1	0	1	2	3	4	5	6	7	8	. . . n-3
$E(4)$:	0	0	2	0	1	2	3	4	5	6	7	. . . n-4
$E(5)$:	0	1	1	3	0	1	2	3	4	5	6	. . . n-5
$E(6)$:	0	0	0	2	4	0	1	2	3	4	5	. . . n-6
$E(7)$:	0	1	2	1	3	5	0	1	2	3	4	. . . n-7
$E(8)$:	0	0	1	0	2	4	6	0	1	2	3	. . . n-8
$E(9)$:	0	1	0	3	1	3	5	7	0	1	2	. . . n-9
$E(10)$:	0	0	2	2	0	2	4	6	8	0	1	. . . n-10
$E(11)$:	0	1	1	1	4	1	3	5	7	9	0	. . . n-11
. . .												
$E(n)$:	r_1	r_2	r_3	r_4	r_5	r_6	r_7	r_8	r_9	r_{10}	r_{11}	. . . 0
. . .												

Fig.2: The natural-number based residue sequences $E(n)$

C: The output of a natural-number based algorithm $E_{\mathbb{N}}$

We give below in Fig.3 the output for $1 \leq n \leq 11$ of a natural-number based algorithm $E_{\mathbb{N}}$ that computes the values $r_i(n)$ of the sequence $E_{\mathbb{N}}(n)$ for only $1 \leq i \leq n$ for any given n.

Divisors:	1	2	3	4	5	6	7	8	9	10	11	. . . n . . .
$E_{\mathbb{N}}(1)$:	0											
$E_{\mathbb{N}}(2)$:	0	0										
$E_{\mathbb{N}}(3)$:	0	1	0									
$E_{\mathbb{N}}(4)$:	0	0	2	0								
$E_{\mathbb{N}}(5)$:	0	1	1	3	0							
$E_{\mathbb{N}}(6)$:	0	0	0	2	4	0						
$E_{\mathbb{N}}(7)$:	0	1	2	1	3	5	0					
$E_{\mathbb{N}}(8)$:	0	0	1	0	2	4	6	0				
$E_{\mathbb{N}}(9)$:	0	1	0	3	1	3	5	7	0			
$E_{\mathbb{N}}(10)$:	0	0	2	2	0	2	4	6	8	0		
$E_{\mathbb{N}}(11)$:	0	1	1	1	4	1	3	5	7	9	0	
. . .												
$E_{\mathbb{N}}(n)$:	r_1	r_2	r_3	r_4	r_5	r_6	r_7	r_8	r_9	r_{10}	r_{11}	. . . 0
. . .												

Fig.3: The output of the natural-number based algorithm $E_{\mathbb{N}}$

D: The output of the prime-number based algorithm $E_{\mathbb{P}}$

Fig.4 gives the output for $2 \leq n \leq 31$ of a prime-number based algorithm $E_{\mathbb{Q}}$ that computes the values $q_i(n) = r_{p_i}(n)$ of the sequence $E_{\mathbb{P}}(n)$ for only each prime $2 \leq p_i \leq n$ for any given n.

Prime: Divisor:	p_1 2	p_2 3	p_3 5	p_4 7	p_5 11	p_6 13	p_7 17	p_8 19	p_9 23	p_{10} 29	p_{11} 31	$\ldots p_n \ldots$ $\ldots p_n \ldots$
$E_{\mathbb{P}}(2)$:	0											
$E_{\mathbb{P}}(3)$:	1	0										
$E_{\mathbb{P}}(4)$:	0	2										
$E_{\mathbb{P}}(5)$:	1	1	0									
$E_{\mathbb{P}}(6)$:	0	0	4									
$E_{\mathbb{P}}(7)$:	1	2	3	0								
$E_{\mathbb{P}}(8)$:	0	1	2	6								
$E_{\mathbb{P}}(9)$:	1	0	1	5								
$E_{\mathbb{P}}(10)$:	0	2	0	4								
$E_{\mathbb{P}}(11)$:	1	1	4	3	0							
$E_{\mathbb{P}}(12)$:	0	0	3	2	10							
$E_{\mathbb{P}}(13)$:	1	2	2	1	9	0						
$E_{\mathbb{P}}(14)$:	0	1	1	0	8	12						
$E_{\mathbb{P}}(15)$:	1	0	0	6	7	11						
$E_{\mathbb{P}}(16)$:	0	2	4	5	6	10						
$E_{\mathbb{P}}(17)$:	1	1	3	4	5	9	0					
$E_{\mathbb{P}}(18)$:	0	0	2	3	4	8	16					
$E_{\mathbb{P}}(19)$:	1	2	1	2	3	7	15	0				
$E_{\mathbb{P}}(20)$:	0	1	0	1	2	6	14	18				
$E_{\mathbb{P}}(21)$:	1	0	4	0	1	5	13	17				
$E_{\mathbb{P}}(22)$:	0	2	3	6	0	4	12	16				
$E_{\mathbb{P}}(23)$:	1	1	2	5	10	3	11	15	0			
$E_{\mathbb{P}}(24)$:	0	0	1	4	9	2	10	14	22			

$E_{\mathbb{P}}(25)$:	1	2	0	3	8	1	9	13	21			
$E_{\mathbb{P}}(26)$:	0	1	4	2	7	0	8	12	20			
$E_{\mathbb{P}}(27)$:	1	0	3	1	6	12	7	11	19			
$E_{\mathbb{P}}(28)$:	0	2	2	0	5	11	6	10	18			
$E_{\mathbb{P}}(29)$:	1	1	1	6	4	10	5	9	17	0		
$E_{\mathbb{P}}(30)$:	0	0	0	5	3	9	4	8	16	28		
$E_{\mathbb{P}}(31)$:	1	2	4	4	2	8	3	7	15	27	0	
...												
$E_{\mathbb{P}}(n)$:	q_1	q_2	q_3	q_4	q_5	q_6	q_7	q_8	q_9	q_{10}	q_{11}	$\ldots 0$
...												

Fig.4: The output of the prime-number based algorithm $E_{\mathbb{P}}$

E: The output of the prime-number based algorithms $E_{\mathbb{P}}$ and $E_{\mathbb{Q}}$

We give below in Fig.5 the output for $2 \leq n \leq 121$ of the two prime-number based algorithms $E_{\mathbb{P}}$ (whose output $\{q_i(n) = r_{p_i}(n) : 1 \leq i \leq \pi(n)\}$ is shown only partially, partly in cyan) and $E_{\mathbb{Q}}$ (whose output $q_i(n) = \{r_{p_i}(n) : 1 \leq i \leq \pi(\sqrt{n})\}$ is highlighted in black and red, the latter indicating the generation of a prime sequence[3].

Prime:	p_1	p_2	p_3	p_4	p_5	p_6	p_7	p_8	p_9	p_{10}	p_{11}	$\ldots p_n \ldots$
Divisor:	2	3	5	7	11	13	17	19	23	29	31	$\ldots p_n \ldots$
Function:	Q_1	Q_2	Q_3	Q_4	Q_5	Q_6	Q_7	Q_8	Q_9	Q_{10}	Q_{11}	$\ldots$
$E_{\mathbb{Q}}(2)$:	0	(Prime by definition)										
$E_{\mathbb{Q}}(3)$:	1	0										
$E_{\mathbb{Q}}(4)$:	0	2										
$E_{\mathbb{Q}}(5)$:	1	1	0									
$E_{\mathbb{Q}}(6)$:	0	0	4									
$E_{\mathbb{Q}}(7)$:	1	2	3	0								
$E_{\mathbb{Q}}(8)$:	0	1	2	6								
$E_{\mathbb{Q}}(9)$:	1	0	1	5								
$E_{\mathbb{Q}}(10)$:	0	2	0	4								
$E_{\mathbb{Q}}(11)$:	1	1	4	3	0							
$E_{\mathbb{Q}}(12)$:	0	0	3	2	10							
$E_{\mathbb{Q}}(13)$:	1	2	2	1	9	0						
$E_{\mathbb{Q}}(14)$:	0	1	1	0	8	12						
$E_{\mathbb{Q}}(15)$:	1	0	0	6	7	11						
$E_{\mathbb{Q}}(16)$:	0	2	4	5	6	10						
$E_{\mathbb{Q}}(17)$:	1	1	3	4	5	9	0					
$E_{\mathbb{Q}}(18)$:	0	0	2	3	4	8	16					
$E_{\mathbb{Q}}(19)$:	1	2	1	2	3	7	15	0				
$E_{\mathbb{Q}}(20)$:	0	1	0	1	2	6	14	18				
$E_{\mathbb{Q}}(21)$:	1	0	4	0	1	5	13	17				
$E_{\mathbb{Q}}(22)$:	0	2	3	6	0	4	12	16				
$E_{\mathbb{Q}}(23)$:	1	1	2	5	10	3	11	15	0			
$E_{\mathbb{Q}}(24)$:	0	0	1	4	9	2	10	14	22			
$E_{\mathbb{Q}}(25)$:	1	2	0	3	8	1	9	13	21			
$E_{\mathbb{Q}}(26)$:	0	1	4	2	7	0	8	12	20			
$E_{\mathbb{Q}}(27)$:	1	0	3	1	6	12	7	11	19			
$E_{\mathbb{Q}}(28)$:	0	2	2	0	5	11	6	10	18			
$E_{\mathbb{Q}}(29)$:	1	1	1	6	4	10	5	9	17	0		
$E_{\mathbb{Q}}(30)$:	0	0	0	5	3	9	4	8	16	28		
$E_{\mathbb{Q}}(31)$:	1	2	4	4	2	8	3	7	15	27	0	
$E_{\mathbb{Q}}(32)$:	0	1	3	3	1	7	2	6	14	26	30	
$E_{\mathbb{Q}}(33)$:	1	0	2	2	0	6	1	5	13	25	29	
$E_{\mathbb{Q}}(34)$:	0	2	1	1	10	5	0	4	12	24	28	
$E_{\mathbb{Q}}(35)$:	1	1	0	0	9	4	16	3	11	23	27	
$E_{\mathbb{Q}}(36)$:	0	0	4	6	8	3	15	2	10	22	26	
$E_{\mathbb{Q}}(37)$:	1	2	3	5	7	2	14	1	9	21	25	
$E_{\mathbb{Q}}(38)$:	0	1	2	4	6	1	13	0	8	20	24	
$E_{\mathbb{Q}}(39)$:	1	0	1	3	5	0	12	18	7	19	23	
$E_{\mathbb{Q}}(40)$:	0	2	0	2	4	12	11	17	6	18	22	
$E_{\mathbb{Q}}(41)$:	1	1	4	1	3	11	10	16	5	17	21	
$E_{\mathbb{Q}}(42)$:	0	0	3	0	2	10	9	15	4	16	20	
$E_{\mathbb{Q}}(43)$:	1	2	2	6	1	9	8	14	3	15	19	
$E_{\mathbb{Q}}(44)$:	0	1	1	5	0	8	7	13	2	14	18	
$E_{\mathbb{Q}}(45)$:	1	0	0	4	10	7	6	12	1	13	17	
$E_{\mathbb{Q}}(46)$:	0	2	4	3	9	6	5	11	0	12	16	
$E_{\mathbb{Q}}(47)$:	1	1	3	2	8	5	4	10	22	11	15	
$E_{\mathbb{Q}}(48)$:	0	0	2	1	7	4	3	9	21	10	14	
$E_{\mathbb{Q}}(49)$:	1	2	1	0	6	3	2	8	20	9	13	
$E_{\mathbb{Q}}(50)$:	0	1	0	6	5	2	1	7	19	8	12	
$E_{\mathbb{Q}}(51)$:	1	0	4	5	4	1	0	6	18	7	11	

[3]For informal reference and perspective, formal definitions of both the prime-number based algorithms $E_{\mathbb{P}}$ and $E_{\mathbb{Q}}$ are given in this work in progress *Factorising all $m \leq n$ is of order* $\Theta(\sum_{i=2}^{n} \pi(\sqrt{i}))$.

$E_{\mathbb{Q}}(52)$:	0	2	3	4	3	0	16	5	17	6	10	
$E_{\mathbb{Q}}(53)$:	1	1	2	3	2	12	15	4	16	5	9	
$E_{\mathbb{Q}}(54)$:	0	0	1	2	1	11	14	3	15	4	8	
$E_{\mathbb{Q}}(55)$:	1	2	0	1	0	10	13	2	14	3	7	
$E_{\mathbb{Q}}(56)$:	0	1	4	0	10	9	12	1	13	2	6	
$E_{\mathbb{Q}}(57)$:	1	0	3	6	9	8	11	0	12	1	5	
$E_{\mathbb{Q}}(58)$:	0	2	2	5	8	7	10	18	11	0	4	
$E_{\mathbb{Q}}(59)$:	1	1	1	4	7	6	9	17	10	28	3	
$E_{\mathbb{Q}}(60)$:	0	0	0	3	6	5	8	16	9	27	2	
$E_{\mathbb{Q}}(61)$:	1	2	4	2	5	4	7	15	8	26	1	
$E_{\mathbb{Q}}(62)$:	0	1	3	1	4	3	6	14	7	25	0	
$E_{\mathbb{Q}}(63)$:	1	0	2	0	3	2	5	13	6	24	30	
$E_{\mathbb{Q}}(64)$:	0	2	1	6	2	1	4	12	5	23	29	
$E_{\mathbb{Q}}(65)$:	1	1	0	5	1	0	3	11	4	22	28	
$E_{\mathbb{Q}}(66)$:	0	0	4	4	0	12	2	10	3	21	27	
$E_{\mathbb{Q}}(67)$:	1	2	3	3	10	11	1	9	2	20	26	
$E_{\mathbb{Q}}(68)$:	0	1	2	2	9	10	0	8	1	19	25	
$E_{\mathbb{Q}}(69)$:	1	0	1	1	8	9	16	7	0	18	24	
$E_{\mathbb{Q}}(70)$:	0	2	0	0	7	8	15	6	22	17	23	
$E_{\mathbb{Q}}(71)$:	1	1	4	6	6	7	14	5	21	16	22	
$E_{\mathbb{Q}}(72)$:	0	0	3	5	5	6	13	4	20	15	21	
$E_{\mathbb{Q}}(73)$:	1	2	2	4	4	5	12	3	19	14	20	
$E_{\mathbb{Q}}(74)$:	0	1	1	3	3	4	11	2	18	13	19	
$E_{\mathbb{Q}}(75)$:	1	0	0	2	2	3	10	1	17	12	18	
$E_{\mathbb{Q}}(76)$:	0	2	4	1	1	2	9	0	16	11	17	
$E_{\mathbb{Q}}(77)$:	1	1	3	0	0	1	8	18	15	10	16	
$E_{\mathbb{Q}}(78)$:	0	0	2	6	10	0	7	17	14	9	15	
$E_{\mathbb{Q}}(79)$:	1	2	1	5	9	12	6	16	13	8	14	
$E_{\mathbb{Q}}(80)$:	0	1	0	4	8	11	5	15	12	7	13	
$E_{\mathbb{Q}}(81)$:	1	0	4	3	7	10	4	14	11	6	12	
$E_{\mathbb{Q}}(82)$:	0	2	3	2	6	9	3	13	10	5	11	
$E_{\mathbb{Q}}(83)$:	1	1	2	1	5	8	2	12	9	4	10	
$E_{\mathbb{Q}}(84)$:	0	0	1	0	4	7	1	11	8	3	9	
$E_{\mathbb{Q}}(85)$:	1	2	0	6	3	6	0	10	7	2	8	
$E_{\mathbb{Q}}(86)$:	0	1	4	5	2	5	16	9	6	1	7	
$E_{\mathbb{Q}}(87)$:	1	0	3	4	1	4	15	8	5	0	6	
$E_{\mathbb{Q}}(88)$:	0	2	2	3	0	3	14	7	4	28	5	
$E_{\mathbb{Q}}(89)$:	1	1	1	2	10	2	13	6	3	27	4	
$E_{\mathbb{Q}}(90)$:	0	0	0	1	9	1	12	5	2	26	3	
$E_{\mathbb{Q}}(91)$:	1	2	4	0	8	0	11	4	1	25	2	
$E_{\mathbb{Q}}(92)$:	0	1	3	6	7	12	10	3	0	24	1	
$E_{\mathbb{Q}}(93)$:	1	0	2	5	6	11	9	2	22	23	0	
$E_{\mathbb{Q}}(94)$:	0	2	1	4	5	10	8	1	21	22	30	
$E_{\mathbb{Q}}(95)$:	1	1	0	3	4	9	7	0	20	21	29	
$E_{\mathbb{Q}}(96)$:	0	0	4	2	3	8	6	18	19	20	28	
$E_{\mathbb{Q}}(97)$:	1	2	3	1	2	7	5	17	18	19	27	
$E_{\mathbb{Q}}(98)$:	0	1	2	0	1	6	4	16	17	18	26	
$E_{\mathbb{Q}}(99)$:	1	0	1	6	0	5	3	15	16	17	25	
$E_{\mathbb{Q}}(100)$:	0	2	0	5	10	4	2	14	15	16	24	
$E_{\mathbb{Q}}(101)$:	1	1	4	4	9	3	1	13	14	15	23	
$E_{\mathbb{Q}}(102)$:	0	0	3	3	8	2	0	12	13	14	22	
$E_{\mathbb{Q}}(103)$:	1	2	2	2	7	1	16	11	12	13	21	
$E_{\mathbb{Q}}(104)$:	0	1	1	1	6	0	15	10	11	12	20	
$E_{\mathbb{Q}}(105)$:	1	0	0	0	5	12	14	9	10	11	19	
$E_{\mathbb{Q}}(106)$:	0	2	4	6	4	11	13	8	9	10	18	
$E_{\mathbb{Q}}(107)$:	1	1	3	5	3	10	12	7	8	9	17	
$E_{\mathbb{Q}}(108)$:	0	0	2	4	2	9	11	6	7	8	16	
$E_{\mathbb{Q}}(109)$:	1	2	1	3	1	8	10	5	6	7	15	
$E_{\mathbb{Q}}(110)$:	0	1	0	2	0	7	9	4	5	6	14	
$E_{\mathbb{Q}}(111)$:	1	0	4	1	10	6	8	3	4	5	13	
$E_{\mathbb{Q}}(112)$:	0	2	3	0	9	5	7	2	3	4	12	
$E_{\mathbb{Q}}(113)$:	1	1	2	6	8	4	6	1	2	3	11	
$E_{\mathbb{Q}}(114)$:	0	0	1	5	7	3	5	0	1	2	10	
$E_{\mathbb{Q}}(115)$:	1	2	0	4	6	2	4	18	0	1	9	
$E_{\mathbb{Q}}(116)$:	0	1	4	3	5	1	3	17	22	0	8	
$E_{\mathbb{Q}}(117)$:	1	0	3	2	4	0	2	16	21	28	7	
$E_{\mathbb{Q}}(118)$:	0	2	2	1	3	12	1	15	20	27	6	
$E_{\mathbb{Q}}(119)$:	1	1	1	0	2	11	0	14	19	26	5	
$E_{\mathbb{Q}}(120)$:	0	0	0	6	1	10	16	13	18	25	4	
$E_{\mathbb{Q}}(121)$:	1	2	4	5	0	9	15	12	17	24	3	
...												
$E_{\mathbb{Q}}(n)$:	q_1	q_2	q_3	q_4	q_5	q_6	q_7	q_8	q_9	q_{10}	q_{11}	...
...												
Prime:	p_1	p_2	p_3	p_4	p_5	p_6	p_7	p_8	p_9	p_{10}	p_{11}	$\dots p_n \dots$
Divisor:	2	3	5	7	11	13	17	19	23	29	31	$\dots p_n \dots$

Fig.5: The output of the prime-number based algorithms $E_{\mathbb{P}}$ and $E_{\mathbb{Q}}$

CHAPTER 42

Analysing non-heuristic estimates of primes $\leq n$ for $n \leq 1500$

Fig.1: The following table gives comparative values for $\pi(n)$ as approximated non-heuristically by $\pi_L(n) = \sum_{j=1}^{n} \prod_{i=1}^{\pi(\sqrt{j})}(1 - 1/p_j)$, the actual values $\pi(n)$ of the primes less than or equal to n, and the values for $\pi(n)$ as estimated non-heuristically by $\pi_H(n) = n.\prod_{i=1}^{\pi(\sqrt{n})}(1 - 1/p_j)$ of $\pi(n)$, for $4 \leq n \leq 1500$.

n	p_n	$\pi(\sqrt{n})$	$\prod_{j=1}^{n}(1-1/p_j)$	$[\sqrt{n}]$	$\prod_{j=1}^{\pi(\sqrt{n})}(1-1/p_j)$	$\sum_{j=1}^{n}\prod_{i=1}^{\pi(\sqrt{j})}(1-1/p_j)$	$\pi(n)$	$n.\prod_{i=1}^{\pi(\sqrt{n})}(1-1/p_j)$
1	2	0	1/2	1		0.0000	0	0.000
2	3	0	1/3	1	1/2	0.5000	1	1.0000
3	5	0	4/15	1	1/2	1.0000	2	1.5000
4	7	1	8/35	2	1/3	1.3333	2	1.3333
5	11	1	16/77	2	1/3	1.6667	3	1.6667
6	13	1	89/464	2	1/3	2.0000	3	2.0000
7	17	1	165/914	2	1/3	2.3333	4	2.3333
8	19	1	157/918	2	1/3	2.6667	4	2.6667
9	23	2	62/379	3	4/15	2.9333	4	2.4000
10	29	2	157/994	3	4/15	3.2000	4	2.6667
11	31	2	142/929	3	4/15	3.4667	5	2.9333
12	37	2	29/195	3	4/15	3.7333	5	3.2000
13	41	2	139/958	3	4/15	4.0000	6	3.4667
14	43	2	89/628	3	4/15	4.2667	6	3.7333
15	47	2	62/447	3	4/15	4.5333	6	4.0000
16	53	2	112/823	4	4/15	4.8000	6	4.2667
17	59	2	40/299	4	4/15	5.0667	7	4.5333
18	61	2	5/38	4	4/15	5.3333	7	4.8000
19	67	2	7/54	4	4/15	5.6000	8	5.0667
20	71	2	40/313	4	4/15	5.8667	8	5.3333
21	73	2	15/119	4	4/15	6.1333	8	5.6000
22	79	2	85/683	4	4/15	6.4000	8	5.8667
23	83	2	15/122	4	4/15	6.6667	9	6.1333
24	89	2	31/255	4	4/15	6.9333	9	6.4000
25	97	3	106/881	5	8/35	7.1619	9	5.7143
26	101	3	109/915	5	8/35	7.3905	9	5.9429
27	103	3	86/729	5	8/35	7.6190	9	6.1714
28	107	3	97/830	5	8/35	7.8476	9	6.4000
29	109	3	11/95	5	8/35	8.0762	10	6.6286
30	113	3	7/61	5	8/35	8.3048	10	6.8751
31	127	3	101/887	5	8/35	8.5333	11	7.0857
32	131	3	20/177	5	8/35	8.7619	11	7.3143
33	137	3	47/419	5	8/35	8.9905	11	7.5429
34	139	3	49/440	5	8/35	9.2190	11	7.7714
35	149	3	25/226	5	8/35	9.4476	11	8.0000
36	151	3	10/91	6	8/35	9.6762	11	8.2286
37	157	3	63/577	6	8/35	9.9048	12	8.4571
38	163	3	79/728	6	8/35	10.1333	12	8.6857
39	167	3	48/445	6	8/35	10.3619	12	8.9143
40	173	3	77/718	6	8/35	10.5905	12	9.1429
41	179	3	61/572	6	8/35	10.8190	13	9.3714
42	181	3	7/66	6	8/35	11.0476	13	9.6000
43	191	3	94/891	6	8/35	11.2762	14	9.8286
44	193	3	89/848	6	8/35	11.5048	14	10.0571

n	p_n	$\pi(\sqrt{n})$	$\prod_{j=1}^{n}(1-1/p_j)$	$[\sqrt{n}]$	$\prod_{j=1}^{\pi(\sqrt{n})}(1-1/p_j)$	$\sum_{j=1}^{n}\prod_{i=1}^{\pi(\sqrt{j})}(1-1/p_j)$	$\pi(n)$	$n.\prod_{i=1}^{\pi(\sqrt{n})}(1-1/p_j)$
45	197	3	26/249	6	8/35	11.7333	14	10.2857
46	199	3	8/77	6	8/35	11.9619	14	10.5143
47	211	3	76/735	6	8/35	12.1905	15	10.7429
48	223	3	7/68	6	8/35	12.4190	15	10.9714
49	227	4	33/322	7	16/77	12.6268	15	10.1818
50	229	4	5/49	7	16/77	12.8346	15	10.3896
51	233	4	19/187	7	16/77	13.0424	15	10.5974
52	239	4	43/425	7	16/77	13.2502	15	10.8052
53	241	4	40/397	7	16/77	13.4580	16	11.0130
54	251	4	57/568	7	16/77	13.6658	16	11.2208
55	257	4	1/10	7	16/77	13.8736	16	11.4286
56	263	4	24/241	7	16/77	14.0814	16	11.6364
57	269	4	63/635	7	16/77	14.2892	16	11.8442
58	271	4	60/607	7	16/77	14.4970	16	12.0519
59	277	4	13/132	7	16/77	14.7048	17	12.2597
60	281	4	58/591	7	16/77	14.9126	17	12.4675
61	283	4	31/317	7	16/77	15.1203	18	12.6753
62	293	4	23/236	7	16/77	15.3281	18	12.8831
63	307	4	17/175	7	16/77	15.5359	18	13.0909
64	311	4	58/599	8	16/77	15.7437	18	13.2987
65	313	4	61/632	8	16/77	15.9515	18	13.5065
66	317	4	61/634	8	16/77	16.1593	18	13.7143
67	331	4	40/417	8	16/77	16.3671	19	13.9221
68	337	4	68/711	8	16/77	16.5749	19	14.1299
69	347	4	72/755	8	16/77	16.7827	19	14.3377
70	349	4	31/326	8	16/77	16.9905	19	14.5455
71	353	4	11/116	8	16/77	17.1983	20	14.7532
72	359	4	33/349	8	16/77	17.4061	20	14.9610
73	367	4	43/456	8	16/77	17.6139	21	15.1688
74	373	4	79/840	8	16/77	17.8216	21	15.3766
75	379	4	59/629	8	16/77	18.0294	21	15.5844
76	383	4	45/481	8	16/77	18.2372	21	15.7922
77	389	4	60/643	8	16/77	18.4450	21	16.0000
78	397	4	39/419	8	16/77	18.6528	21	16.2078
79	401	4	61/657	8	16/77	18.8606	22	16.4156
80	409	4	64/691	8	16/77	19.0684	22	16.6234
81	419	4	79/855	9	16/77	19.2762	22	16.8312
82	421	4	33/358	9	16/77	19.4840	22	17.0390
83	431	4	8/87	9	16/77	19.6918	23	17.2468
84	433	4	89/970	9	16/77	19.8996	23	17.4545
85	439	4	13/142	9	16/77	20.1074	23	17.6623
86	443	4	39/427	9	16/77	20.3152	23	17.8701
87	449	4	37/406	9	16/77	20.5229	23	18.0779
88	457	4	1/11	9	16/77	20.7307	23	18.2857
89	461	4	48/529	9	16/77	20.9385	24	18.4935
90	463	4	67/740	9	16/77	21.1463	24	18.7013
91	467	4	73/808	9	16/77	21.3541	24	18.9091
92	479	4	11/122	9	16/77	21.5619	24	19.1169
93	487	4	35/389	9	16/77	21.7697	24	19.3247
94	491	4	51/568	9	16/77	21.9775	24	19.5325
95	499	4	69/770	9	16/77	22.1853	24	19.7403
96	503	4	11/123	9	16/77	22.3931	24	19.9481
97	509	4	54/605	9	16/77	22.6009	25	20.1558
98	521	4	71/797	9	16/77	22.8087	25	20.3636
99	523	4	77/866	9	16/77	23.0165	25	20.5714
100	541	4	71/800	10	16/77	23.2242	25	20.7792
101	547	4	59/666	10	16/77	23.4320	26	20.9870
102	557	4	81/916	10	16/77	23.6398	26	21.1948
103	563	4	73/827	10	16/77	23.8476	27	21.4026
104	569	4	43/488	10	16/77	24.0554	27	21.6104
105	571	4	19/216	10	16/77	24.2632	27	21.8182
106	577	4	85/968	10	16/77	24.4710	27	22.0260
107	587	4	27/308	10	16/77	24.6788	28	22.2338
108	593	4	7/80	10	16/77	24.8866	28	22.4416
109	599	4	65/744	10	16/77	25.0944	29	22.6494
110	601	4	43/493	10	16/77	25.3022	29	22.8571
111	607	4	31/356	10	16/77	25.5100	29	23.0649
112	613	4	2/23	10	16/77	25.7177	29	23.2727
113	617	4	23/265	10	16/77	25.9255	30	23.4805
114	619	4	50/577	10	16/77	26.1333	30	23.6883
115	631	4	77/890	10	16/77	26.3411	30	23.8961
116	641	4	85/984	10	16/77	26.5489	30	24.1039
117	643	4	37/429	10	16/77	26.7567	30	24.3117
118	647	4	80/929	10	16/77	26.9645	30	24.5195
119	653	4	46/535	10	16/77	27.1723	30	24.7273
120	659	4	54/629	10	16/77	27.3801	30	24.9351
121	661	5	3/35	11	89/464	27.5719	30	23.2088
122	673	5	41/479	11	89/464	27.7637	30	23.4006

n	p_n	$\pi(\sqrt{n})$	$\prod_{j=1}^{n}(1-1/p_j)$	$[\sqrt{n}]$	$\prod_{j=1}^{\pi(\sqrt{n})}(1-1/p_j)$	$\sum_{j=1}^{n}\prod_{i=1}^{\pi(\sqrt{j})}(1-1/p_j)$	$\pi(n)$	$n.\prod_{i=1}^{\pi(\sqrt{n})}(1-1/p_j)$
123	677	5	10/117	11	89/464	27.9555	30	23.5924
124	683	5	46/539	11	89/464	28.1473	30	23.7842
125	691	5	64/751	11	89/464	28.3391	30	23.9760
126	701	5	4/47	11	89/464	28.5309	30	24.1678
127	709	5	56/659	11	89/464	28.7227	31	24.3596
128	719	5	51/601	11	89/464	28.9146	31	24.5514
129	727	5	5/59	11	89/464	29.1064	31	24.7433
130	733	5	60/709	11	89/464	29.2982	31	24.9351
131	739	5	6/71	11	89/464	29.4900	32	25.1269
132	743	5	33/391	11	89/464	29.6818	32	25.3187
133	751	5	59/700	11	89/464	29.8736	32	25.5105
134	757	5	25/297	11	89/464	30.0654	32	25.7023
135	761	5	67/797	11	89/464	30.2572	32	25.8941
136	769	5	45/536	11	89/464	30.4490	32	26.0859
137	773	5	68/811	11	89/464	30.6408	33	26.2777
138	787	5	17/203	11	89/464	30.8326	33	26.4695
139	797	5	23/275	11	89/464	31.0244	34	26.6613
140	809	5	35/419	11	89/464	31.2163	34	26.8531
141	811	5	73/875	11	89/464	31.4081	34	27.0450
142	821	5	1/12	11	89/464	31.5999	34	27.2368
143	823	5	64/769	11	89/464	31.7917	34	27.4286
144	827	5	33/397	12	89/464	31.9835	34	27.6204
145	829	5	45/542	12	89/464	32.1753	34	27.8122
146	839	5	17/205	12	89/464	32.3671	34	28.0040
147	853	5	41/495	12	89/464	32.5589	34	28.1958
148	857	5	23/278	12	89/464	32.7507	34	28.3876
149	859	5	79/956	12	89/464	32.9425	35	28.5794
150	863	5	26/315	12	89/464	33.1343	35	28.7712
151	877	5	31/376	12	89/464	33.3261	36	28.9630
152	881	5	7/85	12	89/464	33.5179	36	29.1548
153	883	5	51/620	12	89/464	33.7098	36	29.3467
154	887	5	47/572	12	89/464	33.9016	36	29.5385
155	907	5	49/597	12	89/464	34.0934	36	29.7303
156	911	5	71/866	12	89/464	34.2852	36	29.9221
157	919	5	19/232	12	89/464	34.4770	37	30.1139
158	929	5	76/929	12	89/464	34.6688	37	30.3057
159	937	5	38/465	12	89/464	34.8606	37	30.4975
160	941	5	4/49	12	89/464	35.0524	37	30.6893
161	947	5	19/233	12	89/464	35.2442	37	30.8811
162	953	5	29/356	12	89/464	35.4360	37	31.0729
163	967	5	52/639	12	89/464	35.6278	38	31.2647
164	971	5	10/123	12	89/464	35.8196	38	31.4565
165	977	5	51/628	12	89/464	36.0115	38	31.6484
166	983	5	46/567	12	89/464	36.2033	38	31.8402
167	991	5	62/765	12	89/464	36.3951	39	32.0320
168	997	5	57/704	12	89/464	36.5869	39	32.2238
169	1009	6	11/136	13	165/914	36.7674	39	30.5088
170	1013	6	8/99	13	165/914	36.9479	39	30.6893
171	1019	6	49/607	13	165/914	37.1285	39	30.8698
172	1021	6	5/62	13	165/914	37.3090	39	31.0504
173	1031	6	17/211	13	165/914	37.4895	40	31.2309
174	1033	6	59/733	13	165/914	37.6700	40	31.4114
175	1039	6	39/485	13	165/914	37.8506	40	31.5919
176	1049	6	67/834	13	165/914	38.0311	40	31.7725
177	1051	6	37/461	13	165/914	38.2116	40	31.9530
178	1061	6	17/212	13	165/914	38.3921	40	32.1335
179	1063	6	29/362	13	165/914	38.5727	41	32.3140
180	1069	6	2/25	13	165/914	38.7532	41	32.4946
181	1087	6	79/988	13	165/914	38.9337	42	32.6751
182	1091	6	56/701	13	165/914	39.1142	42	32.8556
183	1093	6	17/213	13	165/914	39.2948	42	33.0361
184	1097	6	37/464	13	165/914	39.4753	42	33.2167
185	1103	6	29/364	13	165/914	39.6558	42	33.3972
186	1109	6	71/892	13	165/914	39.8363	42	33.5777
187	1117	6	47/591	13	165/914	40.0169	42	33.7582
188	1123	6	70/881	13	165/914	40.1974	42	33.9388
189	1129	6	62/781	13	165/914	40.3779	42	34.1193
190	1151	6	51/643	13	165/914	40.5584	42	34.2998
191	1153	6	21/265	13	165/914	40.7390	43	34.4803
192	1163	6	27/341	13	165/914	40.9195	43	34.6609
193	1171	6	25/316	13	165/914	41.1000	44	34.8414
194	1181	6	43/544	13	165/914	41.2805	44	35.0219
195	1187	6	68/861	13	165/914	41.4611	44	35.2024
196	1193	6	58/735	14	165/914	41.6416	44	35.3830
197	1201	6	41/520	14	165/914	41.8221	45	35.5635
198	1213	6	62/787	14	165/914	42.0026	45	35.7440
199	1217	6	27/343	14	165/914	42.1832	46	35.9245
200	1223	6	7/89	14	165/914	42.3637	46	36.1051

n	p_n	$\pi(\sqrt{n})$	$\prod_{j=1}^{n}(1-1/p_j)$	$[\sqrt{n}]$	$\prod_{j=1}^{\pi(\sqrt{n})}(1-1/p_j)$	$\sum_{j=1}^{n}\prod_{i=1}^{\pi(\sqrt{j})}(1-1/p_j)$	$\pi(n)$	$n.\prod_{i=1}^{\pi(\sqrt{n})}(1-1/p_j)$
201	1229	6	69/878	14	165/914	42.5442	46	36.2856
202	1231	6	34/433	14	165/914	42.7247	46	36.4661
203	1237	6	55/701	14	165/914	42.9053	46	36.6466
204	1249	6	45/574	14	165/914	43.0858	46	36.8272
205	1259	6	64/817	14	165/914	43.2663	46	37.0077
206	1277	6	49/626	14	165/914	43.4468	46	37.1882
207	1279	6	14/179	14	165/914	43.6274	46	37.3687
208	1283	6	44/563	14	165/914	43.8079	46	37.5493
209	1289	6	36/461	14	165/914	43.9884	46	37.7298
210	1291	6	65/833	14	165/914	44.1689	46	37.9103
211	1297	6	63/808	14	165/914	44.3495	47	38.0909
212	1301	6	6/77	14	165/914	44.5300	47	38.2714
213	1303	6	71/912	14	165/914	44.7105	47	38.4519
214	1307	6	62/797	14	165/914	44.8910	47	38.6324
215	1319	6	37/476	14	165/914	45.0716	47	38.8130
216	1321	6	8/103	14	165/914	45.2521	47	38.9935
217	1327	6	69/889	14	165/914	45.4326	47	39.1740
218	1361	6	47/606	14	165/914	45.6131	47	39.3545
219	1367	6	31/400	14	165/914	45.7937	47	39.5351
220	1373	6	57/736	14	165/914	45.9742	47	39.7156
221	1381	6	64/827	14	165/914	46.1547	47	39.8961
222	1399	6	29/375	14	165/914	46.3352	47	40.0766
223	1409	6	67/867	14	165/914	46.5158	48	40.2572
224	1423	6	59/764	14	165/914	46.6963	48	40.4377
225	1427	6	24/311	15	165/914	46.8768	48	40.6182
226	1429	6	31/402	15	165/914	47.0574	48	40.7987
227	1433	6	43/558	15	165/914	47.2379	49	40.9793
228	1439	6	69/896	15	165/914	47.4184	49	41.1598
229	1447	6	1/13	15	165/914	47.5989	50	41.3403
230	1451	6	1/13	15	165/914	47.7795	50	41.5208
231	1453	6	1/13	15	165/914	47.9600	50	41.7014
232	1459	6	47/612	15	165/914	48.1405	50	41.8819
233	1471	6	33/430	15	165/914	48.3210	51	42.0624
234	1481	6	51/665	15	165/914	48.5016	51	42.2429
235	1483	6	21/274	15	165/914	48.6821	51	42.4235
236	1487	6	53/692	15	165/914	48.8626	51	42.6040
237	1489	6	61/797	15	165/914	49.0431	51	42.7845
238	1493	6	27/353	15	165/914	49.2237	51	42.9650
239	1499	6	12/157	15	165/914	49.4042	52	43.1456
240	1511	6	11/144	15	165/914	49.5847	52	43.3261
241	1523	6	10/131	15	165/914	49.7652	53	43.5066
242	1531	6	46/603	15	165/914	49.9458	53	43.6871
243	1543	6	17/223	15	165/914	50.1263	53	43.8677
244	1549	6	8/105	15	165/914	50.3068	53	44.0482
245	1553	6	67/880	15	165/914	50.4873	53	44.2287
246	1559	6	7/92	15	165/914	50.6679	53	44.4092
247	1567	6	53/697	15	165/914	50.8484	53	44.5898
248	1571	6	69/908	15	165/914	51.0289	53	44.7703
249	1579	6	6/79	15	165/914	51.2094	53	44.9508
250	1583	6	17/224	15	165/914	51.3900	53	45.1313
251	1597	6	38/501	15	165/914	51.5705	54	45.3119
252	1601	6	26/343	15	165/914	51.7510	54	45.4924
253	1607	6	5/66	15	165/914	51.9315	54	45.6729
254	1609	6	67/885	15	165/914	52.1121	54	45.8534
255	1613	6	23/304	15	165/914	52.2926	54	46.0340
256	1619	6	71/939	16	165/914	52.4731	54	46.2145
257	1621	6	30/397	16	165/914	52.6536	55	46.3950
258	1627	6	29/384	16	165/914	52.8342	55	46.5755
259	1637	6	4/53	16	165/914	53.0147	55	46.7561
260	1657	6	66/875	16	165/914	53.1952	55	46.9366
261	1663	6	64/849	16	165/914	53.3757	55	47.1171
262	1667	6	11/146	16	165/914	53.5563	55	47.2976
263	1669	6	71/943	16	165/914	53.7368	56	47.4782
264	1693	6	38/505	16	165/914	53.9173	56	47.6587
265	1697	6	37/492	16	165/914	54.0978	56	47.8392
266	1699	6	59/785	16	165/914	54.2784	56	48.0197
267	1709	6	16/213	16	165/914	54.4589	56	48.2003
268	1721	6	53/706	16	165/914	54.6394	56	48.3808
269	1723	6	67/893	16	165/914	54.8199	57	48.5613
270	1733	6	3/40	16	165/914	55.0005	57	48.7418
271	1741	6	32/427	16	165/914	55.1810	58	48.9224
272	1747	6	37/494	16	165/914	55.3615	58	49.1029
273	1753	6	39/521	16	165/914	55.5420	58	49.2834
274	1759	6	30/401	16	165/914	55.7226	58	49.4639
275	1777	6	8/107	16	165/914	55.9031	58	49.6445
276	1783	6	55/736	16	165/914	56.0836	58	49.8250
277	1787	6	18/241	16	165/914	56.2641	59	50.0055
278	1789	6	58/777	16	165/914	56.4447	59	50.1860

n	p_n	$\pi(\sqrt{n})$	$\prod_{j=1}^{n}(1-1/p_j)$	$[\sqrt{n}]$	$\prod_{j=1}^{\pi(\sqrt{n})}(1-1/p_j)$	$\sum_{j=1}^{n}\prod_{i=1}^{\pi(\sqrt{j})}(1-1/p_j)$	$\pi(n)$	$n.\prod_{i=1}^{\pi(\sqrt{n})}(1-1/p_j)$
279	1801	6	47/630	16	165/914	56.6252	59	50.3666
280	1811	6	17/228	16	165/914	56.8057	59	50.5471
281	1823	6	74/993	16	165/914	56.9862	60	50.7276
282	1831	6	61/819	16	165/914	57.1668	60	50.9082
283	1847	6	30/403	16	165/914	57.3473	61	51.0887
284	1861	6	59/793	16	165/914	57.5278	61	51.2692
285	1867	6	67/901	16	165/914	57.7083	61	51.4497
286	1871	6	11/148	16	165/914	57.8889	61	51.6303
287	1873	6	13/175	16	165/914	58.0694	61	51.8108
288	1877	6	49/660	16	165/914	58.2499	61	51.9913
289	1879	7	21/283	17	157/918	58.4209	61	49.4259
290	1889	7	31/418	17	157/918	58.5920	61	49.5970
291	1901	7	55/742	17	157/918	58.7630	61	49.7680
292	1907	7	2/27	17	157/918	58.9340	61	49.9390
293	1913	7	2/27	17	157/918	59.1050	62	50.1100
294	1931	7	41/554	17	157/918	59.2761	62	50.2811
295	1933	7	27/365	17	157/918	59.4471	62	50.4521
296	1949	7	19/257	17	157/918	59.6181	62	50.6231
297	1951	7	15/203	17	157/918	59.7891	62	50.7941
298	1973	7	63/853	17	157/918	59.9602	62	50.9652
299	1979	7	11/149	17	157/918	60.1312	62	51.1362
300	1987	7	47/637	17	157/918	60.3022	62	51.3072
301	1993	7	25/339	17	157/918	60.4732	62	51.4782
302	1997	7	30/407	17	157/918	60.6443	62	51.6493
303	1999	7	61/828	17	157/918	60.8153	62	51.8203
304	2003	7	31/421	17	157/918	60.9863	62	51.9913
305	2011	7	63/856	17	157/918	61.1573	62	52.1623
306	2017	7	32/435	17	157/918	61.3284	62	52.3334
307	2027	7	5/68	17	157/918	61.4994	63	52.5044
308	2029	7	28/381	17	157/918	61.6704	63	52.6754
309	2039	7	57/776	17	157/918	61.8414	63	52.8464
310	2053	7	29/395	17	157/918	62.0125	63	53.0174
311	2063	7	51/695	17	157/918	62.1835	64	53.1885
312	2069	7	41/559	17	157/918	62.3545	64	53.3595
313	2081	7	64/873	17	157/918	62.5255	65	53.5305
314	2083	7	17/232	17	157/918	62.6965	65	53.7015
315	2087	7	26/355	17	157/918	62.8676	65	53.8726
316	2089	7	50/683	17	157/918	63.0386	65	54.0436
317	2099	7	3/41	17	157/918	63.2096	66	54.2146
318	2111	7	52/711	17	157/918	63.3806	66	54.3856
319	2113	7	25/342	17	157/918	63.5517	66	54.5567
320	2129	7	35/479	17	157/918	63.7227	66	54.7277
321	2131	7	13/178	17	157/918	63.8937	66	54.8987
322	2137	7	10/137	17	157/918	64.0647	66	55.0697
323	2141	7	17/233	17	157/918	64.2358	66	55.2408
324	2143	7	52/713	18	157/918	64.4068	66	55.4118
325	2153	7	39/535	18	157/918	64.5778	66	55.5828
326	2161	7	29/398	18	157/918	64.7488	66	55.7538
327	2179	7	26/357	18	157/918	64.9199	66	55.9249
328	2203	7	19/261	18	157/918	65.0909	66	56.0959
329	2207	7	35/481	18	157/918	65.2619	66	56.2669
330	2213	7	4/55	18	157/918	65.4329	66	56.4379
331	2221	7	45/619	18	157/918	65.6040	67	56.6090
332	2237	7	21/289	18	157/918	65.7750	67	56.7800
333	2239	7	56/771	18	157/918	65.9460	67	56.9510
334	2243	7	31/427	18	157/918	66.1170	67	57.1220
335	2251	7	50/689	18	157/918	66.2881	67	57.2930
336	2267	7	14/193	18	157/918	66.4591	67	57.4641
337	2269	7	53/731	18	157/918	66.6301	68	57.6351
338	2273	7	5/69	18	157/918	66.8011	68	57.8061
339	2281	7	46/635	18	157/918	66.9721	68	57.9771
340	2287	7	58/801	18	157/918	67.1432	68	58.1482
341	2293	7	49/677	18	157/918	67.3142	68	58.3192
342	2297	7	62/857	18	157/918	67.4852	68	58.4902
343	2309	7	35/484	18	157/918	67.6562	68	58.6612
344	2311	7	6/83	18	157/918	67.8273	68	58.8323
345	2333	7	25/346	18	157/918	67.9983	68	59.0033
346	2339	7	13/180	18	157/918	68.1693	68	59.1743
347	2341	7	27/374	18	157/918	68.3403	69	59.3453
348	2347	7	7/97	18	157/918	68.5114	69	59.5164
349	2351	7	22/305	18	157/918	68.6824	70	59.6874
350	2357	7	23/319	18	157/918	68.8534	70	59.8584
351	2371	7	8/111	18	157/918	69.0244	70	60.0294
352	2377	7	17/236	18	157/918	69.1955	70	60.2005
353	2381	7	71/986	18	157/918	69.3665	71	60.3715
354	2383	7	28/389	18	157/918	69.5375	71	60.5425
355	2389	7	10/139	18	157/918	69.7085	71	60.7135
356	2393	7	21/292	18	157/918	69.8796	71	60.8846

n	p_n	$\pi(\sqrt{n})$	$\prod_{j=1}^{n}(1-1/p_j)$	$[\sqrt{n}]$	$\prod_{j=1}^{\pi(\sqrt{n})}(1-1/p_j)$	$\sum_{j=1}^{n}\prod_{i=1}^{\pi(\sqrt{j})}(1-1/p_j)$	$\pi(n)$	$n.\prod_{i=1}^{\pi(\sqrt{n})}(1-1/p_j)$
357	2399	7	56/779	18	157/918	70.0506	71	61.0556
358	2411	7	12/167	18	157/918	70.2216	71	61.2266
359	2417	7	13/181	18	157/918	70.3926	72	61.3976
360	2423	7	14/195	18	157/918	70.5637	72	61.5686
361	2437	8	15/209	19	62/379	70.7272	72	59.0553
362	2441	8	33/460	19	62/379	70.8908	72	59.2189
363	2447	8	18/251	19	62/379	71.0544	72	59.3825
364	2459	8	61/851	19	62/379	71.2180	72	59.5461
365	2467	8	23/321	19	62/379	71.3816	72	59.7097
366	2473	8	53/740	19	62/379	71.5452	72	59.8733
367	2477	8	31/433	19	62/379	71.7088	73	60.0369
368	2503	8	37/517	19	62/379	71.8724	73	60.2005
369	2521	8	47/657	19	62/379	72.0359	73	60.3640
370	2531	8	64/895	19	62/379	72.1995	73	60.5276
371	2539	8	1/14	19	62/379	72.3631	73	60.6912
372	2543	8	1/14	19	62/379	72.5267	73	60.8548
373	2549	8	1/14	19	62/379	72.6903	74	61.0184
374	2551	8	1/14	19	62/379	72.8539	74	61.1820
375	2557	8	1/14	19	62/379	73.0175	74	61.3456
376	2579	8	58/813	19	62/379	73.1811	74	61.5092
377	2591	8	44/617	19	62/379	73.3447	74	61.6727
378	2593	8	71/996	19	62/379	73.5082	74	61.8363
379	2609	8	30/421	19	62/379	73.6718	75	61.9999
380	2617	8	26/365	19	62/379	73.8354	75	62.1635
381	2621	8	68/955	19	62/379	73.9990	75	62.3271
382	2633	8	20/281	19	62/379	74.1626	75	62.4907
383	2647	8	18/253	19	62/379	74.3262	76	62.6543
384	2657	8	50/703	19	62/379	74.4898	76	62.8179
385	2659	8	61/858	19	62/379	74.6534	76	62.9815
386	2663	8	14/197	19	62/379	74.8169	76	63.1450
387	2671	8	13/183	19	62/379	74.9805	76	63.3086
388	2677	8	37/521	19	62/379	75.1441	76	63.4722
389	2683	8	23/324	19	62/379	75.3077	77	63.6358
390	2687	8	11/155	19	62/379	75.4713	77	63.7994
391	2689	8	31/437	19	62/379	75.6349	77	63.9630
392	2693	8	49/691	19	62/379	75.7985	77	64.1266
393	2699	8	28/395	19	62/379	75.9621	77	64.2902
394	2707	8	9/127	19	62/379	76.1257	77	64.4537
395	2711	8	17/240	19	62/379	76.2892	77	64.6173
396	2713	8	65/918	19	62/379	76.4528	77	65.7809
397	2719	8	39/551	19	62/379	76.6164	78	64.9445
398	2729	8	15/212	19	62/379	76.7800	78	65.1081
399	2731	8	36/509	19	62/379	76.9436	78	65.2717
400	2741	8	7/99	20	62/379	77.1072	78	65.4353
401	2749	8	47/665	20	62/379	77.2708	79	65.5989
402	2753	8	13/184	20	62/379	77.4344	79	65.7625
403	2767	8	44/623	20	62/379	77.5979	79	65.9260
404	2777	8	67/949	20	62/379	77.7615	79	66.0896
405	2789	8	59/836	20	62/379	77.9251	79	66.2532
406	2791	8	63/893	20	62/379	78.0887	79	66.4168
407	2797	8	39/553	20	62/379	78.2523	79	66.5804
408	2801	8	65/922	20	62/379	78.4159	79	66.7440
409	2803	8	58/823	20	62/379	78.5795	80	66.9076
410	2819	8	36/511	20	62/379	78.7431	80	67.0712
411	2833	8	5/71	20	62/379	78.9067	80	67.2347
412	2837	8	44/625	20	62/379	79.0702	80	67.3983
413	2843	8	62/881	20	62/379	79.2338	80	67.5619
414	2851	8	14/199	20	62/379	79.3974	80	67.7255
415	2857	8	41/583	20	62/379	79.5610	80	67.8891
416	2861	8	49/697	20	62/379	79.7246	80	68.0527
417	2879	8	61/868	20	62/379	79.8882	80	68.2163
418	2887	8	64/911	20	62/379	80.0518	80	68.3799
419	2897	8	46/655	20	62/379	80.2154	81	68.5435
420	2903	8	45/641	20	62/379	80.3789	81	68.7070
421	2909	8	4/57	20	62/379	80.5425	82	68.8706
422	2917	8	59/841	20	62/379	80.7061	82	69.0342
423	2927	8	27/385	20	62/379	80.8697	82	69.1978
424	2939	8	19/271	20	62/379	81.0333	82	69.3614
425	2953	8	67/956	20	62/379	81.1969	82	69.5250
426	2957	8	11/157	20	62/379	81.3605	82	69.6886
427	2963	8	18/257	20	62/379	81.5241	82	69.8522
428	2969	8	53/757	20	62/379	81.6876	82	70.0157
429	2971	8	66/943	20	62/379	81.8512	82	70.1793
430	2999	8	41/586	20	62/379	82.0148	82	70.3429
431	3001	8	37/529	20	62/379	82.1784	83	70.5065
432	3011	8	43/615	20	62/379	82.3420	83	70.6701
433	3019	8	13/186	20	62/379	82.5056	84	70.8337
434	3023	8	16/229	20	62/379	82.6692	84	70.9973

n	p_n	$\pi(\sqrt{n})$	$\prod_{j=1}^{n}(1-1/p_j)$	$[\sqrt{n}]$	$\prod_{j=1}^{\pi(\sqrt{n})}(1-1/p_j)$	$\sum_{j=1}^{n}\prod_{i=1}^{\pi(\sqrt{j})}(1-1/p_j)$	$\pi(n)$	$n.\prod_{i=1}^{\pi(\sqrt{n})}(1-1/p_j)$
435	3037	8	19/272	20	62/379	82.8328	84	71.1609
436	3041	8	28/401	20	62/379	82.9964	84	71.3245
437	3049	8	43/616	20	62/379	83.1599	84	71.4880
438	3061	8	3/43	20	62/379	83.3235	84	71.6516
439	3067	8	3/43	20	62/379	83.4871	85	71.8152
440	3079	8	50/717	20	62/379	83.6507	85	71.9788
441	3083	8	29/416	21	62/379	83.8143	85	72.1424
442	3089	8	63/904	21	62/379	83.9779	85	72.3060
443	3109	8	65/933	21	62/379	84.1415	86	72.4696
444	3119	8	53/761	21	62/379	84.3051	86	72.6332
445	3121	8	11/158	21	62/379	84.4686	86	72.7967
446	3137	8	68/977	21	62/379	84.6322	86	72.9603
447	3163	8	43/618	21	62/379	84.7958	86	73.1239
448	3167	8	8/115	21	62/379	84.9594	86	73.2875
449	3169	8	21/302	21	62/379	85.1230	87	73.4511
450	3181	8	13/187	21	62/379	85.2866	87	73.6147
451	3187	8	41/590	21	62/379	85.4502	87	73.7783
452	3191	8	38/547	21	62/379	85.6138	87	73.9419
453	3203	8	5/72	21	62/379	85.7774	87	74.1055
454	3209	8	52/749	21	62/379	85.9409	87	74.2690
455	3217	8	49/706	21	62/379	86.1045	87	74.4326
456	3221	8	63/908	21	62/379	86.2681	87	74.5962
457	3229	8	12/173	21	62/379	86.4317	88	74.7598
458	3251	8	19/274	21	62/379	86.5953	88	74.9234
459	3253	8	54/779	21	62/379	86.7589	88	75.0870
460	3257	8	7/101	21	62/379	86.9225	88	75.2506
461	3259	8	23/332	21	62/379	87.0861	89	75.4142
462	3271	8	41/592	21	62/379	87.2496	89	75.5777
463	3299	8	9/130	21	62/379	87.4132	90	75.7413
464	3301	8	29/419	21	62/379	87.5768	90	75.9049
465	3307	8	42/607	21	62/379	87.7404	90	76.0685
466	3313	8	46/665	21	62/379	87.9040	90	76.2321
467	3319	8	13/188	21	62/379	88.0676	91	76.3957
468	3323	8	58/839	21	62/379	88.2312	91	76.5593
469	3329	8	66/955	21	62/379	88.3948	91	76.7229
470	3331	8	19/275	21	62/379	88.5584	91	76.8865
471	3343	8	23/333	21	62/379	88.7219	91	77.0500
472	3347	8	29/420	21	62/379	88.8855	91	77.2136
473	3359	8	39/565	21	62/379	89.0491	91	77.3772
474	3361	8	59/855	21	62/379	89.2127	91	77.5408
475	3371	8	2/29	21	62/379	89.3763	91	77.7044
476	3373	8	2/29	21	62/379	89.5399	91	77.8680
477	3389	8	2/29	21	62/379	89.7035	91	78.0316
478	3391	8	59/856	21	62/379	89.8671	91	78.1952
479	3407	8	39/566	21	62/379	90.0306	92	78.3587
480	3413	8	29/421	21	62/379	90.1942	92	78.5223
481	3433	8	23/334	21	62/379	90.3578	92	78.6859
482	3449	8	59/857	21	62/379	90.5214	92	78.8495
483	3457	8	17/247	21	62/379	90.6850	92	79.0131
484	3461	8	15/218	22	62/379	90.8486	92	79.1767
485	3463	8	13/189	22	62/379	91.0122	92	79.3403
486	3467	8	59/858	22	62/379	91.1758	92	79.5039
487	3469	8	11/160	22	62/379	91.3394	93	79.6675
488	3491	8	20/291	22	62/379	91.5029	93	79.8310
489	3499	8	9/131	22	62/379	91.6665	93	79.9946
490	3511	8	34/495	22	62/379	91.8301	93	80.1582
491	3517	8	16/233	22	62/379	91.9937	94	80.3218
492	3527	8	67/976	22	62/379	92.1573	94	80.4854
493	3529	8	7/102	22	62/379	92.3209	94	80.6490
494	3533	8	33/481	22	62/379	92.4845	94	80.8126
495	3539	8	19/277	22	62/379	92.6481	94	80.9762
496	3541	8	12/175	22	62/379	92.8116	94	81.1397
497	3547	8	17/248	22	62/379	92.9752	94	81.3033
498	3557	8	49/715	22	62/379	93.1388	94	81.4669
499	3559	8	52/759	22	62/379	93.3024	95	81.6305
500	3571	8	5/73	22	62/379	93.4660	95	81.7941
501	3581	8	48/701	22	62/379	93.6296	95	81.9577
502	3583	8	23/336	22	62/379	93.7932	95	82.1213
503	3593	8	49/716	22	62/379	93.9568	96	82.2849
504	3607	8	13/190	22	62/379	94.1204	96	82.4485
505	3613	8	29/424	22	62/379	94.2839	96	82.6120
506	3617	8	8/117	22	62/379	94.4475	96	82.7756
507	3623	8	35/512	22	62/379	94.6111	96	82.9392
508	3631	8	49/717	22	62/379	94.7747	96	83.1028
509	3637	8	11/161	22	62/379	94.9383	97	83.2664
510	3643	8	64/937	22	62/379	95.1019	97	83.4300
511	3659	8	45/659	22	62/379	95.2655	97	83.5936
512	3671	8	37/542	22	62/379	95.4291	97	83.7572

n	p_n	$\pi(\sqrt{n})$	$\prod_{j=1}^{n}(1-1/p_j)$	$[\sqrt{n}]$	$\prod_{j=1}^{\pi(\sqrt{n})}(1-1/p_j)$	$\sum_{j=1}^{n}\prod_{i=1}^{\pi(\sqrt{j})}(1-1/p_j)$	$\pi(n)$	$n.\prod_{i=1}^{\pi(\sqrt{n})}(1-1/p_j)$
513	3673	8	23/337	22	62/379	95.5926	97	83.9207
514	3677	8	67/982	22	62/379	95.7562	97	84.0843
515	3691	8	56/821	22	62/379	95.9198	97	84.2479
516	3697	8	3/44	22	62/379	96.0834	97	84.4115
517	3701	8	3/44	22	62/379	96.2470	97	84.5751
518	3709	8	58/851	22	62/379	96.4106	97	84.7387
519	3719	8	34/499	22	62/379	96.5742	97	84.9023
520	3727	8	25/367	22	62/379	96.7378	97	85.0659
521	3733	8	19/279	22	62/379	96.9014	98	85.2294
522	3739	8	16/235	22	62/379	97.0649	98	85.3930
523	3761	8	13/191	22	62/379	97.2285	99	85.5566
524	3767	8	23/338	22	62/379	97.3921	99	85.7202
525	3769	8	10/147	22	62/379	97.5557	99	85.8838
526	3779	8	27/397	22	62/379	97.7193	99	86.0474
527	3793	8	65/956	22	62/379	97.8829	99	86.2110
528	3797	8	52/765	22	62/379	98.0465	99	86.3746
529	3803	9	7/103	23	157/994	98.2044	99	83.5541
530	3821	9	57/839	23	157/994	98.3624	99	83.7120
531	3823	9	65/957	23	157/994	98.5203	99	83.8700
532	3833	9	11/162	23	157/994	98.6783	99	84.0279
533	3847	9	26/383	23	157/994	98.8362	99	84.1859
534	3851	9	49/722	23	157/994	98.9942	99	84.3438
535	3853	9	65/958	23	157/994	99.1521	99	84.5018
536	3863	9	66/973	23	157/994	99.3100	99	84.6597
537	3877	9	63/929	23	157/994	99.4680	99	84.8177
538	3881	9	4/59	23	157/994	99.6259	99	84.9756
539	3889	9	65/959	23	157/994	99.7839	99	85.1336
540	3907	9	33/487	23	157/994	99.9418	99	85.2915
541	3911	9	67/989	23	157/994	100.0998	100	85.4494
542	3917	9	17/251	23	157/994	100.2577	100	85.6074
543	3919	9	13/192	23	157/994	100.4157	100	85.7653
544	3923	9	22/325	23	157/994	100.5736	100	85.9233
545	3929	9	67/990	23	157/994	100.7316	100	86.0812
546	3931	9	50/739	23	157/994	100.8895	100	86.2392
547	3943	9	37/547	23	157/994	101.0475	101	86.3971
548	3947	9	33/488	23	157/994	101.2054	101	86.5551
549	3967	9	24/355	23	157/994	101.3634	101	86.7130
550	3989	9	39/577	23	157/994	101.5213	101	86.8710
551	4001	9	5/74	23	157/994	101.6793	101	87.0289
552	4003	9	5/74	23	157/994	101.8372	101	87.1869
553	4007	9	67/992	23	157/994	101.9951	101	87.3448
554	4013	9	21/311	23	157/994	102.1531	101	87.5028
555	4019	9	59/874	23	157/994	102.3110	101	87.6607
556	4021	9	11/163	23	157/994	102.4690	101	87.8187
557	4027	9	67/993	23	157/994	102.6269	102	87.9766
558	4049	9	57/845	23	157/994	102.7849	102	88.1346
559	4051	9	64/949	23	157/994	102.9428	102	88.2925
560	4057	9	6/89	23	157/994	103.1008	102	88.4504
561	4073	9	6/89	23	157/994	103.2587	102	88.6084
562	4079	9	31/460	23	157/994	103.4167	102	88.7663
563	4091	9	19/282	23	157/994	103.5746	103	88.9243
564	4093	9	13/193	23	157/994	103.7326	103	89.0822
565	4099	9	20/297	23	157/994	103.8905	103	89.2402
566	4111	9	41/609	23	157/994	104.0485	103	89.3981
567	4127	9	7/104	23	157/994	104.2064	103	89.5561
568	4129	9	36/535	23	157/994	104.3644	103	89.7140
569	4133	9	59/877	23	157/994	104.5223	104	89.8720
570	4139	9	53/788	23	157/994	104.6803	104	90.0299
571	4153	9	39/580	23	157/994	104.8382	105	90.1879
572	4157	9	8/119	23	157/994	104.9961	105	90.3458
573	4159	9	33/491	23	157/994	105.1541	105	90.5038
574	4177	9	17/253	23	157/994	105.3120	105	90.6617
575	4201	9	35/521	23	157/994	105.4700	105	90.8197
576	4211	9	9/134	24	157/994	105.6279	105	90.9776
577	4217	9	28/417	24	157/994	105.7859	106	91.1355
578	4219	9	29/432	24	157/994	105.9438	106	91.2935
579	4229	9	10/149	24	157/994	106.1018	106	91.4514
580	4231	9	31/462	24	157/994	106.2597	106	91.6094
581	4241	9	43/641	24	157/994	106.4177	106	91.7673
582	4243	9	67/999	24	157/994	106.5756	106	91.9253
583	4253	9	58/865	24	157/994	106.7336	106	92.0832
584	4259	9	12/179	24	157/994	106.8915	106	92.2412
585	4261	9	38/567	24	157/994	107.0495	106	92.3991
586	4271	9	53/791	24	157/994	107.2074	106	92.5571
587	4273	9	14/209	24	157/994	107.3654	107	92.7150
588	4283	9	29/433	24	157/994	107.5233	107	92.8730
589	4289	9	46/687	24	157/994	107.6812	107	93.0309
590	4297	9	65/971	24	157/994	107.8392	107	93.1889

n	p_n	$\pi(\sqrt{n})$	$\prod_{j=1}^{n}(1-1/p_j)$	$[\sqrt{n}]$	$\prod_{j=1}^{\pi(\sqrt{n})}(1-1/p_j)$	$\sum_{j=1}^{n}\prod_{i=1}^{\pi(\sqrt{j})}(1-1/p_j)$	$\pi(n)$	$n.\prod_{i=1}^{\pi(\sqrt{n})}(1-1/p_j)$
591	4327	9	17/254	24	157/994	107.9971	107	93.3468
592	4337	9	55/822	24	157/994	108.1551	107	93.5048
593	4339	9	39/583	24	157/994	108.3130	108	93.6627
594	4349	9	21/314	24	157/994	108.4710	108	93.8207
595	4357	9	45/673	24	157/994	108.6289	108	93.9786
596	4363	9	49/733	24	157/994	108.7869	108	94.1365
597	4373	9	53/793	24	157/994	108.9448	108	94.2945
598	4391	9	29/434	24	157/994	109.1028	108	94.4524
599	4397	9	65/973	24	157/994	109.2607	109	94.6104
600	4409	9	37/554	24	157/994	109.4187	109	94.7683
601	4421	9	42/629	24	157/994	109.5766	110	94.9263
602	4423	9	49/734	24	157/994	109.7346	110	95.0842
603	4441	9	58/869	24	157/994	109.8925	110	95.2422
604	4447	9	1/15	24	157/994	110.0505	110	95.4001
605	4451	9	1/15	24	157/994	110.2084	110	95.5581
606	4457	9	1/15	24	157/994	110.3664	110	95.7160
607	4463	9	1/15	24	157/994	110.5243	111	95.8740
608	4481	9	1/15	24	157/994	110.6822	111	96.0319
609	4483	9	1/15	24	157/994	110.8402	111	96.1899
610	4493	9	1/15	24	157/994	110.9981	111	96.3478
611	4507	9	1/15	24	157/994	111.1561	111	96.5058
612	4513	9	1/15	24	157/994	111.3140	111	96.6637
613	4517	9	61/916	24	157/994	111.4720	112	96.8216
614	4519	9	51/766	24	157/994	111.6299	112	96.9796
615	4523	9	44/661	24	157/994	111.7879	112	97.1375
616	4547	9	38/571	24	157/994	111.9458	112	97.2955
617	4549	9	34/511	24	157/994	112.1038	113	97.4534
618	4561	9	61/917	24	157/994	112.2617	113	97.6114
619	4567	9	55/827	24	157/994	112.4197	114	97.7693
620	4583	9	51/767	24	157/994	112.5776	114	97.9273
621	4591	9	47/707	24	157/994	112.7356	114	98.0852
622	4597	9	65/978	24	157/994	112.8935	114	98.2432
623	4603	9	61/918	24	157/994	113.0515	114	98.4011
624	4621	9	19/286	24	157/994	113.2094	114	98.5591
625	4637	9	18/271	25	157/994	113.3673	114	98.7170
626	4639	9	17/256	25	157/994	113.5253	114	98.8750
627	4643	9	16/241	25	157/994	113.6832	114	99.0329
628	4649	9	61/919	25	157/994	113.8412	114	99.1909
629	4651	9	29/437	25	157/994	113.9991	114	99.3488
630	4657	9	14/211	25	157/994	114.1571	114	99.5068
631	4663	9	40/603	25	157/994	114.3150	115	99.6647
632	4673	9	51/769	25	157/994	114.4730	115	99.8226
633	4679	9	49/739	25	157/994	114.6309	115	99.9806
634	4691	9	59/890	25	157/994	114.7889	115	100.1385
635	4703	9	34/513	25	157/994	114.9468	115	100.2965
636	4721	9	11/166	25	157/994	115.1048	115	100.4544
637	4723	9	53/800	25	157/994	115.2627	115	100.6124
638	4729	9	41/619	25	157/994	115.4207	115	100.7703
639	4733	9	10/151	25	157/994	115.5786	115	100.9283
640	4751	9	48/725	25	157/994	115.7366	115	101.0862
641	4759	9	28/423	25	157/994	115.8945	116	101.2442
642	4783	9	9/136	25	157/994	116.0525	116	101.4021
643	4787	9	44/665	25	157/994	116.2104	117	101.5601
644	4789	9	60/907	25	157/994	116.3683	117	101.7180
645	4793	9	25/378	25	157/994	116.5263	117	101.8760
646	4799	9	65/983	25	157/994	116.6842	117	102.0339
647	4801	9	8/121	25	157/994	116.8422	118	102.1919
648	4813	9	31/469	25	157/994	117.0001	118	102.3498
649	4817	9	15/227	25	157/994	117.1581	118	102.5077
650	4831	9	59/893	25	157/994	117.3160	118	102.6657
651	4861	9	36/545	25	157/994	117.4740	118	102.8236
652	4871	9	7/106	25	157/994	117.6319	118	102.9816
653	4877	9	7/106	25	157/994	117.7899	119	103.1395
654	4889	9	27/409	25	157/994	117.9478	119	103.2975
655	4903	9	53/803	25	157/994	118.1058	119	103.4554
656	4909	9	13/197	25	157/994	118.2637	119	103.6134
657	4919	9	19/288	25	157/994	118.4217	119	103.7713
658	4931	9	25/379	25	157/994	118.5796	119	103.9293
659	4933	9	49/743	25	157/994	118.7376	120	104.0872
660	4937	9	6/91	25	157/994	118.8955	120	104.2452
661	4943	9	59/895	25	157/994	119.0534	121	104.4031
662	4951	9	29/440	25	157/994	119.2114	121	104.5611
663	4957	9	57/865	25	157/994	119.3693	121	104.7190
664	4967	9	28/425	25	157/994	119.5273	121	104.8770
665	4969	9	11/167	25	157/994	119.6852	121	105.0349
666	4973	9	27/410	25	157/994	119.8432	121	105.1929
667	4987	9	16/243	25	157/994	120.0011	121	105.3508
668	4993	9	21/319	25	157/994	120.1591	121	105.5087

n	p_n	$\pi(\sqrt{n})$	$\prod_{j=1}^{n}(1-1/p_j)$	$[\sqrt{n}]$	$\prod_{j=1}^{\pi(\sqrt{n})}(1-1/p_j)$	$\sum_{j=1}^{n}\prod_{i=1}^{\pi(\sqrt{j})}(1-1/p_j)$	$\pi(n)$	$n.\prod_{i=1}^{\pi(\sqrt{n})}(1-1/p_j)$
669	4999	9	31/471	25	157/994	120.3170	121	105.6667
670	5003	9	5/76	25	157/994	120.4750	121	105.8246
671	5009	9	5/76	25	157/994	120.6329	121	105.9826
672	5011	9	64/973	25	157/994	120.7909	121	106.1405
673	5021	9	34/517	25	157/994	120.9488	122	106.2985
674	5023	9	43/654	25	157/994	121.1068	122	106.4564
675	5039	9	33/502	25	157/994	121.2647	122	106.6144
676	5051	9	14/213	26	157/994	121.4227	122	106.7723
677	5059	9	55/837	26	157/994	121.5806	123	106.9303
678	5077	9	9/137	26	157/994	121.7386	123	107.0882
679	5081	9	58/883	26	157/994	121.8965	123	107.2462
680	5087	9	22/335	26	157/994	122.0544	123	107.4041
681	5099	9	13/198	26	157/994	122.2124	123	107.5621
682	5101	9	30/457	26	157/994	122.3703	123	107.7200
683	5107	9	17/259	26	157/994	122.5283	124	107.8780
684	5113	9	46/701	26	157/994	122.6862	124	108.0359
685	5119	9	62/945	26	157/994	122.8442	124	108.1938
686	5147	9	49/747	26	157/994	123.0021	124	108.3518
687	5153	9	4/61	26	157/994	123.1601	124	108.5097
688	5167	9	4/61	26	157/994	123.3180	124	108.6677
689	5171	9	63/961	26	157/994	123.4760	124	108.8256
690	5179	9	35/534	26	157/994	123.6339	124	108.9836
691	5189	9	50/763	26	157/994	123.7919	125	109.1415
692	5197	9	19/290	26	157/994	123.9498	125	109.2995
693	5209	9	64/977	26	157/994	124.1078	125	109.4574
694	5227	9	41/626	26	157/994	124.2657	125	109.6154
695	5231	9	11/168	26	157/994	124.4237	125	109.7733
696	5233	9	51/779	26	157/994	124.5816	125	109.9313
697	5237	9	18/275	26	157/994	124.7395	125	110.0892
698	5261	9	25/382	26	157/994	124.8975	125	110.2472
699	5273	9	53/810	26	157/994	125.0554	125	110.4051
700	5279	9	7/107	26	157/994	125.2134	125	110.5631
701	5281	9	45/688	26	157/994	125.3713	126	110.7210
702	5297	9	24/367	26	157/994	125.5293	126	110.8790
703	5303	9	17/260	26	157/994	125.6872	126	111.0369
704	5309	9	37/566	26	157/994	125.8452	126	111.1948
705	5323	9	10/153	26	157/994	126.0031	126	111.3528
706	5333	9	33/505	26	157/994	126.1611	126	111.5107
707	5347	9	49/750	26	157/994	126.3190	126	111.6687
708	5351	9	55/842	26	157/994	126.4770	126	111.8266
709	5381	9	16/245	26	157/994	126.6349	127	111.9846
710	5387	9	54/827	26	157/994	126.7929	127	112.1425
711	5393	9	63/965	26	157/994	126.9508	127	112.3005
712	5399	9	25/383	26	157/994	127.1088	127	112.4584
713	5407	9	65/996	26	157/994	127.2667	127	112.6164
714	5413	9	46/705	26	157/994	127.4247	127	112.7743
715	5417	9	3/46	26	157/994	127.5826	127	112.9323
716	5419	9	3/46	26	157/994	127.7405	127	113.0902
717	5431	9	3/46	26	157/994	127.8985	127	113.2482
718	5437	9	3/46	26	157/994	128.0564	127	113.4061
719	5441	9	50/767	26	157/994	128.2144	128	113.5641
720	5443	9	35/537	26	157/994	128.3723	128	113.7220
721	5449	9	26/399	26	157/994	128.5303	128	113.8799
722	5471	9	43/660	26	157/994	128.6882	128	114.0379
723	5477	9	37/568	26	157/994	128.8462	128	114.1958
724	5479	9	48/737	26	157/994	129.0041	128	114.3538
725	5483	9	14/215	26	157/994	129.1621	128	114.5117
726	5501	9	25/384	26	157/994	129.3200	128	114.6697
727	5503	9	11/169	26	157/994	129.4780	129	114.8276
728	5507	9	52/799	26	157/994	129.6359	129	114.9856
729	5519	9	19/292	27	157/994	129.7939	129	115.1435
730	5521	9	62/953	27	157/994	129.9518	129	115.3015
731	5527	9	8/123	27	157/994	130.1098	129	115.4594
732	5531	9	8/123	27	157/994	130.2677	129	115.6174
733	5557	9	29/446	27	157/994	130.4256	130	115.7753
734	5563	9	34/523	27	157/994	130.5836	130	115.9333
735	5569	9	13/200	27	157/994	130.7415	130	116.0912
736	5573	9	49/754	27	157/994	130.8995	130	116.2492
737	5581	9	41/631	27	157/994	131.0574	130	116.4071
738	5591	9	28/431	27	157/994	131.2154	130	116.5651
739	5623	9	48/739	27	157/994	131.3733	131	116.7230
740	5639	9	5/77	27	157/994	131.5313	131	116.8809
741	5641	9	5/77	27	157/994	131.6892	131	117.0389
742	5647	9	52/801	27	157/994	131.8472	131	117.1968
743	5651	9	59/909	27	157/994	132.0051	132	117.3548
744	5653	9	22/339	27	157/994	132.1631	132	117.5127
745	5657	9	17/262	27	157/994	132.3210	132	117.6707
746	5659	9	53/817	27	157/994	132.4790	132	117.8286

n	p_n	$\pi(\sqrt{n})$	$\prod_{j=1}^{n}(1-1/p_j)$	$[\sqrt{n}]$	$\prod_{j=1}^{\pi(\sqrt{n})}(1-1/p_j)$	$\sum_{j=1}^{n}\prod_{i=1}^{\pi(\sqrt{j})}(1-1/p_j)$	$\pi(n)$	$n.\prod_{i=1}^{\pi(\sqrt{n})}(1-1/p_j)$
747	5669	9	12/185	27	157/994	132.6369	132	117.9866
748	5683	9	19/293	27	157/994	132.7949	132	118.1445
749	5689	9	26/401	27	157/994	132.9528	132	118.3025
750	5693	9	54/833	27	157/994	133.1108	132	118.4604
751	5701	9	7/108	27	157/994	133.2687	133	118.6184
752	5711	9	58/895	27	157/994	133.4266	133	118.7763
753	5717	9	53/818	27	157/994	133.5846	133	118.9343
754	5737	9	55/849	27	157/994	133.7425	133	118.0922
755	5741	9	41/633	27	157/994	133.9005	133	119.2502
756	5743	9	43/664	27	157/994	134.0584	133	119.4081
757	5749	9	9/139	27	157/994	134.2164	134	119.5660
758	5779	9	38/587	27	157/994	134.3743	134	119.7240
759	5783	9	20/309	27	157/994	134.5323	134	119.8819
760	5791	9	53/819	27	157/994	134.6902	134	120.0399
761	5801	9	11/170	27	157/994	134.8482	135	120.1978
762	5807	9	24/371	27	157/994	135.0061	135	120.3558
763	5813	9	13/201	27	157/994	135.1641	135	120.5137
764	5821	9	41/634	27	157/994	135.3220	135	120.6717
765	5827	9	15/232	27	157/994	135.4800	135	120.8296
766	5839	9	32/495	27	157/994	135.6379	135	120.9876
767	5843	9	17/263	27	157/994	135.7959	135	121.1455
768	5849	9	19/294	27	157/994	135.9538	135	121.3035
769	5851	9	21/325	27	157/994	136.1117	136	121.4614
770	5857	9	48/743	27	157/994	136.2697	136	121.6194
771	5861	9	27/418	27	157/994	136.4276	136	121.7773
772	5867	9	64/991	27	157/994	136.5856	136	121.9353
773	5869	9	39/604	27	157/994	136.7435	137	122.0932
774	5879	9	49/759	27	157/994	136.9015	137	122.2512
775	5881	9	2/31	27	157/994	137.0594	137	122.4091
776	5897	9	2/31	27	157/994	137.2174	137	122.5670
777	5903	9	2/31	27	157/994	137.3753	137	122.7250
778	5923	9	2/31	27	157/994	137.5333	137	122.8829
779	5927	9	2/31	27	157/994	137.6912	137	123.0409
780	5939	9	2/31	27	157/994	137.8492	137	123.1988
781	5953	9	63/977	27	157/994	138.0071	137	123.3568
782	5981	9	47/729	27	157/994	138.1651	137	123.5147
783	5987	9	37/574	27	157/994	138.3230	137	123.6727
784	6007	9	31/481	28	157/994	138.4810	137	123.8306
785	6011	9	27/419	28	157/994	138.6389	137	123.9886
786	6029	9	48/745	28	157/994	138.7969	137	124.1465
787	6037	9	21/326	28	157/994	138.9548	138	124.3045
788	6043	9	19/295	28	157/994	139.1127	138	124.4624
789	6047	9	17/264	28	157/994	139.2707	138	124.6204
790	6053	9	32/497	28	157/994	139.4286	138	124.7783
791	6067	9	15/233	28	157/994	139.5866	138	124.9363
792	6073	9	41/637	28	157/994	139.7445	138	125.0942
793	6079	9	13/202	28	157/994	139.9025	138	125.2521
794	6089	9	24/373	28	157/994	140.0604	138	125.4101
795	6091	9	57/886	28	157/994	140.2184	138	125.5680
796	6101	9	11/171	28	157/994	140.3763	138	125.7260
797	6113	9	51/793	28	157/994	140.5343	139	125.8839
798	6121	9	29/451	28	157/994	140.6922	139	126.0419
799	6131	9	9/140	28	157/994	140.8502	139	126.1998
800	6133	9	9/140	28	157/994	141.0081	139	126.3578
801	6143	9	59/918	28	157/994	141.1661	139	126.5157
802	6151	9	16/249	28	157/994	141.3240	139	126.6737
803	6163	9	39/607	28	157/994	141.4820	139	126.8316
804	6173	9	30/467	28	157/994	141.6399	139	126.9896
805	6197	9	7/109	28	157/994	141.7978	139	127.1475
806	6199	9	7/109	28	157/994	141.9558	139	127.3055
807	6203	9	47/732	28	157/994	142.1137	139	127.4634
808	6211	9	26/405	28	157/994	142.2717	139	127.6214
809	6217	9	19/296	28	157/994	142.4296	140	127.7793
810	6221	9	55/857	28	157/994	142.5876	140	127.9373
811	6229	9	12/187	28	157/994	142.7455	141	128.0952
812	6247	9	46/717	28	157/994	142.9035	141	128.2531
813	6257	9	56/873	28	157/994	143.0614	141	128.4111
814	6263	9	49/764	28	157/994	143.2194	141	128.5690
815	6269	9	37/577	28	157/994	143.3773	141	128.7270
816	6271	9	62/967	28	157/994	143.5353	141	128.8849
817	6277	9	5/78	28	157/994	143.6932	141	129.0429
818	6287	9	5/78	28	157/994	143.8512	141	129.2008
819	6299	9	48/749	28	157/994	144.0091	141	129.3588
820	6301	9	28/437	28	157/994	144.1671	141	129.5167
821	6311	9	64/999	28	157/994	144.3250	142	129.6747
822	6317	9	18/281	28	157/994	144.4830	142	129.8326
823	6323	9	57/890	28	157/994	144.6409	143	129.9906
824	6329	9	60/937	28	157/994	144.7988	143	130.1485

n	p_n	$\pi(\sqrt{n})$	$\prod_{j=1}^{n}(1-1/p_j)$	$[\sqrt{n}]$	$\prod_{j=1}^{\pi(\sqrt{n})}(1-1/p_j)$	$\sum_{j=1}^{n}\prod_{i=1}^{\pi(\sqrt{j})}(1-1/p_j)$	$\pi(n)$	$n.\prod_{i=1}^{\pi(\sqrt{n})}(1-1/p_j)$
825	6337	9	21/328	28	157/994	144.9568	143	130.3065
826	6343	9	37/578	28	157/994	145.1147	143	130.4644
827	6353	9	8/125	28	157/994	145.2727	144	130.6224
828	6359	9	8/125	28	157/994	145.4306	144	130.7803
829	6361	9	62/969	28	157/994	145.5886	145	130.9382
830	6367	9	19/297	28	157/994	145.7465	145	131.0962
831	6373	9	41/641	28	157/994	145.9045	145	131.2541
832	6379	9	11/172	28	157/994	146.0624	145	131.4121
833	6389	9	36/563	28	157/994	146.2204	145	131.5700
834	6397	9	39/610	28	157/994	146.3783	145	131.7280
835	6421	9	14/219	28	157/994	146.5363	145	131.8859
836	6427	9	48/751	28	157/994	146.6942	145	132.0439
837	6449	9	37/579	28	157/994	146.8522	145	132.2018
838	6451	9	63/986	28	157/994	147.0101	145	132.3598
839	6469	9	26/407	28	157/994	147.1681	146	132.5177
840	6473	9	61/955	28	157/994	147.3260	146	132.6757
841	6481	10	38/595	29	142/929	147.4789	146	128.5487
842	6491	10	56/877	29	142/929	147.6317	146	128.7015
843	6521	10	3/47	29	142/929	147.7846	146	128.8544
844	6529	10	3/47	29	142/929	147.9374	146	129.0072
845	6547	10	3/47	29	142/929	148.0903	146	129.1601
846	6551	10	3/47	29	142/929	148.2431	146	129.3129
847	6553	10	55/862	29	142/929	148.3960	146	129.4658
848	6563	10	40/627	29	142/929	148.5488	146	129.6186
849	6569	10	31/486	29	142/929	148.7017	146	129.7715
850	6571	10	25/392	29	142/929	148.8545	146	129.9243
851	6577	10	22/345	29	142/929	149.0074	146	130.0772
852	6581	10	19/298	29	142/929	149.1602	146	130.2300
853	6599	10	16/251	29	142/929	149.3131	147	130.3829
854	6607	10	29/455	29	142/929	149.4659	147	130.5357
855	6619	10	13/204	29	142/929	149.6188	147	130.6886
856	6637	10	49/769	29	142/929	149.7716	147	130.8414
857	6653	10	56/879	29	142/929	149.9245	148	130.9943
858	6659	10	10/157	29	142/929	150.0773	148	131.1471
859	6661	10	10/157	29	142/929	150.2302	149	131.3000
860	6673	10	27/424	29	142/929	150.3830	149	131.4529
861	6679	10	17/267	29	142/929	150.5359	149	131.6057
862	6689	10	24/377	29	142/929	150.6887	149	131.7586
863	6691	10	38/597	29	142/929	150.8416	150	131.9114
864	6701	10	7/110	29	142/929	150.9945	150	132.0643
865	6703	10	7/110	29	142/929	151.1473	150	132.2171
866	6709	10	46/723	29	142/929	151.3002	150	132.3700
867	6719	10	25/393	29	142/929	151.4530	150	132.5228
868	6733	10	18/283	29	142/929	151.6059	150	132.6757
869	6737	10	29/456	29	142/929	151.7587	150	132.8285
870	6761	10	11/173	29	142/929	151.9116	150	132.9814
871	6763	10	48/755	29	142/929	152.0644	150	133.1342
872	6779	10	41/645	29	142/929	152.2173	150	133.2871
873	6781	10	15/236	29	142/929	152.3701	150	133.4399
874	6791	10	19/299	29	142/929	152.5230	150	133.5928
875	6793	10	42/661	29	142/929	152.6758	150	133.7456
876	6803	10	27/425	29	142/929	152.8287	150	133.8985
877	6823	10	35/551	29	142/929	152.9815	151	134.0513
878	6827	10	55/866	29	142/929	153.1344	151	134.2042
879	6829	10	4/63	29	142/929	153.2872	151	134.3570
880	6833	10	4/63	29	142/929	153.4401	151	134.5099
881	6841	10	4/63	29	142/929	153.5929	152	134.6627
882	6857	10	53/835	29	142/929	153.7458	152	134.8156
883	6863	10	37/583	29	142/929	153.8986	153	134.9684
884	6869	10	54/851	29	142/929	154.0515	153	135.1213
885	6871	10	21/331	29	142/929	154.2043	153	135.2742
886	6883	10	55/867	29	142/929	154.3572	153	135.4270
887	6899	10	47/741	29	142/929	154.5101	154	135.5799
888	6907	10	13/205	29	142/929	154.6629	154	135.7327
889	6911	10	61/962	29	142/929	154.8158	154	135.8856
890	6917	10	22/347	29	142/929	154.9686	154	136.0384
891	6947	10	40/631	29	142/929	155.1215	154	136.1913
892	6949	10	9/142	29	142/929	155.2743	154	136.3441
893	6959	10	59/931	29	142/929	155.4272	154	136.4970
894	6961	10	55/868	29	142/929	155.5800	154	136.6498
895	6967	10	51/805	29	142/929	155.7329	154	136.8027
896	6971	10	14/221	29	142/929	155.8857	154	136.9555
897	6977	10	19/300	29	142/929	156.0386	154	137.1084
898	6983	10	43/679	29	142/929	156.1914	154	137.2612
899	6991	10	29/458	29	142/929	156.3443	154	137.4141
900	6997	10	44/695	30	142/929	156.4971	154	137.5669
901	7001	10	5/79	30	142/929	156.6500	154	137.7198
902	7013	10	5/79	30	142/929	156.8028	154	137.8726

n	p_n	$\pi(\sqrt{n})$	$\prod_{j=1}^{n}(1-1/p_j)$	$[\sqrt{n}]$	$\prod_{j=1}^{\pi(\sqrt{n})}(1-1/p_j)$	$\sum_{j=1}^{n}\prod_{i=1}^{\pi(\sqrt{j})}(1-1/p_j)$	$\pi(n)$	$n.\prod_{i=1}^{\pi(\sqrt{n})}(1-1/p_j)$
903	7019	10	5/79	30	142/929	156.9557	154	138.0255
904	7027	10	46/727	30	142/929	157.1085	154	138.1783
905	7039	10	31/490	30	142/929	157.2614	154	138.3312
906	7043	10	21/332	30	142/929	157.4142	154	138.4840
907	7057	10	53/838	30	142/929	157.5671	155	138.6369
908	7069	10	59/933	30	142/929	157.7199	155	138.7898
909	7079	10	38/601	30	142/929	157.8728	155	138.9426
910	7103	10	11/174	30	142/929	158.0257	155	139.0955
911	7109	10	50/791	30	142/929	158.1785	156	139.2483
912	7121	10	45/712	30	142/929	158.3314	156	139.4012
913	7127	10	57/902	30	142/929	158.4842	156	139.4450
914	7129	10	52/823	30	142/929	158.6371	156	139.7069
915	7151	10	41/649	30	142/929	158.7899	156	139.8597
916	7159	10	6/95	30	142/929	158.9428	156	140.0126
917	7177	10	6/95	30	142/929	159.0956	156	140.1654
918	7187	10	6/95	30	142/929	159.2485	156	140.3183
919	7193	10	37/586	30	142/929	159.4013	157	140.4711
920	7207	10	25/396	30	142/929	159.5542	157	140.6240
921	7211	10	19/301	30	142/929	159.7070	157	140.7768
922	7213	10	45/713	30	142/929	159.8599	157	140.9297
923	7219	10	13/206	30	142/929	160.0127	157	141.0825
924	7229	10	53/840	30	142/929	160.1656	157	141.2354
925	7237	10	47/745	30	142/929	160.3184	157	141.3882
926	7243	10	34/539	30	142/929	160.4713	157	141.5411
927	7247	10	7/111	30	142/929	160.6241	157	141.6939
928	7253	10	7/111	30	142/929	160.7770	157	141.8468
929	7283	10	57/904	30	142/929	160.9298	158	141.9996
930	7297	10	29/460	30	142/929	161.0827	158	142.1525
931	7307	10	22/349	30	142/929	161.2355	158	142.3054
932	7309	10	15/238	30	142/929	161.3884	158	142.4582
933	7321	10	38/603	30	142/929	161.5413	158	142.6111
934	7331	10	31/492	30	142/929	161.6941	158	142.7639
935	7333	10	55/873	30	142/929	161.8470	158	142.9168
936	7349	10	8/127	30	142/929	161.9998	158	143.0696
937	7351	10	57/905	30	142/929	162.1527	159	143.2225
938	7369	10	58/921	30	142/929	162.3055	159	143.3753
939	7393	10	59/937	30	142/929	162.4584	159	143.5282
940	7411	10	43/683	30	142/929	162.6112	159	143.6810
941	7417	10	35/556	30	142/929	162.7641	160	143.8339
942	7433	10	9/143	30	142/929	162.9169	160	143.9867
943	7451	10	9/143	30	142/929	163.0698	160	144.1396
944	7457	10	37/588	30	142/929	163.2226	160	144.2924
945	7459	10	19/302	30	142/929	163.3755	160	144.4453
946	7477	10	29/461	30	142/929	163.5283	160	144.5981
947	7481	10	59/938	30	142/929	163.6812	161	144.7510
948	7487	10	10/159	30	142/929	163.8340	161	144.9038
949	7489	10	41/652	30	142/929	163.9869	161	145.0567
950	7499	10	21/334	30	142/929	164.1397	161	145.2095
951	7507	10	43/684	30	142/929	164.2926	161	145.3624
952	7517	10	11/175	30	142/929	164.4454	161	145.5152
953	7523	10	45/716	30	142/929	164.5983	162	145.6681
954	7529	10	23/366	30	142/929	164.7511	162	145.8210
955	7537	10	59/939	30	142/929	164.9040	162	145.9738
956	7541	10	12/191	30	142/929	165.0569	162	146.1267
957	7547	10	25/398	30	142/929	165.2097	162	146.2795
958	7549	10	51/812	30	142/929	165.3626	162	146.4324
959	7559	10	13/207	30	142/929	165.5154	162	146.5852
960	7561	10	27/430	30	142/929	165.6683	162	146.7381
961	7573	11	14/223	31	29/195	165.8170	162	142.9209
962	7577	11	43/685	31	29/195	165.9657	162	143.0696
963	7583	11	59/940	31	29/195	166.1144	162	143.2183
964	7589	11	61/972	31	29/195	166.2631	162	143.3671
965	7591	11	47/749	31	29/195	166.4119	162	143.5158
966	7603	11	49/781	31	29/195	166.5606	162	143.6645
967	7607	11	17/271	31	29/195	166.7093	163	143.8132
968	7621	11	35/558	31	29/195	166.8580	163	143.9619
969	7639	11	18/287	31	29/195	167.0067	163	144.1107
970	7643	11	19/303	31	29/195	167.1555	163	144.2594
971	7649	11	59/941	31	29/195	167.3042	164	144.4081
972	7669	11	41/654	31	29/195	167.4529	164	144.5568
973	7673	11	43/686	31	29/195	167.6016	164	144.7055
974	7681	11	45/718	31	29/195	167.7504	164	144.8543
975	7687	11	47/750	31	29/195	167.8991	164	145.0030
976	7691	11	49/782	31	29/195	168.0478	164	145.1517
977	7699	11	26/415	31	29/195	168.1965	165	145.3004
978	7703	11	55/878	31	29/195	168.3452	165	145.4491
979	7717	11	29/463	31	29/195	168.4940	165	145.5979
980	7723	11	31/495	31	29/195	168.6427	165	145.7466

n	p_n	$\pi(\sqrt{n})$	$\prod_{j=1}^{n}(1-1/p_j)$	$[\sqrt{n}]$	$\prod_{j=1}^{\pi(\sqrt{n})}(1-1/p_j)$	$\sum_{j=1}^{n}\prod_{i=1}^{\pi(\sqrt{j})}(1-1/p_j)$	$\pi(n)$	$n.\prod_{i=1}^{\pi(\sqrt{n})}(1-1/p_j)$
981	7727	11	33/527	31	29/195	168.7914	165	145.8953
982	7741	11	36/575	31	29/195	168.9401	165	146.0440
983	7753	11	38/607	31	29/195	169.0888	166	146.1928
984	7757	11	42/671	31	29/195	169.2376	166	146.3415
985	7759	11	45/719	31	29/195	169.3863	166	146.4902
986	7789	11	50/799	31	29/195	169.5350	166	146.6389
987	7793	11	56/895	31	29/195	169.6837	166	146.7876
988	7817	11	1/16	31	29/195	169.8324	166	146.9364
989	7823	11	1/16	31	29/195	169.9812	166	147.0851
990	7829	11	1/16	31	29/195	170.1299	166	147.2338
991	7841	11	1/16	31	29/195	170.2786	167	147.3825
992	7853	11	1/16	31	29/195	170.4273	167	147.5312
993	7867	11	1/16	31	29/195	170.5761	167	147.6800
994	7873	11	1/16	31	29/195	170.7248	167	147.8287
995	7877	11	1/16	31	29/195	170.8735	167	147.9774
996	7879	11	1/16	31	29/195	171.0222	167	148.1261
997	7883	11	1/16	31	29/195	171.1709	168	148.2748
998	7901	11	1/16	31	29/195	171.3197	168	148.4236
999	7907	11	1/16	31	29/195	171.4684	168	148.5723
1000	7919	11	1/16	31	29/195	171.6171	168	148.7210
1001	7927	11	1/16	31	29/195	171.7658	168	148.8697
1002	7933	11	1/16	31	29/195	171.9145	169	149.0185
1003	7937	11	1/16	31	29/195	172.0633	169	149.1672
1004	7949	11	60/961	31	29/195	172.2120	169	149.3159
1005	7951	11	54/865	31	29/195	172.3607	169	149.4646
1006	7963	11	48/769	31	29/195	172.5094	169	149.6133
1007	7993	11	44/705	31	29/195	172.6581	169	149.7621
1008	8009	11	41/657	31	29/195	172.8069	169	149.9108
1009	8011	11	38/609	31	29/195	172.9556	169	150.0595
1010	8017	11	35/561	31	29/195	173.1043	169	150.2082
1011	8039	11	33/529	31	29/195	173.2530	169	150.3569
1012	8053	11	61/978	31	29/195	173.4018	169	150.5057
1013	8059	11	29/465	31	29/195	173.5505	170	150.6544
1014	8069	11	27/433	31	29/195	173.6992	170	150.8031
1015	8081	11	26/417	31	29/195	173.8479	170	150.9518
1016	8087	11	49/786	31	29/195	173.9966	170	151.1005
1017	8089	11	47/754	31	29/195	174.1454	170	151.2493
1018	8093	11	45/722	31	29/195	174.2941	170	151.3980
1019	8101	11	43/690	31	29/195	174.4428	171	151.5467
1020	8111	11	62/995	31	29/195	174.5915	171	151.6954
1021	8117	11	20/321	31	29/195	174.7402	172	151.8442
1022	8123	11	19/305	31	29/195	174.8890	172	151.9929
1023	8147	11	55/883	31	29/195	175.0377	172	152.1416
1024	8161	11	53/851	32	29/195	175.1864	172	152.2903
1025	8167	11	17/273	32	29/195	175.3351	172	152.4390
1026	8171	11	33/530	32	29/195	175.4838	172	152.5878
1027	8179	11	16/257	32	29/195	175.6326	172	152.7365
1028	8191	11	31/498	32	29/195	175.7813	172	152.8852
1029	8209	11	15/241	32	29/195	175.9300	172	153.0339
1030	8219	11	44/707	32	29/195	176.0787	172	153.1826
1031	8221	11	57/916	32	29/195	176.2275	173	153.3314
1032	8231	11	14/225	32	29/195	176.3762	173	153.4801
1033	8233	11	27/434	32	29/195	176.5249	174	153.6288
1034	8237	11	13/209	32	29/195	176.6736	174	153.7775
1035	8243	11	13/209	32	29/195	176.8223	174	153.9262
1036	8263	11	25/402	32	29/195	176.9711	174	154.0750
1037	8269	11	61/981	32	29/195	177.1198	174	154.2237
1038	8273	11	12/193	32	29/195	177.2685	174	154.3724
1039	8287	11	35/563	32	29/195	177.4172	175	154.5211
1040	8291	11	57/917	32	29/195	177.5659	175	154.6699
1041	8293	11	11/177	32	29/195	177.7147	175	154.8186
1042	8297	11	11/177	32	29/195	177.8634	175	154.9673
1043	8311	11	32/515	32	29/195	178.0121	175	155.1160
1044	8317	11	21/338	32	29/195	178.1608	175	155.2647
1045	8329	11	41/660	32	29/195	178.3095	175	155.4135
1046	8353	11	10/161	32	29/195	178.4583	175	155.5622
1047	8363	11	10/161	32	29/195	178.6070	175	155.7109
1048	8369	11	29/467	32	29/195	178.7557	175	155.8596
1049	8377	11	19/306	32	29/195	178.9044	176	156.0083
1050	8387	11	28/451	32	29/195	179.0532	176	156.1571
1051	8389	11	46/741	32	29/195	179.2019	177	156.3058
1052	8419	11	9/145	32	29/195	179.3506	177	156.4545
1053	8423	11	62/999	32	29/195	179.4993	177	156.6032
1054	8429	11	61/983	32	29/195	179.6480	177	156.7519
1055	8431	11	60/967	32	29/195	179.7968	177	156.9007
1056	8443	11	59/951	32	29/195	179.9455	177	157.0494
1057	8447	11	25/403	32	29/195	180.0942	177	157.1981
1058	8461	11	49/790	32	29/195	180.2429	177	157.3468

n	p_n	$\pi(\sqrt{n})$	$\prod_{j=1}^{n}(1-1/p_j)$	$[\sqrt{n}]$	$\prod_{j=1}^{\pi(\sqrt{n})}(1-1/p_j)$	$\sum_{j=1}^{n}\prod_{i=1}^{\pi(\sqrt{j})}(1-1/p_j)$	$\pi(n)$	$n.\prod_{i=1}^{\pi(\sqrt{n})}(1-1/p_j)$
1059	8467	11	8/129	32	29/195	180.3916	177	157.4956
1060	8501	11	8/129	32	29/195	180.5404	177	157.6443
1061	8513	11	39/629	32	29/195	180.6891	178	157.7930
1062	8521	11	54/871	32	29/195	180.8378	178	157.9417
1063	8527	11	38/613	32	29/195	180.9865	179	158.0904
1064	8537	11	15/242	32	29/195	181.1352	179	158.2392
1065	8539	11	59/952	32	29/195	181.2840	179	158.3879
1066	8543	11	51/823	32	29/195	181.4327	179	158.5366
1067	8563	11	43/694	32	29/195	181.5814	179	158.6853
1068	8573	11	7/113	32	29/195	181.7301	179	158.8340
1069	8581	11	7/113	32	29/195	181.8789	180	158.9828
1070	8597	11	7/113	32	29/195	182.0276	180	159.1315
1071	8599	11	34/549	32	29/195	182.1763	180	159.2802
1072	8609	11	47/759	32	29/195	182.3250	180	159.4289
1073	8623	11	20/323	32	29/195	182.4737	180	159.5776
1074	8627	11	59/953	32	29/195	182.6225	180	159.7264
1075	8629	11	13/210	32	29/195	182.7712	180	159.8751
1076	8641	11	32/517	32	29/195	182.9199	180	160.0238
1077	8647	11	19/307	32	29/195	183.0686	180	160.1725
1078	8663	11	25/404	32	29/195	183.2173	180	160.3213
1079	8669	11	37/598	32	29/195	183.3661	180	160.4700
1080	8677	11	55/889	32	29/195	183.5148	180	160.6187
1081	8681	11	6/97	32	29/195	183.6635	180	160.7674
1082	8689	11	6/97	32	29/195	183.8122	180	160.9161
1083	8693	11	6/97	32	29/195	183.9609	180	161.0649
1084	8699	11	35/566	32	29/195	184.1097	180	161.2136
1085	8707	11	52/841	32	29/195	184.2584	180	161.3623
1086	8713	11	40/647	32	29/195	184.4071	180	161.5110
1087	8719	11	17/275	32	29/195	184.5558	181	161.6597
1088	8731	11	28/453	32	29/195	184.7046	181	161.8085
1089	8737	11	61/987	33	29/195	184.8533	181	161.9572
1090	8741	11	11/178	33	29/195	185.0020	181	162.1059
1091	8747	11	38/615	33	29/195	185.1507	182	162.2546
1092	8753	11	43/696	33	29/195	185.2994	182	162.4033
1093	8761	11	16/259	33	29/195	185.4482	183	162.5521
1094	8779	11	58/939	33	29/195	185.5969	183	162.7008
1095	8783	11	47/761	33	29/195	185.7456	183	162.8495
1096	8803	11	31/502	33	29/195	185.8943	183	162.9982
1097	8807	11	41/664	33	29/195	186.0430	184	163.1470
1098	8819	11	61/988	33	29/195	186.1918	184	163.2957
1099	8821	11	5/81	33	29/195	186.3405	184	163.4444
1100	8831	11	5/81	33	29/195	186.4892	184	163.5931
1101	8837	11	5/81	33	29/195	186.6379	184	163.7418
1102	8839	11	49/794	33	29/195	186.7866	184	163.8906
1103	8849	11	34/551	33	29/195	186.9354	185	164.0393
1104	8861	11	24/389	33	29/195	187.0841	185	164.1880
1105	8863	11	19/308	33	29/195	187.2328	185	164.3367
1106	8867	11	52/843	33	29/195	187.3815	185	164.4854
1107	8887	11	14/227	33	29/195	187.5302	185	164.6342
1108	8893	11	14/227	33	29/195	187.6790	185	164.7829
1109	8923	11	23/373	33	29/195	187.8277	186	164.9316
1110	8929	11	32/519	33	29/195	187.9764	186	1650803
1111	8933	11	9/146	33	29/195	188.1251	186	165.2290
1112	8941	11	9/146	33	29/195	188.2739	186	165.3778
1113	8951	11	49/795	33	29/195	188.4226	186	165.5265
1114	8963	11	31/503	33	29/195	188.5713	186	165.6752
1115	8969	11	57/925	33	29/195	188.7200	186	165.8239
1116	8971	11	13/211	33	29/195	188.8687	186	165.9726
1117	8999	11	13/211	33	29/195	189.0175	187	166.1214
1118	9001	11	30/487	33	29/195	189.1662	187	166.2701
1119	9007	11	17/276	33	29/195	189.3149	187	166.4188
1120	9011	11	38/617	33	29/195	189.4636	187	166.5675
1121	9013	11	21/341	33	29/195	189.6123	187	166.7163
1122	9029	11	54/877	33	29/195	189.7611	187	166.8650
1123	9041	11	33/536	33	29/195	189.9098	188	167.0137
1124	9043	11	41/666	33	29/195	190.0585	188	167.1624
1125	9049	11	61/991	33	29/195	190.2072	188	167.3111
1126	9059	11	4/65	33	29/195	190.3559	188	167.4599
1127	9067	11	4/65	33	29/195	190.5047	188	167.6086
1128	9091	11	4/65	33	29/195	190.6534	188	167.7573
1129	9103	11	4/65	33	29/195	190.8021	189	167.9060
1130	9109	11	51/829	33	29/195	190.9508	189	168.0547
1131	9127	11	39/634	33	29/195	191.0996	189	168.2035
1132	9133	11	58/943	33	29/195	191.2483	189	168.3522
1133	9137	11	50/813	33	29/195	191.3970	189	168.5009
1134	9151	11	42/683	33	29/195	191.5457	189	168.6496
1135	9157	11	19/309	33	29/195	191.6944	189	168.7983
1136	9161	11	49/797	33	29/195	191.8432	189	168.9471

n	p_n	$\pi(\sqrt{n})$	$\prod_{j=1}^{n}(1-1/p_j)$	$[\sqrt{n}]$	$\prod_{j=1}^{\pi(\sqrt{n})}(1-1/p_j)$	$\sum_{j=1}^{n}\prod_{i=1}^{\pi(\sqrt{j})}(1-1/p_j)$	$\pi(n)$	$n.\prod_{i=1}^{\pi(\sqrt{n})}(1-1/p_j)$
1137	9173	11	15/244	33	29/195	191.9919	189	169.0958
1138	9181	11	26/423	33	29/195	192.1406	189	169.2445
1139	9187	11	48/781	33	29/195	192.2893	189	169.3932
1140	9199	11	11/179	33	29/195	192.4380	189	169.5420
1141	9203	11	51/830	33	29/195	192.5868	189	169.6907
1142	9209	11	29/472	33	29/195	192.7355	189	169.8394
1143	9221	11	18/293	33	29/195	192.8842	189	169.9881
1144	9227	11	25/407	33	29/195	193.0329	189	170.1368
1145	9239	11	32/521	33	29/195	193.1816	189	170.2856
1146	9241	11	60/977	33	29/195	193.3304	189	170.4343
1147	9257	11	7/114	33	29/195	193.4791	189	170.5830
1148	9277	11	7/114	33	29/195	193.6278	189	170.7317
1149	9281	11	52/847	33	29/195	193.7765	189	170.8804
1150	9283	11	31/505	33	29/195	193.9253	189	171.0292
1151	9293	11	24/391	33	29/195	194.0740	190	171.1779
1152	9311	11	17/277	33	29/195	194.2227	190	171.3266
1153	9319	11	44/717	33	29/195	194.3714	191	171.4753
1154	9323	11	37/603	33	29/195	194.5201	191	171.6240
1155	9337	11	10/163	33	29/195	194.6689	191	171.7728
1156	9341	11	10/163	34	29/195	194.8176	191	171.9215
1157	9343	11	43/701	34	29/195	194.9663	191	172.0702
1158	9349	11	23/375	34	29/195	195.1150	191	172.2189
1159	9371	11	49/799	34	29/195	195.2637	191	172.3677
1160	9377	11	13/212	34	29/195	195.4125	191	172.5164
1161	9391	11	42/685	34	29/195	195.5612	191	172.6651
1162	9397	11	45/734	34	29/195	195.7099	191	172.8138
1163	9403	11	16/261	34	29/195	195.8586	192	172.9625
1164	9413	11	54/881	34	29/195	196.0073	192	173.1113
1165	9419	11	19/310	34	29/195	196.1561	192	173.2600
1166	9421	11	22/359	34	29/195	196.3048	192	173.4087
1167	9431	11	25/408	34	29/195	196.4535	192	173.5574
1168	9433	11	28/457	34	29/195	196.6022	192	173.7061
1169	9437	11	34/555	34	29/195	196.7510	192	173.8549
1170	9439	11	40/653	34	29/195	196.8997	192	174.0036
1171	9461	11	52/849	34	29/195	197.0484	193	174.1523
1172	9463	11	3/49	34	29/195	197.1971	193	174.3010
1173	9467	11	3/49	34	29/195	197.3458	193	174.4497
1174	9473	11	3/49	34	29/195	197.4946	193	174.5985
1175	9479	11	3/49	34	29/195	197.6433	193	174.7472
1176	9491	11	3/49	34	29/195	197.7920	193	174.8959
1177	9497	11	3/49	34	29/195	197.9407	193	175.0446
1178	9511	11	59/964	34	29/195	198.0894	193	175.1934
1179	9521	11	47/768	34	29/195	198.2382	193	175.3421
1180	9533	11	38/621	34	29/195	198.3869	193	175.4908
1181	9539	11	32/523	34	29/195	198.5356	194	175.6395
1182	9547	11	26/425	34	29/195	198.6843	194	175.7882
1183	9551	11	23/376	34	29/195	198.8330	194	175.9370
1184	9587	11	43/703	34	29/195	198.9818	194	176.0857
1185	9601	11	57/932	34	29/195	199.1305	194	176.2344
1186	9613	11	17/278	34	29/195	199.2792	194	176.3831
1187	9619	11	48/785	34	29/195	199.4279	195	176.5318
1188	9623	11	59/965	34	29/195	199.5767	195	176.6806
1189	9629	11	14/229	34	29/195	199.7254	195	176.8293
1190	9631	11	39/638	34	29/195	199.8741	195	176.9780
1191	9643	11	36/589	34	29/195	200.0228	195	177.1267
1192	9649	11	11/180	34	29/195	200.1715	195	177.2754
1193	9661	11	11/180	34	29/195	200.3203	196	177.4242
1194	9677	11	41/671	34	29/195	200.4690	196	177.5729
1195	9679	11	19/311	34	29/195	200.6177	196	177.7216
1196	9689	11	46/753	34	29/195	200.7664	196	177.8703
1197	9697	11	35/573	34	29/195	200.9151	196	178.0191
1198	9719	11	59/966	34	29/195	201.0639	196	178.1678
1199	9721	11	8/131	34	29/195	201.2126	196	178.3165
1200	9733	11	8/131	34	29/195	201.3613	196	178.4652
1201	9739	11	45/737	34	29/195	201.5100	197	178.6139
1202	9743	11	29/475	34	29/195	201.6587	197	178.7627
1203	9749	11	21/344	34	29/195	201.8075	197	178.9114
1204	9767	11	47/770	34	29/195	201.9562	197	179.0601
1205	9769	11	13/213	34	29/195	202.1049	197	179.2088
1206	9781	11	44/721	34	29/195	202.2536	197	179.3575
1207	9787	11	49/803	34	29/195	202.4024	197	179.5063
1208	9791	11	59/967	34	29/195	202.5511	197	179.6550
1209	9803	11	23/377	34	29/195	202.6998	197	179.8037
1210	9811	11	28/459	34	29/195	202.8485	197	179.9524
1211	9817	11	38/623	34	29/195	202.9972	197	180.1011
1212	9829	11	53/869	34	29/195	203.1460	197	180.2499
1213	9833	11	5/82	34	29/195	203.2947	198	180.3986
1214	9839	11	5/82	34	29/195	203.4434	198	180.5473

n	p_n	$\pi(\sqrt{n})$	$\prod_{j=1}^{n}(1-1/p_j)$	$[\sqrt{n}]$	$\prod_{j=1}^{\pi(\sqrt{n})}(1-1/p_j)$	$\sum_{j=1}^{n}\prod_{i=1}^{\pi(\sqrt{j})}(1-1/p_j)$	$\pi(n)$	$n.\prod_{i=1}^{\pi(\sqrt{n})}(1-1/p_j)$
1215	9851	11	5/82	34	29/195	203.5921	198	180.6960
1216	9857	11	5/82	34	29/195	203.7408	198	180.8448
1217	9859	11	42/689	34	29/195	203.8896	199	180.9935
1218	9871	11	32/525	34	29/195	204.0383	199	181.1422
1219	9883	11	49/804	34	29/195	204.1870	199	181.2909
1220	9887	11	22/361	34	29/195	204.3357	199	181.4396
1221	9901	11	17/279	34	29/195	204.4844	199	181.5884
1222	9907	11	46/755	34	29/195	204.6332	199	181.7371
1223	9923	11	41/673	34	29/195	204.7819	200	181.8858
1224	9929	11	12/197	34	29/195	204.9306	200	182.0345
1225	9931	11	12/197	35	29/195	205.0793	200	182.1832
1226	9941	11	31/509	35	29/195	205.2281	200	182.3320
1227	9949	11	19/312	35	29/195	205.3768	200	182.4807
1228	9967	11	26/427	35	29/195	205.5255	200	182.6294
1229	9973	11	26/427	35	29/195	205.6742	201	182.7781
1230	10007	11	33/542	35	29/195	205.8229	201	182.9268
1231	10009	11	33/542	35	29/195	205.9717	202	183.0756
1232	10037	11	54/887	35	29/195	206.1204	202	183.2243
1233	10039	11	54/887	35	29/195	206.2691	202	183.3730
1234	10061	11	7/115	35	29/195	206.4178	202	183.5217
1235	10067	11	7/115	35	29/195	206.5665	202	183.6705
1236	10069	11	7/115	35	29/195	206.7153	202	183.8192
1237	10079	11	7/115	35	29/195	206.8640	203	183.9679
1238	10091	11	58/953	35	29/195	207.0127	203	184.1166
1239	10093	11	58/953	35	29/195	207.1614	203	184.2653
1240	10099	11	37/608	35	29/195	207.3101	203	184.4141
1241	10103	11	37/608	35	29/195	207.4589	203	184.5628
1242	10111	11	23/378	35	29/195	207.6076	203	184.7115
1243	10133	11	23/378	35	29/195	207.7563	203	184.8602
1244	10139	11	39/641	35	29/195	207.9050	203	185.0089
1245	10141	11	39/641	35	29/195	208.0537	203	185.1577
1246	10151	11	16/263	35	29/195	208.2025	203	185.3064
1247	10159	11	16/263	35	29/195	208.3512	203	185.4451
1248	10163	11	41/674	35	29/195	208.4999	203	185.6038
1249	10169	11	41/674	35	29/195	208.6486	204	185.7525
1250	10177	11	59/970	35	29/195	208.7974	204	185.9013
1251	10181	11	59/970	35	29/195	208.9461	204	186.0500
1252	10193	11	52/855	35	29/195	209.0948	204	186.1987
1253	10211	11	52/855	35	29/195	209.2435	204	186.3474
1254	10223	11	9/148	35	29/195	209.3922	204	186.4961
1255	10243	11	9/148	35	29/195	209.5410	204	186.6449
1256	10247	11	9/148	35	29/195	209.6897	204	186.7936
1257	10253	11	9/148	35	29/195	209.8384	204	186.9423
1258	10259	11	38/625	35	29/195	209.9871	204	187.0910
1259	10267	11	38/625	35	29/195	210.1358	205	187.2398
1260	10271	11	49/806	35	29/195	210.2846	205	187.3885
1261	10273	11	49/806	35	29/195	210.4333	205	187.5372
1262	10289	11	20/329	35	29/195	210.5820	205	187.6859
1263	10301	11	20/329	35	29/195	210.7307	205	187.8346
1264	10303	11	31/510	35	29/195	210.8794	205	187.9834
1265	10313	11	31/510	35	29/195	211.0282	205	188.1321
1266	10321	11	11/181	35	29/195	211.1769	205	188.2808
1267	10331	11	11/181	35	29/195	211.3256	205	188.4295
1268	10333	11	11/181	35	29/195	211.4743	205	188.5782
1269	10337	11	11/181	35	29/195	211.6231	205	188.7270
1270	10343	11	46/757	35	29/195	211.7718	205	188.8757
1271	10357	11	46/757	35	29/195	211.9205	205	189.0244
1272	10369	11	24/395	35	29/195	212.0692	205	189.1731
1273	10391	11	24/395	35	29/195	212.2179	205	189.3218
1274	10399	11	50/823	35	29/195	212.3667	205	189.4706
1275	10427	11	50/823	35	29/195	212.5154	205	189.6193
1276	10429	11	13/214	35	29/195	212.6641	205	189.7680
1277	10433	11	13/214	35	29/195	212.8128	206	189.9167
1278	10453	11	54/889	35	29/195	212.9615	206	190.0655
1279	10457	11	54/889	35	29/195	213.1103	207	190.2142
1280	10459	11	28/461	35	29/195	213.2590	207	190.3629
1281	10463	11	28/461	35	29/195	213.4077	207	190.5116
1282	10477	11	15/247	35	29/195	213.5564	207	190.6603
1283	10487	11	15/247	35	29/195	213.7051	208	190.8091
1284	10499	11	47/774	35	29/195	213.8539	208	190.9578
1285	10501	11	47/774	35	29/195	214.0026	208	191.1065
1286	10513	11	49/807	35	29/195	214.1513	208	191.2552
1287	10529	11	49/807	35	29/195	214.3000	208	191.4039
1288	10531	11	17/280	35	29/195	214.4488	208	191.5527
1289	10559	11	17/280	35	29/195	214.5975	209	191.7014
1290	10567	11	36/593	35	29/195	214.7462	209	191.8501
1291	10589	11	36/593	35	29/195	214.8949	210	191.9988
1292	10597	11	19/313	35	29/195	215.0436	210	192.1475

n	p_n	$\pi(\sqrt{n})$	$\prod_{j=1}^{n}(1-1/p_j)$	$[\sqrt{n}]$	$\prod_{j=1}^{\pi(\sqrt{n})}(1-1/p_j)$	$\sum_{j=1}^{n}\prod_{i=1}^{\pi(\sqrt{j})}(1-1/p_j)$	$\pi(n)$	$n.\prod_{i=1}^{\pi(\sqrt{n})}(1-1/p_j)$
1293	10601	11	19/313	35	29/195	215.1924	210	192.2963
1294	10607	11	21/346	35	29/195	215.3411	210	192.4450
1295	10613	11	21/346	35	29/195	215.4898	210	192.5937
1296	10627	11	44/725	36	29/195	215.6385	210	192.7424
1297	10631	11	44/725	36	29/195	215.7872	211	192.8912
1298	10639	11	23/379	36	29/195	215.9360	211	193.0399
1299	10651	11	23/379	36	29/195	216.0847	211	193.1886
1300	10657	11	25/412	36	29/195	216.2334	211	193.3373
1301	10663	11	25/412	36	29/195	216.3821	212	193.4860
1302	10667	11	27/445	36	29/195	216.5308	212	193.6348
1303	10687	11	27/445	36	29/195	216.6796	213	193.7835
1304	10691	11	60/989	36	29/195	216.8283	213	193.9322
1305	10709	11	60/989	36	29/195	216.9770	213	194.0809
1306	10711	11	33/544	36	29/195	217.1257	213	194.2296
1307	10723	11	33/544	36	29/195	217.2745	214	194.3784
1308	10729	11	37/610	36	29/195	217.4232	214	194.5271
1309	10733	11	37/610	36	29/195	217.5719	214	194.6758
1310	10739	11	41/676	36	29/195	217.7206	214	194.8245
1311	10753	11	41/676	36	29/195	217.8693	214	194.9732
1312	10771	11	47/775	36	29/195	218.0181	214	195.1220
1313	10781	11	47/775	36	29/195	218.1668	214	195.2707
1314	10789	11	55/907	36	29/195	218.3155	214	195.4194
1315	10799	11	55/907	36	29/195	218.4642	214	195.5681
1316	10831	11	2/33	36	29/195	218.6129	214	195.7169
1317	10837	11	2/33	36	29/195	218.7617	214	195.8656
1318	10847	11	2/33	36	29/195	218.9104	214	196.0143
1319	10853	11	2/33	36	29/195	219.0591	215	196.1630
1320	10859	11	2/33	36	29/195	219.2078	215	196.3117
1321	10861	11	2/33	36	29/195	219.3565	216	196.4605
1322	10867	11	2/33	36	29/195	219.5053	216	196.6092
1323	10883	11	2/33	36	29/195	219.6540	216	196.7579
1324	10889	11	2/33	36	29/195	219.8027	216	196.9066
1325	10891	11	2/33	36	29/195	219.9514	216	197.0553
1326	10903	11	2/33	36	29/195	220.1002	216	197.2041
1327	10909	11	2/33	36	29/195	220.2489	217	197.3528
1328	10937	11	2/33	36	29/195	220.3976	217	197.5015
1329	10939	11	2/33	36	29/195	220.5463	217	197.6502
1330	10949	11	2/33	36	29/195	220.6950	217	197.7989
1331	10957	11	2/33	36	29/195	220.8438	217	197.9477
1332	10973	11	2/33	36	29/195	220.9925	217	198.0964
1333	10979	11	2/33	36	29/195	221.1412	217	198.2451
1334	10987	11	2/33	36	29/195	221.2899	217	198.3938
1335	10993	11	2/33	36	29/195	221.4386	217	198.5426
1336	11003	11	2/33	36	29/195	221.5874	217	198.6913
1337	11027	11	2/33	36	29/195	221.7361	217	198.8400
1338	11047	11	55/908	36	29/195	221.8848	217	198.9887
1339	11057	11	55/908	36	29/195	222.0335	217	199.1374
1340	11059	11	47/776	36	29/195	222.1822	217	199.2862
1341	11069	11	47/776	36	29/195	222.3310	217	199.4349
1342	11071	11	41/677	36	29/195	222.4797	217	199.5836
1343	11083	11	41/677	36	29/195	222.6284	217	199.7323
1344	11087	11	37/611	36	29/195	222.7771	217	199.8810
1345	11093	11	37/611	36	29/195	222.9259	217	200.0298
1346	11113	11	33/545	36	29/195	223.0746	217	200.1785
1347	11117	11	33/545	36	29/195	223.2233	217	200.3272
1348	11119	11	60/991	36	29/195	223.3720	217	200.4759
1349	11131	11	31/512	36	29/195	223.5207	217	200.6246
1350	11149	11	56/925	36	29/195	223.6695	217	200.7734
1351	11159	11	56/925	36	29/195	223.8182	217	200.9221
1352	11161	11	25/413	36	29/195	223.9669	217	201.0708
1353	11171	11	52/859	36	29/195	224.1156	217	201.2195
1354	11173	11	48/793	36	29/195	224.2643	217	201.3683
1355	11177	11	48/793	36	29/195	224.4131	217	201.5170
1356	11197	11	44/727	36	29/195	224.5618	217	201.6657
1357	11213	11	23/380	36	29/195	224.7105	217	201.8144
1358	11239	11	21/347	36	29/195	224.8592	217	201.9631
1359	11243	11	21/347	36	29/195	225.0079	217	202.1119
1360	11251	11	59/975	36	29/195	225.1567	217	202.2606
1361	11257	11	59/975	36	29/195	225.3054	218	202.4093
1362	11261	11	19/314	36	29/195	225.4541	218	202.5580
1363	11273	11	19/314	36	29/195	225.6028	218	202.7067
1364	11279	11	53/876	36	29/195	225.7515	218	202.8555
1365	11287	11	53/876	36	29/195	225.9003	218	203.0042
1366	11299	11	17/281	36	29/195	226.0490	218	203.1529
1367	11311	11	17/281	36	29/195	226.1977	219	203.3016
1368	11317	11	32/529	36	29/195	226.3464	219	203.4503
1369	11321	12	32/529	37	139/958	226.4915	219	198.6332
1370	11329	12	15/248	37	139/958	226.6366	219	198.7783

n	p_n	$\pi(\sqrt{n})$	$\prod_{j=1}^{n}(1-1/p_j)$	$[\sqrt{n}]$	$\prod_{j=1}^{\pi(\sqrt{n})}(1-1/p_j)$	$\sum_{j=1}^{n}\prod_{i=1}^{\pi(\sqrt{j})}(1-1/p_j)$	$\pi(n)$	$n.\prod_{i=1}^{\pi(\sqrt{n})}(1-1/p_j)$
1371	11351	12	15/248	37	139/958	226.7817	219	198.9234
1372	11353	12	15/248	37	139/958	226.9268	219	199.0685
1373	11369	12	15/248	37	139/958	227.0719	220	199.2136
1374	11383	12	28/463	37	139/958	227.2170	220	199.3587
1375	11393	12	28/463	37	139/958	227.3621	220	199.5038
1376	11399	12	54/893	37	139/958	227.5072	220	199.6489
1377	11411	12	54/893	37	139/958	227.6523	220	199.7940
1378	11423	12	13/215	37	139/958	227.7974	220	199.9391
1379	11437	12	13/215	37	139/958	227.9425	220	200.0842
1380	11443	12	50/827	37	139/958	228.0876	220	200.2293
1381	11447	12	50/827	37	139/958	228.2327	221	200.3744
1382	11467	12	24/397	37	139/958	228.3777	221	200.5195
1383	11471	12	24/397	37	139/958	228.5228	221	200.6645
1384	11483	12	35/579	37	139/958	228.6679	221	200.8096
1385	11489	12	35/579	37	139/958	228.8130	221	200.9547
1386	11491	12	11/182	37	139/958	228.9581	221	201.0998
1387	11497	12	11/182	37	139/958	229.1032	221	201.2449
1388	11503	12	11/182	37	139/958	229.2483	221	201.3900
1389	11519	12	11/182	37	139/958	229.3934	221	201.5351
1390	11527	12	53/877	37	139/958	229.5385	221	201.6802
1391	11549	12	53/877	37	139/958	229.6836	221	201.8253
1392	11551	12	31/513	37	139/958	229.8287	221	201.9704
1393	11579	12	31/513	37	139/958	229.9738	221	202.1155
1394	11587	12	20/331	37	139/958	230.1189	221	202.2606
1395	11593	12	20/331	37	139/958	230.2640	221	202.4057
1396	11597	12	29/480	37	139/958	230.4091	221	202.5508
1397	11617	12	29/480	37	139/958	230.5542	221	202.6959
1398	11621	12	38/629	37	139/958	230.6992	221	202.8410
1399	11633	12	38/629	37	139/958	230.8443	222	202.9860
1400	11657	12	9/149	37	139/958	230.9894	222	203.1311
1401	11677	12	9/149	37	139/958	231.1345	222	203.2762
1402	11681	12	9/149	37	139/958	231.2796	222	203.4213
1403	11689	12	9/149	37	139/958	231.4247	222	203.5664
1404	11699	12	9/149	37	139/958	231.5698	222	203.7115
1405	11701	12	9/149	37	139/958	231.7149	222	203.8566
1406	11717	12	34/563	37	139/958	231.8600	222	204.0017
1407	11719	12	43/712	37	139/958	232.0051	222	204.1468
1408	11731	12	25/414	37	139/958	232.1502	222	204.2919
1409	11743	12	25/414	37	139/958	232.2953	223	204.4370
1410	11777	12	57/944	37	139/958	232.4404	223	204.5821
1411	11779	12	57/944	37	139/958	232.5855	223	204.7272
1412	11783	12	16/265	37	139/958	232.7306	223	204.8723
1413	11789	12	16/265	37	139/958	232.8756	223	205.0174
1414	11801	12	39/646	37	139/958	233.0207	223	205.1624
1415	11807	12	39/646	37	139/958	233.1658	223	205.3075
1416	11813	12	23/381	37	139/958	233.3109	223	205.4526
1417	11821	12	23/381	37	139/958	233.4560	223	205.5977
1418	11827	12	30/497	37	139/958	233.6011	223	205.7428
1419	11831	12	30/497	37	139/958	233.7462	223	205.8879
1420	11833	12	44/729	37	139/958	233.8913	223	206.0330
1421	11839	12	44/729	37	139/958	234.0364	223	206.1781
1422	11863	12	7/116	37	139/958	234.1815	223	206.3232
1423	11867	12	7/116	37	139/958	234.3266	224	206.4683
1424	11887	12	7/116	37	139/958	234.4717	224	206.6134
1425	11897	12	7/116	37	139/958	234.6168	224	206.7585
1426	11903	12	7/116	37	139/958	234.7619	224	206.9036
1427	11909	12	7/116	37	139/958	234.9070	225	207.0487
1428	11923	12	54/895	37	139/958	235.0521	225	207.1938
1429	11927	12	54/895	37	139/958	235.1971	226	207.3389
1430	11933	12	33/547	37	139/958	235.3422	226	207.4839
1431	11939	12	40/663	37	139/958	235.4873	226	207.6290
1432	11941	12	26/431	37	139/958	235.6324	226	207.7741
1433	11953	12	26/431	37	139/958	235.7775	227	207.9192
1434	11959	12	45/746	37	139/958	235.9226	227	208.0643
1435	11969	12	45/746	37	139/958	236.0677	227	208.2094
1436	11971	12	19/315	37	139/958	236.2128	227	208.3545
1437	11981	12	19/315	37	139/958	236.3579	227	208.4996
1438	11987	12	31/514	37	139/958	236.5030	227	208.6447
1439	12007	12	31/514	37	139/958	236.6481	228	208.7898
1440	12011	12	12/199	37	139/958	236.7932	228	208.9349
1441	12037	12	12/199	37	139/958	236.9383	228	209.0800
1442	12041	12	12/199	37	139/958	237.0834	228	209.2251
1443	12043	12	12/199	37	139/958	237.2285	228	209.3702
1444	12049	12	53/879	38	139/958	237.3736	228	209.5153
1445	12071	12	53/879	38	139/958	237.5186	228	209.6604
1446	12073	12	29/481	38	139/958	237.6637	228	209.8054
1447	12097	12	29/481	38	139/958	237.8088	229	209.9505
1448	12101	12	17/282	38	139/958	237.9539	229	210.0956

n	p_n	$\pi(\sqrt{n})$	$\prod_{j=1}^{n}(1-1/p_j)$	$[\sqrt{n}]$	$\prod_{j=1}^{\pi(\sqrt{n})}(1-1/p_j)$	$\sum_{j=1}^{n}\prod_{i=1}^{\pi(\sqrt{j})}(1-1/p_j)$	$\pi(n)$	$n.\prod_{i=1}^{\pi(\sqrt{n})}(1-1/p_j)$
1449	12107	12	17/282	38	139/958	238.0990	229	210.2407
1450	12109	12	56/929	38	139/958	238.2441	229	210.3858
1451	12113	12	17/282	38	139/958	238.3892	230	210.5309
1452	12119	12	22/365	38	139/958	238.5343	230	210.6760
1453	12143	12	22/365	38	139/958	238.6794	231	210.8211
1454	12149	12	49/813	38	139/958	238.8245	231	210.9662
1455	12157	12	49/813	38	139/958	238.9696	231	211.1113
1456	12161	12	59/979	38	139/958	239.1147	231	211.2564
1457	12163	12	59/979	38	139/958	239.2598	231	211.4015
1458	12197	12	37/614	38	139/958	239.4049	231	211.5466
1459	12203	12	37/614	38	139/958	239.5500	232	211.6917
1460	12211	12	47/780	38	139/958	239.6951	232	211.8368
1461	12227	12	47/780	38	139/958	239.8401	232	211.9819
1462	12239	12	5/83	38	139/958	239.9852	232	212.1269
1463	12241	12	5/83	38	139/958	240.1303	232	212.2720
1464	12251	12	5/83	38	139/958	240.2754	232	212.4171
1465	12253	12	5/83	38	139/958	240.4205	232	212.5622
1466	12263	12	5/83	38	139/958	240.5656	232	212.7073
1467	12269	12	5/83	38	139/958	240.7107	232	212.8524
1468	12277	12	5/83	38	139/958	240.8558	232	212.9975
1469	12281	12	5/83	38	139/958	241.0009	232	213.1426
1470	12289	12	5/83	38	139/958	241.1460	232	213.2877
1471	12301	12	5/83	38	139/958	241.2911	233	213.4328
1472	12323	12	48/797	38	139/958	241.4362	233	213.5779
1473	12329	12	53/880	38	139/958	241.5813	233	213.7230
1474	12343	12	38/631	38	139/958	241.7264	233	213.8681
1475	12347	12	38/631	38	139/958	241.8715	233	214.0132
1476	12373	12	28/465	38	139/958	242.0166	233	214.1583
1477	12377	12	28/465	38	139/958	242.1616	233	214.3034
1478	12379	12	51/847	38	139/958	242.3067	233	214.4484
1479	12391	12	51/847	38	139/958	242.4518	233	214.5935
1480	12401	12	41/681	38	139/958	242.5969	233	214.7386
1481	12409	12	23/382	38	139/958	242.7420	234	214.8837
1482	12413	12	18/299	38	139/958	242.8871	234	215.0288
1483	12421	12	18/299	38	139/958	243.0322	235	215.1739
1484	12433	12	49/814	38	139/958	243.1773	235	215.3190
1485	12437	12	49/814	38	139/958	243.3224	235	215.4641
1486	12451	12	44/731	38	139/958	243.4675	235	215.6092
1487	12457	12	31/515	38	139/958	243.6126	236	215.7543
1488	12473	12	13/216	38	139/958	243.7577	236	215.8994
1489	12479	12	13/216	38	139/958	243.9028	237	216.0445
1490	12487	12	13/216	38	139/958	244.0479	237	216.1896
1491	12491	12	13/216	38	139/958	244.1930	237	216.3347
1492	12497	12	34/565	38	139/958	244.3380	237	216.4798
1493	12503	12	47/781	38	139/958	244.4831	238	216.6248
1494	12511	12	21/349	38	139/958	244.6282	238	216.7699
1495	12517	12	21/349	38	139/958	244.7733	238	216.9150
1496	12527	12	50/831	38	139/958	244.9184	238	217.0601
1497	12539	12	50/831	38	139/958	245.0635	238	217.2052
1498	12541	12	37/615	38	139/958	245.2086	238	217.3503
1499	12547	12	37/615	38	139/958	245.3537	239	217.4954
1500	12553	12	53/881	38	139/958	245.4988	239	217.6405
								E & OE

Fig.1: The above table compares values for $\pi(n)$ as approximated non-heuristically by $\pi_L(n) = \sum_{j=1}^{n}\prod_{i=1}^{\pi(\sqrt{j})}(1-1/p_j)$, the actual values $\pi(n)$ of the primes less than or equal to n, and the values for $\pi(n)$ as estimated non-heuristically by $\pi_H(n) = n.\prod_{i=1}^{\pi(\sqrt{n})}(1-1/p_j)$ of $\pi(n)$, for $4 \leq n \leq 1500$.[1]

42.1. Error between actual and expected primes

Observation and analysis of the error between the actual (*Act p*) number, $\pi(p_{n+1}^2) - \pi(p_n^2)$, of primes in the interval (*Int*) (p_n^2, p_{n+1}^2), and the non-heuristically expected (*Exp p*) number, $\pi_L(p_{n+1}^2) - \pi_L(p_n^2)$, of primes in the interval (*Int*) (p_n^2, p_{n+1}^2) for $1 \leq n \leq 11$ raises the following query:

Query 42.1. Does the ratio:

[1] The downloadable .xlxs source file is accessible here.

$$\mathbb{R} = \frac{Cum\ SD}{Cum\ Exp\ p} = \frac{Cumulative\ standard\ deviation\ of\ the\ cumulative\ sum\ of\ expected\ primes\ in\ the\ interval\ (4,\ p^2_{n+1})}{Cumulative\ sum\ \pi_L(p^2_{n+1}) = \sum_{j=1}^{p^2_{n+1}} \prod_{i=1}^{\pi(\sqrt{j})}(1-1/p_j)\ of\ expected\ primes\ in\ the\ interval\ (4,\ p^2_{n+1})}$$

tend to a limit?

Fig.2: Ratio for $\pi_L(p^2_{n+1})$ of $Cum\ SD/Cum\ Exp\ p = \sum_{i=1}^{n}$ Standard Deviation in $(p^2_i, p^2_{i+1})/\sum_{i=1}^{n}$ Expected Primes in (p^2_i, p^2_{i+1})

n	*Interval* $p^2_n - p^2_{n+1}$	*Int Size*	*Int Act p*	*Int Exp p*	*Int Error*	*Cum Act p*	*Cum Exp p*	*Cum Error*	% *Error*	*Int density* $\prod_1^n(1-\frac{1}{p_i})$	*Int SD*	*Cum SD*	*Ratio* $\mathbb{R}$
1	$4-9$	5	2	1.6	0.4000	2	1.6	0.4000	20.00	0.3333	1.0541	1.0541	0.6588
2	$9-25$	16	5	4.2	0.7714	7	5.8	1.1714	16.73	0.2667	1.7689	2.8230	0.4843
3	$25-49$	24	6	5.5	0.5351	13	11.3	1.7065	13.13	0.2286	2.0571	4.8801	0.4321
4	$49-121$	72	15	14.9	0.0549	28	26.2	1.7614	6.29	0.2078	3.4427	8.3228	0.3172
5	$121-169$	48	9	9.2	-0.1955	37	35.4	1.5659	4.23	0.1918	2.7278	11.0506	0.3119
6	$169-289$	120	22	21.7	0.3465	59	57.1	1.9124	3.24	0.1805	4.2133	15.2640	0.2674
7	$289-361$	72	11	12.3	-1.3063	70	69.4	0.6061	0.87	0.1710	3.1950	18.4589	0.2660
8	$361-529$	168	29	27.5	1.5228	99	96.9	2.1289	2.15	0.1636	4.7945	23.2534	0.2400
9	$529-841$	312	47	49.3	-2.2745	146	146.1	-0.1456	-0.10	0.1579	6.4417	29.6951	0.2032
10	$841-961$	120	16	18.4	-2.3381	162	164.5	-2.4837	-1.53	0.1529	3.9419	33.6371	0.2045
11	$961-1369$	408	57	60.7	-3.6745	219	225.2	-6.1582	-2.81	0.1487	7.1870	40.8241	0.1813
12	$1369-1500$	131	20	19.0	0.9927	239	244.2	-5.1655	-2.16	0.1451	4.0311	44.8551	0.1837
													E & OE

Fig.2: The above table compares values of the ratio for $\pi_L(p^2_{n+1})$ of $Cum\ SD/Cum\ Exp\ p$ $= \sum_{i=1}^{n}$ Standard Deviation in $(p^2_i, p^2_{i+1})/\sum_{i=1}^{n}$ Expected Primes in (p^2_i, p^2_{i+1}).

Part 11

The significance of *evidence-based* reasoning for Cognitive Science

CHAPTER 43

Mathematical idea analysis

In their compelling narrative *Where Mathematics Comes From* ([**LR00**]), cognitive scientists Lakoff and Núñez attempt to address the nature of what is commonly accepted as the body of knowledge intuitively viewed as the domain of *abstract* mathematical ideas, by introducing the concept of *mathematical idea analysis* and enquiring:

QUERY 43.1. How can cognitive science bring systematic *scientific rigor* to the realm of human mathematical ideas, which lies outside the rigor of mathematics itself?

Lakoff and Núñez argue that:

- Mathematics needs to be understood from a cognitive perspective;
- Mathematics is the epitome of precision;
- Intellectual content of mathematics lies in its ideas, not symbols;
- Formal symbols merely characterise the nature and structure of mathematical ideas;
- Human ideas are grounded in sensory-motor mechanisms;
- Abstract human ideas make use of precisely formulatable cognitive mechanisms such as conceptual metaphors that import modes of reasoning from sensory-motor experience;
- It is *always* an empirical question what human ideas are like, mathematical or not.

They specifically attempt to address the issues:

- How can human beings understand the idea of actual infinity?
- Where do the laws of mathematics come from?
- Why does every proposition follow from a contradiction?

They argue that this involves a prior understanding of:

- Basic cognitive semantics;
- Understanding the cognitive structure of mathematics.

Mathematical idea analysis: Lakoff and Núñez' cognitive perspective

"We are cognitive scientists—a linguist and a psychologist—each with a long-standing passion for the beautiful ideas of mathematics. As specialists within a field that studies the nature and structure of ideas, we realized that despite the remarkable advances in cognitive science and a long tradition in philosophy and history, there was still no discipline of *mathematical idea analysis* from a cognitive perspective—no cognitive science of mathematics. ...

A discipline of this sort is needed for a simple reason. Mathematics is deep, fundamental, and essential to the human experience. As such, it is crying out to be understood.

It has not been.

Mathematics is seen as the epitome of precision, manifested in the use of symbols in calculation and in formal proofs. Symbols are, of course, just symbols, not ideas. The intellectual content of mathematics lies in its ideas, not in the symbols themselves. In short, the intellectual content of mathematics does not lie where the mathematical rigor can be most easily seen—namely, in the symbols. Rather, it lies in human ideas.

But mathematics by itself does not and cannot *empirically* study human ideas; human cognition is simply not its subject matter. It is up to cognitive science and the neurosciences to do what mathematics itself cannot do—namely apply the science of mind to human mathematical ideas. ...

One might think that the nature of mathematical ideas is a simple and obvious matter, that such ideas are just what mathematicians have consciously taken them to be. From that perspective, the commonplace formal symbols do as good a job as any at characterizing the nature and structure of those ideas. If that were true, nothing more would need to be said.

But those of us who study the nature of concepts within cognitive science know, from research in the field, that the study of human ideas is not so simple. Human ideas are, to a large extent, grounded in sensory-motor experience. Abstract human ideas make use of precisely formulatable cognitive mechanisms such as conceptual metaphors that import modes of reasoning from sensory-motor experience. It is *always* an empirical question what human ideas are like, mathematical or not.

The central question we ask is this: How can cognitive science bring systematic *scientific rigor* to the realm of human mathematical ideas, which lies outside the rigor of mathematics itself? Our job is to help make precise what mathematics itself cannot—the nature of mathematical ideas."

... Lakoff and Núñez: [**LR00**]*, Preface, pp.xi-xii.*

Now, prima facie such a perspective faces a number of philosophical and mathematical challenges from *evidence-based* reasoning. For instance:

- "*The intellectual content of mathematics lies in its ideas, not in the symbols themselves.*"

  As compared to the *evidence-based* perspective of this investigation that mathematics is a set of formal languages (as detailed in §21.4; also Chapter 23), what is the concept of 'mathematics' that Lakoff and Núñez have in mind? What is the assurance that both authors are referring to the same concept? To what does 'its' refer?

- "*In short, the intellectual content of mathematics does not lie where the mathematical rigor can be most easily seen—namely, in the symbols. Rather, it lies in human ideas.*"

  To what does the expression 'human ideas' refer in this context? From the *evidence-based* perspective of this investigation, are what Lakoff and

Núñez refer to as 'human ideas' here conceptual metaphors that ought to be treated as Carnap's *explicandum* (see Chapter 14); or ought they to be treated, classically, as what mathematicians would refer to as the interpretations of a formal mathematical language—over the domain in which the metaphors are formulated or defined—in Tarski's sense (as detailed in Chapter 6)?

We note that this domain can also, again not unreasonably, be taken to be that of an informal interpretation of the first-order set theory ZFC over Lakoff and Núñez's conceptual metaphors, since a tacit thesis of this investigation (Thesis 44.1) is that their analysis establishes that all the abstract mathematical concepts dissected in Chapters 5 to 14 of[**LR00**]—including concepts involving 'potential' and 'actual' infinities—can be viewed as conceptual metaphors which are expressible (if treated as Carnap's *explicandum*) in the language of the first-order Set Theory ZFC; a perspective that would lend legitimacy to conventional wisdom which—as detailed in Chapter 18 (see also [**Ma18**])—is that all mathematical concepts are definable in ZFC.

- "*...human cognition is simply not its subject matter.*"

  What can the term 'mathematics' refer to in this context? Would the authors accept that 'mathematics' is a set of formal, symbolic, languages? If so, how can a language per se have a subject matter?

- "*It is up to cognitive science and the neurosciences to do what mathematics itself cannot do—namely apply the science of mind to human mathematical ideas.*"

  Do the authors mean ideas about the interpretations of mathematical symbols, or ideas expressible in mathematical symbols (where we would take the former to be the conceptual metaphors by which we intend to represent our sensory perceptions in a language)?

- "*One might think that the nature of mathematical ideas is a simple and obvious matter, that such ideas are just what mathematicians have consciously taken them to be.*"

  Which mathematicians?

  – Those (see §3.1) who believe—without evidence—both that first-order logic is consistent, and that Hilbert's formal, ε-based, definitions of quantification will not lead to a fatal mathematical contradiction?

  – Or those (see §3.2) who—again without evidence—do not accept first-order logic as consistent (since they deny the Law of the Excluded Middle), whilst following Brouwer in denying legitimacy to Hilbert's formal definitions of quantification in mathematical reasoning?

    * The former treat mathematical reasoning as manipulation of a selected, finite, set of identifiable symbols into patterns (termed 'proofs') obeying a well-defined set of finitary rules, without requiring the symbols or patterns to be necessarily associated with any meaning (interpretation). Mathematical ideas to them are precisely the formal properties of, and inter-relations

between, such patterns. They do not need an interpretation into a non-symbolic universe.

* The latter treat mathematical reasoning as representing statements that can be interpreted as either 'true' or "false' with reference to *evidence-based* properties of objects in the physical universe.

- "*It is always an empirical question what human ideas are like, mathematical or not.*"

  Does this mean that, for Lakoff and Núñez, ideas can be mathematical or not? If so, what would be a non-mathematical idea? Could an idea expressed in English be termed as an 'English' idea?

- "*Our job is to help make precise what mathematics itself cannot—the nature of mathematical ideas.*"

  Would this not implicitly imply that ideas can exist in a Platonic universe of ideas?

Thus, from the *evidence-based* perspective of this investigation, it would seem that Lakoff and Núñez unwittingly conflate the use of the term 'mathematics' when referring to a set of formal, symbolic, languages[1] (in the sense of §21.4), with what is intended to be expressed or represented in such languages.

The distinction may be significant for Lakoff and Núñez's *mathematical idea analysis*, especially if the goal of such analysis is 'to provide a new level of understanding in mathematics'.

43.1. Extending Lakoff and Núñez's intent on 'understanding'

> " The purpose of of mathematical idea analysis is to provide a new level of understanding in mathematics. It seeks to explain *why* theorems are true on the basis of what they mean. It asks what ideas—especially what metaphorical ideas—are built into axioms and definitions. It asks what ideas are implicit in equations and how *ideas* can be expressed by mere numbers. And finally it asks what is the ultimate grounding of each complex idea. That, as we shall see, may require some complicated analysis:
>
> 1. tracing through a complex mathematical idea network to see what the ultimate grounding metaphors in the network are;
> 2. isolating the linking metaphors to see how basic grounded ideas are linked together;
> 3. figuring out how the immediate understanding provided by the individual grounding metaphors permits one to comprehend thye complex idea as a whole."
>
> *... Lakoff and Núñez:* [**LR00**]*, Chapter 15, p.338.*

However, in this informal interpretation of Lakoff and Núñez's argumentation, we shall ignore such pedantries and, without engaging in technical niceties regarding cognition and cognitive semantics[2], for the purposes of this investigation attempt to informally extend Lakoff and Núñez's intent on the nature of *understanding* by

[1]Surprisingly, the word 'language' is indexed as occurring only on 5 pages in the book!

[2]For a critical review of Lakoff and Núñez's concept of *mathematical idea analysis* from a cognitive perspective see [**Md01**].

an individual mind[3] of a concept created in the mind by differentiating as below (compare §23.2 in Chapter 23):

(a) *Subjective understanding*: which we view as an individual mind's perspective involving pattern recognition of a selected set of truth assignments by the individual to declarative sentences of a symbolic language, based on the individual's *uncritical personal* beliefs of a correspondence between:

   – what is *believed* as true (as reflected by the truth assignments); and

   – what is perceived or pronounced as 'factual' (reflecting *uncritical* conclusions drawn from individual cognitive experience) in a common external world;

(b) *Projective understanding*: which we view as an individual mind's perspective involving pattern recognition of a selected set of truth assignments by the individual to declarative sentences of a symbolic language, based on the individual's *critical plausible* belief of a correspondence between:

   – what is *assumed*, or *postulated*, as true (as reflected by the truth assignments); and

   – what is perceived or projected as 'factual' (reflecting *plausible* conclusions drawn from individual cognitive experience) in a common external world;

(c) *Collaborative (objective) understanding*: which we view as an individual mind's perspective involving pattern recognition of a selected set of truth assignments by the individual to declarative sentences of a symbolic language, based on the individual's *shared evidence-based* belief of a correspondence between:

   – what is *accepted by convention* as true (as reflected by *evidence-based* truth assignments—such as those in Chapter 7, Chapter 8, and Chapter 9); and

   – what is perceived or conjectured as 'factual' (reflecting shared *evidence-based* cognitive experiences) in a common external world.

In other words, from an *evidence-based* perspective, the 'understanding' of an abstract mental concept—whether *subjective*, *projective*, or *collaborative*—is not limited, as Lakoff and Núñez appear to suggest, in merely identifying the conceptual metaphors that are used to describe the concept within a language; it must encompass, further, awareness of the *evidence-based* assignments of truth values to the declarative sentences of the language—in which the conceptual metaphors are expressed—that correspond, or are believed to correspond, to what is perceived or conjectured as 'factual' cognitive experiences in a common external world.

From the perspective of Information Theory, the distinction sought to be made here may be broadly viewed as that drawn by Björn Lundgren between 'the property of being information and the property of being informative':

[3]Although Lakoff and Núñez restrict their considerations to the sensory perceptions of the human mind, we shall assume that their findings and conclusions would apply to the sensory perceptions of any intelligence that is capable of creating a mechanical intelligence which can reason as detailed in [**An16**].

> "Ever since Luciano Floridi re-invigorated the veridicality thesis (that [semantic] information must be true, or truthful), the discussion of this issue has been expanding (see Floridi 2004, 2005; cf. Fetzer 2004; Dodig-Crnkovic 2005). Although Floridi claims that various critical comments have "been proved unjustified, and as a result, there is now a growing consensus" about his approach (Floridi 2012, p. 432, footnotes removed), the discussion has continued. Recently, I argued that Floridi's proposed definitions suffer from counter-examples such that *the sentence x is information if, and only if, x is not information* (see Lundgren 2015a). The same idea was later developed and expanded by Macaulay Ferguson (2015), who furthermore argues that the choice of the definition of semantic information (between a veridical and an alethically neutral conception) is a dilemma because it is a choice between two paradoxes: information liar paradoxes and the Bar-Hillel Carnap paradox (BCP); both will be explained in this paper. This dilemma will serve as part of the dialectics of this essay.
>
> The main aim of this essay is to argue for an alethically neutral conception of semantic information. This argument will be made by presenting counter-arguments against Floridi's main arguments for the veridicality thesis, as well as showing that a veridical conception of semantic information leads to a contradiction. I consider Floridi's arguments because he is currently the most influential proponent of the veridicality thesis and of a semantic conception of information. The main contribution of this essay is that an alethically neutral conception of semantic information can avoid the BCP, thus resolving the supposed dilemma between alethically neutral and veridical conceptions of semantic information. This is done by introducing a distinction between the property of being information and the property of being informative. Overall, combined with the other arguments, this speaks in favor of an alethically neutral conception of semantic information and against the veridicality thesis.
>
> However, a preference for an alethically neutral conception over a veridical conception of semantic information does not mean that we cannot, or should not, retain the latter concept. I conclude that we should retain it as a subconcept of the former concept, i.e., as *veridical semantic information*."
>
> *... Lundgren:* [**Lun17**]*, p.2.*

Accordingly, we shall treat Lakoff and Núñez's *mathematical ideas* to refer not to some putative *content* of some abstract structure, conceived by an individual mind in a platonic domain of *ideas* some of which can be termed as of a mathematical nature, but to the pattern recognition of some selected set of 'truth' assignments to (presumed faithful[4]) representations—of conceptual metaphors grounded in sensory motor perceptions—by an individual mind in an artificially constructed symbolic language that can be termed as 'mathematical'.

'Mathematical' in the sense that the language—in sharp contrast to languages of common discourse, which embrace ambiguity as essential for capturing and expressing the full gamut of any cognitive experience of our common external world[5]—is designed to facilitate unambiguous pattern recognition of a narrowly

[4]By some effective procedure such as, for example, Tarski's inductive definitions of the satisfiability and truth of the formulas of a formal mathematical language under a Tarskian interpretation (as detailed in Chapter 6).

[5]The absurd extent to which languages of common discourse need to tolerate ambiguity; both for ease of expression and for practical—even if not theoretically unambiguous and effective—communication in non-critical cases amongst intelligences capable of a lingua franca, is briefly addressed in Chapter 24.

selected aspect of a cognitive experience[6]—and its effective communication to another mind—between the limited perception which was sought to be represented, and its representation at any future recall.

This reflects the underlying thesis of this investigation that (see §21.4; also Chapter 23):

(i) Mathematics is to be considered as a set of precise, symbolic, languages.

(ii) Any language of such a set, say the first order Peano Arithmetic PA (or Russell and Whitehead's PM in Principia Mathematica, or the Set Theory ZF), is intended to express—in a finite, unambiguous, and communicable manner—relations between elements that are external to the language PA (or to PM, or to ZF).

(iii) Moreover, each such language is two-valued if we assume that a specific relation either holds or does not hold externally under any valid interpretation of the language.

43.2. How can human beings understand the idea of actual infinity?

Lakoff and Núñez's lack of an unambiguous perspective towards their use of the term 'mathematics' is also reflected in their analysis of how human beings understand the idea of actual infinity from a cognitive perspective:

> **How can human beings understand the idea of actual infinity?**
>
> ". . . Núñez had begun an intellectual quest to answer these questions: How can human beings understand the idea of actual infinity?—infinity conceptualized as a thing, not merely as an unending process? What is the concept of actual infinity in its mathematical manifestations—points at infinity, infinite sets, infinite decimals, infinite intersections, transfinite numbers, infinitesimals? He reasoned that since we do not encounter actual infinity directly in the world, since our conceptual systems are finite, and since we have no cognitive mechanisms to perceive infinity, there is a good possibility that metaphorical thought may be necessary for human beings to conceptualize infinity. If so, new results about the structure of metaphorical concepts might make it possible to precisely characterize the metaphors used in mathematical concepts of infinity.
>
> . . . We soon realized that such a question could not be answered in isolation. We would need to develop enough of the foundations of mathematical idea analysis so that the question could be asked and answered in a precise way. We would need to understand the cognitive structure not only of basic arithmetic but also of symbolic logic, the Boolean logic of classes, set theory, parts of algebra, and a fair amount of classical mathematics: analytic geometry, trigonometry, calculus, and complex numbers. That would be a task of many lifetimes. . . .
>
> So we adopted an alternative strategy. We asked, What would be the minimum background needed
>
> - to answer Núñez's questions about infinity,
> - to provide a serious beginning for a discipline of mathematical idea analysis, . . .

[6]Compare this with Löb's remarks that: "While classical mathematics owes its development to a naive meta-physical conception of the physical world, from the constructivist point of view mathematics may rather be regarded to be an abstract reconstruction of a private phenomenological world." [**Lob59**], p.164.

> As a consequence, our discussion of arithmetic, set theory, logic, and algebra are just enough to set the stage for our subsequent discussions of infinity and classical mathematics. just enough for that job, but not trivial ...
>
> *... Lakoff and Núñez:* [**LR00**], *Preface, p.xii-p.xiii.*

> And as we shall see, Núñez was right about the centrality of conceptual metaphor to a full understanding of infinity in mathematics. There are two infinity concepts in mathematics—one literal and one metaphorical. The literal concept ("in-finity"—lack of an end) is called "potential infinity". It is simply a process that goes on without end, like counting without stopping, extending a line segment indefinitely, or creating polygons with more and more sides. No metaphorical ideas are needed in this case. Potential infinity is a useful notion in mathematics, but the main event is elsewhere. The idea of "actual infinity," where infinity becomes a *thing*—an infinite set, a point at infinity, a transfinite number, the sum of an infinite series—is what is really important. Actual infinity is fundamentally a metaphorical idea, just as Núñez had suspected. The surprise for us was that *all* forms of actual infinity—points at infinity, infinite intersections, transfinite numbers, and so on—appear to be special cases of just one Basic Metaphor of Infinity. This is anything but obvious. ..."
>
> *... Lakoff and Núñez:* [**LR00**], *Preface, p.xvi.*

From the *evidence-based* perspective of this investigation, however, it is precisely because 'we do not encounter actual infinity directly', and 'since we have no cognitive mechanisms to perceive infinity', that mathematicians classically—following Hilbert—postulate an 'idealised' existence for such a concept by means of a—not necessarily *evidence-based*—'definitional' axiom in the sense of Weyl's 'implicit definition' (see §21.17) and then create symbols such as $\infty, \omega, \aleph$, etc., in a purely artificial mathematical universe.

The subjective—and arbitrary—postulational character of such axioms becomes evident if we view axioms not as implicit or explicit definitions, but as part of the rules of the logic that, reasonably, *seeks* to assign *unambiguous* truth values to the well-formed formulas of a language as proposed by Definitions 21.3, 21.4 and 21.5 in §21.2.

As further expressed by Weyl from an early-intuitionistic point of view:

> "An arithmetical construction of geometry that respects the logical content of the geometric axioms is clearly a significant step toward a system of concepts explicitly defined on the basis of purely logical concepts. This quest to logicize mathematics gains further ground in the well-known theory of the irrationals due to Cantor, Dedekind, and Weierstrass in which the concept of the real numbers is reduced to that of the rational and, eventually, the natural numbers $1, 2, 3, \ldots$. But the work of Dedekind and Cantor showed that the natural numbers and the associated operations of addition, multiplication, etc. are based on a discipline exceedingly close to pure logic: Cantor's set theory. So we now consider set theory to be, from a logical standpoint, the genuine foundation of the mathematical sciences and, hence, we must turn to it if we wish to formulate principles of definition that suffice, not just for elementary geometry, but for mathematics as a whole.
>
> Now, however, suspicions having been aroused by some contradictions (real or imagined), there is a clash of contrary opinions about the fundamental questions of set theory. In discussions of these questions, logico-mathematical and psychological points of view have often been mixed together.
>
> In the development of the human intellect (*Geist*), the concept of set and number has passed through distinct stages. At the first stage, an actual

aggregation (*eigentliche Inbegriffsvorstellung*) occurs when a unitary interest draws from the content of our consciousness the perceptions (*Vorstellungen*) of several separately observed (*für sich bemerkter*) objects and unites them. At this stage, the earliest numerals (e.g., 2, 3, and 4) designate immediately observable differentiations of the psychic act operating in the aggregation.

At the second stage, symbolic representations replace actual perceptions (*tretenfür die eigentlichen Vorstellungen symbolische ein*). The most significant product of this second period is the well-known symbolic procedure of counting, familiar to every child, through which sets (and not just the smallest) can be distinguished in terms of their cardinal number. Here a certain feeling for the possible is one of the essential formative elements. In our effort to cope with the external world, we do not feel constrained by the accidental limitations and shortcomings of our sense organs and cognitive faculties. Cantor's introduction of his transfinite ordinals (an innovation motivated by the iterated formation of derived point-sets) perfectly illustrates the procedure characteristic of this second stage. Cantor placed a new element ω after the series $1, 2, 3, \ldots$ and conceived the progressive extension of the domain of numbers as follows:

$$\begin{gathered} 1, 2, 3, \ldots \\ \omega, \omega+1, \omega+2, \ldots \\ (\omega 2), (\omega 2)+1, (\omega 2)+2, \ldots \\ \ldots\ldots\ldots \\ \omega^2, \omega^2+1, \omega^2+2, \ldots \\ \omega^2+\omega \\ \ldots\ldots\ldots \\ \omega^3, \ldots \\ \ldots\ldots\ldots \\ \omega^\omega, \ldots \\ \ldots\ldots\ldots \end{gathered}$$

An actual perception of infinite sets—in the sense that their individual elements are simultaneously present as separately observed contents in our consciousness—is unattainable. It does not follow, though, that infinite sets are logically illegitimate. After all, an actual presentation to consciousness of a set with a large number of elements can be unattainable even when the set is finite. So it is true that "there is no actual infinity" only in the sense that the actual presence to consciousness of infinite manifolds is impossible."
... Weyl: [**We10**], *pp.6-7.*

It is thus the axioms themselves that are, then, the conceptual metaphors for the symbols that are intended to represent the postulated Platonic entities. In the absence of *evidence-based* conventions, the symbols not only have no physical significance—as Weyl seeks to convey—but, as the examples in §24.3 have shown, they can be misleading as to the actual behaviour of physical systems in the limiting cases which are sought to be adequately expressed and unambiguously communicated in a mathematical language.

43.3. What does a mathematical representation reflect?

Nevertheless, the significance for *evidence-based* reasoning of Lakoff and Núñez's analysis of those conceptual metaphors which are most appropriately represented in a mathematical language, lies in their conclusion that all representations of physical phenomena in a mathematical language are ultimately grounded not in any 'abstract, transcendent', genetically inherited, knowledge, but in conceptual metaphors that import modes of reasoning reflecting, and endemic to, human sensory-motor-experience.

What do the mathematical representations of the laws of arithmetic reflect?

"... We seek, *from a cognitive perspective*, to provide answers to such questions as, Where do the laws of arithmetic come from?[7] Why is there a unique empty class and why is it a subclass of all classes? Indeed why, in formal logic, does every proposition follow from a contradiction? Why should anything at all follow from a contradiction?[8]

From a cognitive perspective, these questions cannot be answered merely by giving definitions, axioms, and formal proofs. That just pushes the question one step further back. How are those definitions and axioms understood? To answer questions at this level requires an account of ideas and cognitive mechanisms. Formal definitions and axioms are *not* basic cognitive mechanisms; indeed, they themselves require an account in cognitive terms.

One might think that the best way to understand mathematical ideas would be simply to ask mathematicians what they are thinking. Indeed, many famous mathematicians, such as Descartes, Boole, Dedekind, Poincaré, Cantor, and Weyl, applied this method to themselves, introspecting about their own thoughts. Contemporary research on the mind shows that as valuable as this can be, it can at best tell a partial and not fully accurate story. Most of our thoughts and our system of concepts are part of the cognitive unconscious ... We human beings have no direct access to our deepest forms of understanding. The analytic techniques of cognitive science are necessary if we are to understand how we understand.

But the more we have applied what we know about cognitive science to understand the cognitive structure of mathematics, the more it has become clear that this romance cannot be true. Human mathematics, the only kind of mathematics that human beings know, cannot be a subspecies of an abstract, transcendent mathematics. Instead, it appears that mathematics as we know it arises from the nature of our brains and our embodied experience. As a consequence, *every* part of the romance appears to be false, for reasons that we will be discussing.

Perhaps most surprising of all, we have discovered that a great many of the most fundamental mathematical ideas are inherently metaphorical in nature:

- The *number line*, where numbers are conceptualized metaphorically as points on a line.
- Boole's *algebra of classes*, where the formation of classes of objects is conceptualized metaphorically in terms of algebraic operations and elements: plus, times, zero, one, and so on.
- *Symbolic logic*, where reasoning is conceptualized metaphorically as mathematical calculation using symbols.
- *Trignometric functions*, where angles are conceptualized metaphorically as numbers.
- The *complex plane*, where multiplication is conceptualized metaphorically in terms of rotation.

[7]From an *evidence-based* perspective, the 'laws' of a mathematical language (i.e., the axioms and rules of inference) are the 'logical' conventions (in the sense of §21.2) that assign veridicality to mathematical assertions purporting to adequately express and unambiguously communicate properties about objects in the real world that are accessible to our senses.

[8]From an evidence-based perspective, 'logic' is purely a convention that, in the sense of §21.2, artificially 'completes' the world of facts by adding non-facts (in the sense of §44.3(e)).

> . . . None of what we have discovered is obvious. Moreover, it requires a prior understanding of a fair amount of basic cognitive semantics and of the overall cognitive structure of mathematics." . . .
>
> . . . *Lakoff and Núñez:* [**LR00**], *Preface, pp.xiii-xvii.*

43.4. Lakoff and Núñez's cognitive argument

Moreover, from the *evidence-based* perspective of this investigation, a significant conclusion of Lakoff and Núñez's cognitive argumentation is that:

> "Mathematics as we know it has been created and used by human beings: mathematicians, physicists, computer scientists, and economists—all members of the species *Homo sapiens.* This may be an obvious fact, but it has an important consequence. Mathematics as we know it is limited and structured by the human brain and human mental capacities. The only mathematics we know or can know is a brain-and-mind based mathematics.
>
> As cognitive science and neuroscience have learned more about the human brain and mind, it has become clear that the brain is not a general-purpose device. The brain and body co-evolved so that the brain could make the body function optimally. Most of the brain is devoted to vision, motion, spatial understanding, interpersonal interaction, coordination, emotions, language, and everyday reasoning. Human concepts and human language are not random or arbitrary; they are highly structured and limited, because of the limits and structure of the brain, the body, and the world." . . .
>
> . . . *Lakoff and Núñez:* [**LR00**], *Introduction, p.1.*

Accordingly—within the already noted limitations of their perspective of *mathematical idea analysis*—Lakoff and Núñez argue that any postulation of the existence of Platonic mathematical entities that are not ultimately grounded in metaphors reflecting our sensory motor perceptions is not supported by the findings of cognitive scientists.

Such postulation can only, therefore, be treated as an essentially unverifiable article of faith that reflects a *personal* belief (in the sense of §23.2(i)) which can have no bearing on any application of mathematical reasoning to the understanding (in the sense of §43.1) of what is common to either our mental concepts, or our external world (as argued persuasively by Krajewski on purely philosophical and mathematical grounds in [**Kr16**]—see Chapter 2).

Moreover, Lakoff and Núñez argue further that their above *observation immediately raises two questions*:

> "1. Exactly what mechanisms of the human brain and mind allow human beings to formulate mathematical ideas and reason mathematically?
>
> 2. Is brain-and-mind based mathematics all that mathematics *is*? Or is there, as Platonists have suggested, a disembodied mathematics transcending all bodies and minds and structuring the universe—this universe and every possible universe?
>
> Question 1 asks where mathematical ideas come from and how mathematical ideas are to be analyzed from a cognitive perspective. Question 1 is a scientific question, a question to be answered by cognitive science, the interdisciplinary science of the mind. As an empirical question about the human mind and brain, it cannot be studied purely within mathematics. And as a question for empirical science, it cannot be answered by an a priori philosophy or by mathematics itself. It requires an understanding of human cognitive

processes and the human brain. Cognitive science matters to mathematics because only cognitive science can answer this question.

. . . We will be asking how normal human cognitive mechanisms are employed in the creation and understanding of mathematical ideas. Accordingly, we will be developing techniques of mathematical idea analysis.

But it is Question 2 that is at the heart of the philosophy of mathematics. It is a question that most people want answered. Our answer is straightforward:

- Theorems that human beings prove are within a human mathematical conceptual system.
- All the mathematical knowledge that we have or can have is knowledge within human mathematics.
- There is no way to know whether theorems proved by human mathematicians have any objective truth, external to human beings or any other beings.

The basic form of the argument is this:

1. The question of the existence of a Platonic mathematics cannot be addressed *scientifically*. At best, it can only be a matter of faith, much like faith in a God. That is, Platonic mathematics, like God, cannot in itself be perceived or comprehended via the human body, brain, and mind. Science alone can neither prove nor disprove the existence of a Platonic mathematics, just as it cannot prove or disprove the existence of a God.
2. As with the conceptualization of God, all that is possible for human beings is an understanding of mathematics in terms of what the human brain and mind afford. The only conceptualization that we can have of mathematics is a human conceptualization. Therefore, mathematics as we know it and teach it can only be humanly created and humanly conceptualized mathematics.
3. What human mathematics is, is an empirical scientific question, not a mathematical or a priori philosophical question.
4. Therefore, it is only through cognitive science—the interdisciplinary study of mind, brain, and their relation—that we can answer the question: What is the nature of the only mathematics that human beings know or can know?
5. Therefore, if you view the nature of mathematics as a scientific question, then mathematics *is* mathematics as conceptualized by human beings using the brain's cognitive mechanisms.
6. However, you may view the nature of mathematics itself not as a scientific question but as a philosophical or religious question. The burden of scientific proof is on those who claim that an external Platonic mathematics does exist, and that theorems proved in human mathematics are objectively true, external to the existence of any beings or any conceptual systems, human or otherwise. At present there is no known way to carry out such a scientific proof in principle. . . ."

. . . Lakoff and Núñez: [**LR00**]*, Introduction, pp.1-3.*

Lakoff and Núñez note that there is an important part of this argument that needs further elucidation:

"What accounts for what the physicist Eugene Wigner has referred to as "the unreasonable effectiveness of mathematics in the natural sciences" (Wigner, 1960)? How can we make sense of the fact that scientists have been able to find or fashion forms of mathematics that accurately characterize many

> aspects of the physical world and even make correct predictions? It is sometimes assumed that the effectiveness of mathematics as a scientific tool shows that mathematics itself exists *in the structure of the physical universe*. This, of course, is not a scientific argument with any empirical scientific basis.
>
> . . . Our argument, in brief, will be that whatever "fit" there is between mathematics and the world occurs in the minds of scientists who have observed the world closely, learned the appropriate mathematics well (or invented it), and fit them together (often effectively) using their all-too-human minds and brains. . . . "
>
> *. . . Lakoff and Núñez:* [**LR00**]*, Introduction, p.3.*

Lakoff and Núñez then argue persuasively that any Platonic philosophy of mathematics is not supported by the findings of cognitive science, since it ignores that interpretation—a necessary prelude to understanding—of those concepts which are expressed in a mathematical language involves identification—sometimes layers upon layers—of conceptual metaphors grounded, ultimately, in our sensory-motor experiences:

> "Finally, there is the issue of whether human mathematics is an instance of, or an approximation to, a transcendental Platonic mathematics. This position presupposes a nonscientific faith in the existence of Platonic mathematics. We will argue that even this position cannot be true. The argument rests on analyses . . . to the effect that human mathematics makes fundamental use of conceptual metaphor in characterizing mathematical concepts. Conceptual metaphor is limited to the minds of living beings. Therefore, human mathematics (which is constituted in significant part by conceptual metaphor) cannot be a part of Platonic mathematics, which—if it existed—would be purely literal.
>
> Our conclusions will be:
>
> 1. Human beings can have no access to a transcendent Platonic mathematics, if it exists. A belief in Platonic mathematics is therefore a metaphor of faith, much like religious faith. There can be no scientific evidence for or against the existence of a Platonic mathematics.
> 2. The only mathematics that human beings know or can know is, therefore, *a mind-based mathematics*, limited and structured by human brains and minds. The only scientific account of the nature of mathematics is therefore an an account, via cognitive science, of human mind-based mathematics. Mathematical idea analysis provides such an account.
> 3. Mathematical idea analysis shows that human mind-based mathematics uses conceptual metaphors as part of the mathematics itself.
> 4. Therefore human mathematics cannot be a part of a transcendent Platonic mathematics, if such exists. . . . "
>
> *. . . Lakoff and Núñez:* [**LR00**]*, Introduction, p.4.*

Lakoff and Núñez base their conclusions upon advances in cognitive science that have deepened understanding of how *human beings conceptualize abstract concepts in concrete terms, using ideas and modes of reasoning grounded in the sensory-motor system*:

> "In recent years, there have been revolutionary advances in cognitive science—advances that have an important bearing on our understanding of mathematics. Perhaps the most profound of these new insights are the following:

1. *The embodiment of mind.* The detailed nature of our bodies, our brains, and our everyday functioning in the world structures human concepts and human reason. This includes mathematical concepts and mathematical reason.
2. *The cognitive unconscious.* Most thought is unconscious—not repressed in the Freudian sense but simply inaccessible to direct conscious introspection. We cannot look directly at our conceptual systems and at our low-level thought processes. This includes most mathematical thought.
3. *Metaphorical thought.* For the most part, human beings conceptualize abstract concepts in concrete terms, using ideas and modes of reasoning grounded in the sensory-motor system. The mechanism by which abstract is comprehended in terms of the concrete is called *conceptual metaphor.* Mathematical thought also makes use of conceptual metaphor, as when we conceptualize numbers as points on a line. ..."

... Lakoff and Núñez: [**LR00**]*, Introduction, pp.4-5.*

They argue that, contrary to the wisdom prevailing even in the cognitive sciences of the 1960's—when symbolic logic was thought by many to be endemic to abstract thinking—*symbolic logic is itself a mathematical enterprise that requires a cognitive analysis*:

"... Insights of the sort we will be giving ... were not even imaginable in the days of the old cognitive science of the disembodied mind, developed in the 1960s and early 1970s. In those days, thought was taken to be the manipulation of purely abstract symbols and all concepts were seen as literal—free of all biological constraints and of discoveries about the brain. Thought, then, was taken by many to be a form of symbolic logic. As we shall see ... symbolic logic is itself a mathematical enterprise that requires a cognitive analysis. For a discussion of the differences between the old cognitive science and the new, see *Philosophy in the Flesh* (Lakoff & Johnson, 1999) and *Reclaiming Cognition* (Núñez & Freeman, eds., 1999). ..."

... Lakoff and Núñez: [**LR00**]*, Introduction, p.5.*

The central thesis of Lakoff and Núñez's argument in [**LR00**] is that mathematical reasoning *layers metaphor upon metaphor* with such intricacy that *it is the job of the cognitive scientist to tease them apart so as to reveal their underlying cognitive structure*, since the *cognitive science of mathematics asks questions that mathematics does not, and cannot, ask about itself*:

"Mathematics, as we shall see, layers metaphor upon metaphor. When a single mathematical idea incorporates a dozen or so metaphors, it is the job of the cognitive scientist to tease them apart so as to reveal their underlying cognitive structure.

This is a task of inherent scientific interest. But it also can have an important application in the teaching of mathematics. We believe that revealing the cognitive structure of mathematics makes mathematics much more accessible and comprehensible. Because the metaphors are based on common experiences, the mathematical ideas that use them can be understood for the most part in everyday terms.

The cognitive science of mathematics asks questions that mathematics does not, and cannot, ask about itself. How do we understand such basic concepts as infinity, zero, lines, points, and sets using our everyday conceptual apparatus? How are we to make sense of mathematical ideas that, to the novice, are paradoxical—ideas like space-filling curves, infinitesimal numbers, the point at infinity, and non-well-founded sets (i.e., sets that "contain themselves" as members)? ...

... we will be concerned not just with *what* is true but with what mathematical ideas *mean*, how they can be understood, and *why* they are true. We will also be concerned with the nature of mathematical truth from the perspective of a mind-based mathematics.

One of our main concerns will be the concept of infinity in its various manifestations: infinite sets, transfinite numbers, infinite series, the point at infinity, infinitesimals, and objects created by taking values of sequences "at infinity," such as space-filling curves. We will show that there is a single Basic Metaphor of Infinity that all of these are special cases of. This metaphor originates outside mathematics, but it appears to be the basis of our understanding of infinity in virtually all mathematical domains. When we understand the Basic Metaphor of Infinity, many classic mysteries disappear and the apparently incomprehensible becomes relatively easy to understand."
...

... Lakoff and Núñez: [**LR00**]*, Introduction, pp.7-8.*

Lakoff and Núñez emphasise that the results of their inquiry *are not results reflecting the conscious thoughts of mathematicians; rather, they describe the unconscious conceptual system used by people who do mathematics*:

The results of our inquiry are, for the most part, not mathematical results but results in the cognitive science of mathematics. They are results about the human conceptual system that makes mathematical ideas possible and in which mathematics makes sense. But to a large extent they are not results reflecting the conscious thoughts of mathematicians; rather, they describe the *unconscious* conceptual system used by people who do mathematics. The results of our inquiry should not change mathematics in any way, but they may radically change the way mathematics is understood and what mathematical results are taken to mean.

Some of our findings may be startling to many readers. Here are examples:

- Symbolic logic is not the basis of all rationality, and it is not absolutely true. It is a beautiful metaphorical system, which has some rather bizarre metaphors. It is useful for certain purposes but quite inadequate for characterizing anything like the full range of the mechanisms of human reason.
- The real numbers do not "fill" the number line. There is a mathematical subject matter, the hyperreal numbers, in which the real numbers are rather sparse on the line.
- The modern definition of *continuity* for functions, as well as the so-called *continuum*, do not use the idea of continuity as it is normally understood.
- So-called *space-filling curves* do not fill space.
- There is no absolute yes-or-no answer to whether $0.99999\ldots = 1$. It will depend on the conceptual system one chooses. There is a mathematical subject matter in which $0.99999\ldots = 1$, and another in which $0.99999\ldots \neq 1$.

These are not new mathematical findings but new ways of understanding well-known results. They are findings in the cognitive science of mathematics—results about the role of the mind in creating mathematical subject matters.

Though our research does not affect mathematical results in themselves, it does have a bearing on the understanding of mathematical results and on the claims made by many mathematicians. Our research also matters for the philosophy of mathematics. *Mind-based mathematics*, as we describe it

. . . , is not consistent with any of the existing philosophies of mathematics: Platonism, intuitionism, and formalism. Nor is it consistent with recent post-modernist accounts of mathematics as a purely social construction. Based on our findings, we will be suggesting a very different approach to the philosophy of mathematics. We believe that the philosophy of mathematics should be consistent with scientific findings about the only mathematics that human beings know or can know. We will argue . . . that the *theory of embodied mathematics* . . . determines an empirically based philosophy of mathematics, one that is coherent with the "'embodied realism" discussed in Lakoff and Johnson (1999) and with "'ecological naturalism" as a foundation for embodiment (Núñez, 1995, 1997).

Mathematics as we know it is human mathematics, a product of the human mind. Where does mathematics come from? It comes from us! We create it, but it is not arbitrary—not a mere historically contingent social construction. What makes ,mathematics nonarbitrary is that it uses the basic conceptual mechanisms of the embodied mind as it has evolved in the real world. Mathematics is a product of the neural capacities of our brains, the nature of our bodies, our evolution, our environment, and our long social and cultural history." . . .

. . . *Lakoff and Núñez:* [**LR00**], *Introduction, pp.8-9.*

CHAPTER 44

The Veridicality of Mathematical Propositions

Based on our above interpretation of Lakoff and Núñez's analysis in [**LR00**], we could express a tacit thesis of this investigation as:

Thesis 44.1. Those of our conceptual metaphors which we commonly accept as of a mathematical nature—whether grounded directly in an external reality, or in an internally conceptualised Platonic universe of conceived concepts (such as, for example, Cantor's first transfinite ordinal ω)—when treated as Carnap's *explicandum*, are expressed most naturally in the language of the first-order Set Theory ZFC.

This reflects the *evidence-based* perspective of this investigation that (see §21.4; also Chapter 23):

- Mathematics is a set of symbolic languages;
- A language has two functions—to express and to communicate mental concepts[1];
- The language of a first-order Set Theory such as ZFC is sufficient to adequately represent (Carnap's *explicatum*: see Chapter 14) those of our mental concepts (Carnap's *explicandum*: see Chapter 14) which can be communicated unambiguously; whilst the first-order Peano Arithmetic PA best communicates such representations to an other categorically.

It also reflects Weyl's perspective that the 'genuine value and significance' of any mathematical language lies in the 'extent that its concepts can be interpreted intuitively without affecting the truth of our assertions about those concepts':

> "Returning now to Richard's antinomy, we must acknowledge a kernel of truth in the apparent contradiction: set theory and logicized mathematics involve only countably many relation-concepts, but certainly not just countably many things or sets. This is primarily because the introduction of new sets is not limited to the extraction of subsets of a given set, as the aforementioned axiom allows, the elements of that subset being characterized by a definite property. There is also set formation through addition, multiplication, and exponentiation, operations whose possibility is posited by Zermelo's remaining axioms. There is absolutely no question of an antinomy here.
>
> Might we say that mathematics is the science of ε and those relations definable from ε by means of the principles we have mentioned? Developments to date make this seem likely and perhaps this analysis really does correctly determine the logical content of mathematics. Consider, however, a set theoretically constructed conceptual system for logicized mathematics. It seems to me that this system will have genuine value and significance only to

[1] Qn: Is this reflected in the structure or activity of the brain?

> the extent that its concepts can be interpreted intuitively without affecting the truth of our assertions about those concepts."
> ... *Weyl:* [**We10**], *p.10.*

We would further conjecture that:

THESIS 44.2. The need for adequately expressing such conceptual metaphors in a mathematical language reflects an evolutionary urge of an organic intelligence to determine which of the metaphors that it is able to conceptualise can be unambiguously communicated to another intelligence—whether organic or mechanical—by means of *evidence-based* reasoning and, ipso facto, can be treated as faithful representations of a commonly accepted external reality (universe).

The conjecture is obliquely reflected in Dennett's remarks:

> We and only we, among all the creatures on the planet, developed language. Language is very special when it comes to being an information handling medium because it permits us to talk about things that arent present, to talk about things that don't exist, to put together all manner of concepts and ideas in ways that are only indirectly anchored in our biological experience in the world. Compare it, for instance, with a vervet monkey alarm call. The vervet sees an eagle and issues the eagle alarm call. We can understand that as an alarm signal, and we can see the relationship of the seen eagle and the behavior on the part of the monkey and on the part of the audience of that monkeys alarm call. Thats a nice root case."
> ... *Dennett:* [**De17**].

Moreover, we may then need to consider whether:

- A plausible perspective as to what is, or is not, a valid mathematical concept would be to regard such concepts as those conceptual metaphors that:

  (a) an ω-consistent (and demonstrably undecidable by [**Go31**], Theorem VI) language—such as a first-order set theory ZFC—can adequately express subjectively (in the sense of §23.1(a));

  and, thereafter, which of these conceptual metaphors:

  (b) an ω-*in*consistent (and demonstrably categorical by [**An16**], Theorem 7.2) language—such as the first-order Peano Arithmetic PA—is able to unambiguously communicate objectively (in the sense of §23.1(b)).

In other words, we may need to consider whether (in sharp contrast to the perspective offered by Maddy in [**Ma18**] and [**Ma18a**]):

- Set theory is most appropriately viewed as the foundation for those of our conceptual metaphors which can be adequately expressed in a first-order mathematical language;

whilst:

- Arithmetic is most appropriately viewed as the foundation for those of our conceptual metaphors which can be unambiguously communicated in a first-order mathematical language.

Such a perspective would reflect an underlying thesis of this investigation (§23), which is that mathematics ought to be viewed simply as a set of languages;

- some of adequate expression,
- and some of unambiguous and effective communication,

for Lakoff and Núñez's conceptual metaphors.

Moreover[2], that the veridicality of mathematical propositions can ultimately be grounded in only *those* conceptual metaphors whose formal representations within the language we can either:

- label as 'finitarily true' by convention if, and only if, they either correspond to *evidence-based* axioms and rules of inference (i.e., to some constructively well-defined logic by Definition 21.5) of some language;

or:

- label as 'experientally true' by convention if, and only if, they are mappings of *evidence-based* observations of a commonly accepted external universe.

One is then led to develop and isolate from these philosophies a more holistic perspective of 'where mathematics comes from', rather than the epistemically grounded perspective of conventional wisdom—as articulated, for instance, in [**LR00**][3] or [**Shr13**]—which ignores the distinction between the multi-dimensional nature of the logic of a formal mathematical language (Definition 21.5), and the one-dimensional nature of the veridicality of its assertions.

Such a synthesised view of 'where mathematics comes from' should, it seems, be able to offer complementary perspectives for the basic issues on which the various philosophies were founded. Such as, amongst others:

- the logicist's identity of mathematics and logic;
- the formalist's stress on the internal validity and self-sufficiency criteria of a theory;
- the intuitionist's objection to passing from the negation of a general statement to an existential one without additional safeguards;
- the conventionalist's contention that the rules of a language delineate its ontology;
- as also the nominalist's scruples about the existence of classes of classes.

We conclude by naïvely addressing some of the perspectives—implicit in this investigation—on how we perceive the nature and formation of abstract mental concepts that are expressed in the usual mathematical languages in terms of Carnap's *explicatum* and *explicandum* (see Chapter 14).

44.1. Where does the veridicality of mathematics come from?

We address the query: Where does the veridicality of mathematical propositions come from?

[2]As expressed by Tarski in a broader context ([**Ta35**]): 'Snow is white' is a true sentence if, and only if, snow *is* white.

[3]A more appropriate title for which, from such a perspective, would be *Where the Veridicality of Mathematical Propositions Comes From.*

(a) I form concepts. That much seems reasonably clear to me. Their location I assume to be in the commonly referred to intuition. Concept space may be a better name for it.

(b) An analysis of these concepts I find to be a more difficult task than indicating their significance. So I intend to study merely the latter. However, I do take individuals, properties and facts as concepts.

(c) Events in physical space, indeed the space itself, are perceived and digested by my senses, whence they transform into concepts.

(d) My concepts I may map into a language. This map you may decode into your concepts.

Assuming that both of us accept a common external world, I can understand why language is so useful.

(e) When I set up a language, there is what I talk about. Serious dispute cannot arise so long as my language faithfully refers to my concepts.

(f) I may feel the need to include Pegasus among my concepts. Your stoutest efforts will not convince me to analyse the name out à la Russell. A description into non-trivial terms of my ontology I would consider inadequate. And the trivial description of 'pegasises' I would only agree to as an introduction of a name for a concept of being Pegasus—a concept antecedent to the being of Pegasus among my concepts.

Or I may protest altogether against the being of any 'pegasises' concept in my concept space, and refuse to admit discovery or creation of any such concept.

(g) Confusion may sometimes arise. You may wrongly translate my language into your concepts. My conceptual scheme may contradict the external world. I may have concepts not accessible to you.

In the first case you would be mistaken. In the second I should be convicted of error—or possibly idealism! But who is to judge?

Of some interest is the third. This I see as the cause of all genuine ontological disputes. From philosophy through to theology.

Taken to be a question of individual concepts, ontology seems more a matter of taste, inclination and, above all, feeling and belief in this case.

So its interest as a problem is, after all, trivial. As it should be.

(h) For, as long as I concern myself with ontology, restricting myself to a language constructed on the basis of my mental concepts, I shall for all practical purposes be dealing with the small aspect of the world which is conceptualised by my senses. And this, as Zeno's reflections seem to indicate, can hardly be said to exhaust nature's complexity (as sought to be illustrated in §24.5 and §25.1.).

(i) So I turn my back for the moment on concepts. All I am left with then is language, and possibly codifications of nature into language.

And my inability to grasp the totality of nature's concepts is contained in my use of variable names, and the transition from propositions to schemata.

And the test of any codifications as suitable for nature will be the inclusion in it of the concepts that are within my grasp.

(j) But what there is in addition may, after all, depend on language in cases where empirical verification is lacking.

44.2. Russel's paradox?

We briefly consider Russell's paradox from a naïve *set-theoretical* perspective that seeks to adequately express some of our conceptual metaphors in a symbolic language.

(a) Consider the ZFC expression:

(i) $x \notin x$.

If we suppose that there is a class 'a' in our language ZFC representing an individual entity 'a^*—that exists, or must necessarily exist, as the root of one of our conceptual metaphors—whose members are precisely those that satisfy (a)(i), then we would hold that, in this instance, we have discovered a true statement schema:

(ii) $x \in a \leftrightarrow x \notin x$,

which expresses a host of facts concerning 'a^*' and all the various members of some pre-existing universe that the metaphors are *taken* to conceptualise.

But this belief is surely mistaken, for:

(iii) $a \in a \leftrightarrow a \notin a$,

is clearly false in ZFC.

(b) Suppose, on the other hand, we say that we are merely defining a class 'a' in ZFC that represents an individual entity that may already exist—or might conceivably exist—as the root of our conceptual metaphors by:

(i) $x \in a$ if, and only if, $x \notin x$.

Though this should now be a true statement in our language ZFC about the metaphors, it may no longer be a statement about anything in the universe that the metaphors *aim* to conceptualise (Compare Skolem's remarks in [**Sk22**], p.295; see also §22.4).

(c) But if we treat definition as a creative activity for producing a larger 'conceivable' ontology, it is not surprising that we can arrive back at a paradoxical, but supposedly true, ZFC statement:

(ii) $a \in a \leftrightarrow a \notin a$,

about the putative universe that the metaphors *claim* to conceptualise.

This position regarding creativity may differ but formally from our earlier Platonistic stand.

(d) However, if we do not view definition as mere name-giving to newly born or already flourishing objects, then it is not easy to see what all the fuss is about.

For, if definition requires eliminability, then expressions such as '$a \in a$' and '$a \notin a$' are immediately suspect—since we are able to eliminate only '$x \in a$' from any expression.

And 'a' in isolation is merely a strange creature giving rise to pseudo-expressions which confuse us as to their admissibility into our formal language because of their familiar appearance (a point that we have illustrated when highlighting the fragility of the conventional arguments for the existence of non-standard models of Arithmeetic in §20.1).

But then, so too does Pegasus confuse us into sometimes creating a putative inhabitant of a putatively common Platonic world of permanent ideas and unactualised possibilities out of merely the subjective, and fleeting, conceptual metaphors created within our cognition with respect to the word 'Pegasus'!

In other words, as Quine ([**Qu53**]) has compellingly argued, a name need not name anything that we would accept as the root of a grounded conceptual metaphor (even though a name might itself give rise to a consequent conceptual metaphor grounded on the 'name' itself).

For names belong to language essentially. And, even when patently absurd or vacuous—e.g., Squircle defined as a 'square circle', or 'Louis XX' defined as 'the present king of France'—are easy to construct.

(e) There is a fuss, for the contradictions still haunt some of us. So possibly we are loath to admit an error in our earliest discovery. The seeming 'truth' of the statement schema:

(i) $x \in a \leftrightarrow x \notin x$.

Now could it be that this reluctance to accept the negation of Cantor's Comprehension Axiom is—as Lakoff and Núñez's analysis of the origin of 'mathematical' conceptual metaphors seems to suggest—psychologically motivated?

For instance, as Pereplyotchik remarks:

"There are, broadly speaking, three competing frameworks for answering the foundational questions of linguistic theory—cognitivism (e.g., Chomsky 1995, 2000), platonism (e.g., Katz 1981, 2000), and nominalism (e.g., Devitt 2006, 2008).

Platonism is the view that the subject matter of linguistics is an uncountable set of*abstracta*—entities that are located outside of spacetime and enter into no causal interactions. On this view, the purpose of a grammar is to lay bare the essential properties of such entities and the metaphysically necessary relations between them, in roughly the way that mathematicians do with numbers and functions. The question of which grammar a speaker cognizes is to be settled afterward, by psychologists, using methods that are quite different from the*nonempirical*methods of linguistic inquiry.

The nominalist, too, denies that grammars are psychological hypotheses. But she takes the subject matter of linguistics to consist in concrete physical tokens—inscriptions, acoustic blasts, bodily movements, and the like. Taken together, these entities comprise public systems of communication, governed by social conventions. The purpose of a grammar, on this view, is to explain why some of these entities are, e.g., grammatical, co-referential, or contradictory, and why some entail, bind, or c-command others.

Cognitivism, by contrast, is the view that linguistics is a branch of psychology—i.e., that grammars are hypotheses about the language faculty, an aspect of the human mind/brain. A true grammar would be psychologically real, in the sense that it would correctly describe the tacit knowledge that every competent speaker

> has—a system of psychological states that is causally implicated in the use and acquisition of language."
> . . . *Pereplyotchik:* [**Per17**].

The cause to which we are clinging so stubbornly—armed with Russell's types, Zermelo's efforts, amongst others—may be that starting from an ontological acceptance of some individuals and properties, we must somehow have the right to build up further properties into our putative universe. The paradoxes seem to prevent us from doing so with complete freedom.

(f) But why do we not feel the need to a similar liberty in the other direction? Regarding individuals.

Why do we not feel as strongly or as readily that by defining all the properties that occur in our ontology for a new individual, we may enlarge our universe?

(g) The path may not be any smoother. For suppose we intend to introduce the individual 'k' into our ontology. And our ontology contains a property schema $P(x, y)$. (Which may, for example be 'y loves x').

If our desire for liberty was sincere, we should feel free to then assign properties at will to the new entry.

But what happens?

(h) Let us assign the $P(x, y)$'s to the entity 'k' as follows:

(i) $P(x, k)$ if, and only if, $\neg P(x, x)$.

Since 'k' is part of our ontology, do we have:

(ii) $P(k, k)$

or

(iii) $\neg P(k, k)$?

(i) My point is that as long as we have the desire to construct new relations amongst existing entities, we should also have the equal desire to construct new entities out of existing relations.

That if we have the feeling we can discover all kinds of possible relations amongst the individuals, we should also feel we can discover all kinds of individuals enmeshed in our relations.

That the guidelines in one case should be as useful in the other. That if every open formula in individuals seems to define a predicate, then every open formula in predicates should define an individual. To take a very naïve view.

That we may be psychologically misled into feeling that a predicate open formula defines an entity known as the predicate of a predicate.

(j) So maybe there is much to be said for the nominalist stand. And isn't the idea that every individual be equivalent to the set of all the predicates that it satisfies at the heart of Leibniz's notion of indiscernibles? As also at the heart of phenomenalism and positivism?

(k) And where the external world is concerned, is it possible that quantum-interpreted phenomena may contain instances of plurality where the objects are indiscernibles—notwithstanding Leibniz's contention?

(k) And inspite of Russell's claim of having no content to his universe does not the fact that it has no indiscernibles give it content—at least in the form of a special characteristic?

44.3. An illustrative model: language and ontology

(a) I have a concept of a possible universe that I should like to codify into language.

(b) In my universe there are individuals, and there are properties. The landscape is otherwise deserted.

(c) The individuals I shall name a, b, c, d, e. The properties F, G, H.

(d) There are also (in some sense of being which is not entirely clear to me) facts in my universe. These I shall represent in my language as:

$F(a), F(b), G(b), G(c), G(e), H(b), H(c)$ and $H(e)$.

I shall call these true expressions in my language.

(e) There are no such things (or whatever it is that facts are supposed to be) as non-facts in my universe. All the same, I admit certain expressions into my language—possibly for the sake of symmetry, but more so because tradition seems to demand such an action. These are:

$F(c), F(d), F(e), G(a), G(d), H(a)$, and $H(d)$.

I shall call these false expressions.

(f) Though my language, containing these expressions, is thus two-valued, in my universe there are only facts.

(g) A very natural question may be asked for any set of individuals. Is there a property satisfied by all the members of the set, and none others?

I think I must be very clear about the nature of my enquiry. I am not asking whether my language can countenance the introduction of a further expression purporting to be a property. Such an entry, like the introduction of false expressions, may not present formidable difficulties. But I am enquiring whether my universe already contains such a property.

(h) Taking $\{a, b, d\}$, as the set, I find no property which gives rise to true expressions for this set only. My finding is, of course, empirical.

(i) For the set $\{a, b\}$ however, the property F does give rise to true expressions; and no other individual satisfies F. And I may conveniently identify the set with F insofar as they are both names of the same entity.

(j) What of the set $\{b, c, e\}$? Both G and H express facts for the members of this set only. But there is no unique property identifiable with this set. And, in passing, I may remark that such an event does not cause any concern usually. Properties with the same extension are tolerated easily.

(k) I conclude that not every set of individuals can be identified with a unique property.

So, a set of individuals may not name anything in my universe.

(l) A question of far greater significance is as to the nature of sets of properties. Classically these have been treated as being identifiable with a different quality of being in the universe from that of properties and individuals.

(m) But though my language is prolific in sets, my universe is starved for entities. So I look for some more direct identifications for these sets than those suggested by precedent.

Surprisingly, I am successful—or so it seems. And my solution appears so natural that I begin to suspect that tradition may well have been merely disguising it.

(n) For a set of properties, I ask the question whether any individual has just those properties, and none others.

For the set $\{F, G\}$ there is no such individual.

The set $\{F, G, H\}$ may be identified with the individual b, which is the only one satisfying all three properties.

Similarly, $\{F\}$ may be identified with a.

(o) But now I consider the set $\{G, H\}$. Both c and e satisfy only this set. Which is a most surprising characteristic of my universe. It contains two indiscernibles!

(Inspite of Leibniz, and Russell's subsequent backing of his ideas on the intuitive notion of equality, modern physics has made a universe with such characteristics rather feasible. What is required for such a feature is that some set of properties be identified with a plurality of individuals.)

I find, then, that not every set of properties is identifiable with an individual.

(p) So, if I contain myself to the ontology outlined, some sets of properties, as also of individuals, don't exist, while some do, and still others exhibit an ambiguous character.

But all this is peculiar to my universe. And not every universe need be of this type. The universe being constructed by an intuitionist may have differing qualities. Depending on the manner in which he sets up his intuitive concepts of individuals and relations, and expresses his facts.

(q) But what is important to note—for I feel it has caused the greatest confusion—is that sets belong to language, and their corresponding existence in the universe lies in their identifiability, along the lines already indicated, with the entities of the universe.

Such identifiability may be empirically determinable, if the universe is capable of representation as above. Or it may be conventional, when the universe is being constructed.

44.4. Is the Russell-Frege definition of number significant?

(a) I cannot countenance a predicate of predicates unreservedly.

I am able to cheerfully admit the existence of individuals in a universe.

I can also, hesitantly at first, embrace the seemingly necessary existence of properties.

(b) But now I see two things.

That each property has an extension, in my language at least, of all the individuals satisfying it. And each individual has an extension of all the properties that it possesses.

And any class of individuals that I am able to construct in my language can only—if at all—be identifiable as the extension of a possible property satisfied by the members of the class. The existence of such a property—and hence the reflection of the fact of this existence, in my language—must remain an empirical truth—or a truth by convention.

And, similarly, any class of properties that I can produce in my language is not the reflection of some creature known as a predicate of predicates, but—at the most—the extension identifiable with a possible individual having only the properties contained in the class. The existence of such an individual is again, I dare say, an empirical fact—or a convention.

Now, why does my mind rebel at the thought of indiscriminately creating such individuals?

The reason is chiefly heuristic. As may be expected.

(c) Given a set of individuals, and a two-valued language, I am able to construct 2^n distinct classes. If all these exist as properties, then each property is identifiable with some particular class of not more than n individuals. It is not even necessary to insist for the moment that the class be evident to me. So long as I admit that it is a determined class in my language.

Clearly each individual is also identifiable with some class of not more than 2^n properties.

(d) But now there are 2^{2^n} new individuals which are constructible—at least theoretically so—in my language (which may even embrace a class theory for the construction of its classes, if this is in some way thought possible).

If I try to introduce these in my universe, then the extensions of some of my previous properties will have to be enlarged.

In what sense can I then speak of a property as the static concept it usually is taken to be? Without divorcing it completely from my individuals? In which case, how may I even construct a new property? Unless, of course, I adopt a system of double book-keeping.

And, possibly, this is the reason that Cantor's axiom of comprehension, when applied to ontology, is invalid. As also the reason that a distinction needs to be drawn between classes and sets in set theory—which is, I believe, implicitly taken to be applicable to both language and ontology.

Whether such a distinction has been validly and consistently made relative to the view that I have taken above is a different question. One well worth investigating.

(e) But now I see a major defect in logicism.

$2(f)$ is defined to mean that there exists an x, and there exists a y, satisfying f, and x is not equal to y, and if there is some z satisfying f, then either z is equal to x, or z is equal to y.

The class, in my language of course, of fs for which this is true is then identified with an object in the universe containing f over which x and y range.

Such an object, as I have already averred, I can only take to be an individual, say '2'.

But then it appears that every property which has only two true arguments in my universe must necessarily have '2' as one of these (amongst its) arguments! A patently unacceptable conclusion.

At least from an aesthetic point of view, so far as my common sense is concerned. But common sense is not a very reliable guide, and it remains to be seen whether this is also logically (in some sense of the word logic) unacceptable. As I feel it must be. The point is an important one and needs to be investigated.

(f) So I do not accept the individual '2' as identifiable anyhow in my universe. Even though $2(f)$ is a meaningful, and very significant, sentential formula in my language. For it does contain the essence of the meaning-in-use of the number 'two'. And this, I believe, is the really outstanding achievement of logicism. Its analysis of the origin of the number concept ([**Rus19**], Chapter II, pp.11-19). But not its so-called logical construction of the concepts of the integers.

Of course Russell has, to my way of thinking, managed to cloud the issue by ascribing a different level of existence to the individuals constructed from classes of predicates. Which again appears to be a case of multiple standards, since not all classes of predicates—as I have tried to show earlier—need necessarily give rise to the type of difficulty discussed above. Some classes are easily and most naturally identifiable with individuals.

Russell's types are then seen to be nothing more than the setting up of various *synthetic* universes in a kind of chain formation. The lowest being a universe either set up by convention, or which is evident to my senses. The next—not by addition to the first—but rather by identification with expressions of the language in which I talk of my initial universe. And so on.

(g) And of course the language I use to reflect my initial universe will contain expressions for all the possible entities and facts that could possibly occur in it, irrespective of what actually may be occurring at the time I discover/construct it. So Russell may quite readily, though unpardonably for having obfuscated the issue, claim that his universe—which actually contains all the members of the chain that I referred to above—has no content.

And whether we call it one universe or a chain of universes is hardly worth a demonstration at Trafalgar Square.

So long as we can remember that all the successor universes have been constructed from language.

(h) Which gives me enough reason to try and explain why language and ontology have so often been confused.

And my way of justifying the seeming prolificacy of language—which I already hinted at above—is this.

I think it would be readily agreed that in the external world there are facts—which may be said to have existence. To ascribe an existence to a non-fact in this universe seems to me somewhat far-fetched, despite McX and Wyman ([**Qu53**]).

Yet I am able, in my language about the external world, to create both factual and non-factual or false expressions.

And this seems a very fortuitous occurrence in view of my desire to communicate with, and be communicated to faithfully by, a fallible humanity.

So the expressions in my language seem—at least to my naïvely finite senses—to exceed the facts in the universe.

(i) Which of course may be an assumption of a very basic and significant nature underlying all my mathematics—hence giving a possible circularity to Cantor's Theorem that 2^n exceeds n for all numbers.

44.5. Summary

(01) Discovery of what there is, or construction (by convention—other means if thought feasible) of what I feel should be, I take as the basic idea underlying all my mental activity.

(02) Language, as the means by which such discovery, or construction, is expressed or conveyed to you.

(03) Logical notions as the instruments used to extend what 'is' in any given case to what is possible or could have been possible—in addition to, or as alternative to—the given case.

(04) So logic in effect symmetricises language—originally conceived as a carrier of only what there 'is', or, more precisely, of what I believe there 'is'—into containing 'more' than what actually 'is', in terms of what is possible or conceivable.

(05) Which gives me a freedom, on the basis of these conceivable entities, entertained by my language (corresponding to the expressions containing free variables, or sets as they are also called) and taking into account what already is, to construct by some means a 'larger', clearly artificial, universe.

(06) Larger in the sense that a suitable construction immediately seems to give me Cantor's Theorem—at least if I include all conceivable entities of the first into the second.

(07) But my constructions necessarily give me a new universe. Though I may be able to map my initial ontology into it in some way.

(08) And the obviously recursive procedure gives me a series of universes which Russell calls types.

(09) Though there seems no meaningful way in which we can talk of all the universes being united into a universe of universes, with their various entities co-existing peaceably.

(10) And the Continuum Hypothesis may be but a convention (compare §19.3)—a relation between two successive universes—reflecting the manner in which one is constructed out of the other. A relation, then, (like Cantor's) between what is taken 'to be' in a universe, and all that can be constructed from it by means of language.

(11) And, so, in some sense what there 'is' does depend on language. At least in all the universes succeeding the initial. And on convention.

(12) And whether this thing is what we call 'mathematics' depends on whether my initial universe has entities that are only expressed in a mathematical language.

APPENDIX A

Some comments on standard definitions, notations, and concepts

Axioms and rules of inference of the first-order Peano Arithmetic PA

PA$_1$ $[(x_1 = x_2) \rightarrow ((x_1 = x_3) \rightarrow (x_2 = x_3))]$;

PA$_2$ $[(x_1 = x_2) \rightarrow (x_1' = x_2')]$;

PA$_3$ $[0 \neq x_1']$;

PA$_4$ $[(x_1' = x_2') \rightarrow (x_1 = x_2)]$;

PA$_5$ $[(x_1 + 0) = x_1]$;

PA$_6$ $[(x_1 + x_2') = (x_1 + x_2)']$;

PA$_7$ $[(x_1 \star 0) = 0]$;

PA$_8$ $[(x_1 \star x_2') = ((x_1 \star x_2) + x_1)]$;

PA$_9$ For any well-formed formula $[F(x)]$ of PA:
$[F(0) \rightarrow (((\forall x)(F(x) \rightarrow F(x'))) \rightarrow (\forall x)F(x))]$.

Generalisation in PA If $[A]$ is PA-provable, then so is $[(\forall x)A]$.

Modus Ponens in PA If $[A]$ and $[A \rightarrow B]$ are PA-provable, then so is $[B]$.

Cauchy sequence: A sequence $x_1, x_2, x_3, \ldots$ of real numbers is a Cauchy sequence if, and only if, for every real number $\epsilon > 0$, there is a an integer $N > 0$ such that, for all natural numbers $m, n > N$, $|x_m - x_n| \leq \epsilon$.

Conservative extension: A theory T_2 is a (proof theoretic) conservative extension of a theory T_1 if the language of T_2 extends the language of T_2; that is, every theorem of T_1 is a theorem of T_2, and any theorem of T_2 in the language of T_1 is already a theorem of T_1.

First-order language (we essentially follow the definitions in [**Me64**], p.29): A first-order language L consists of:

(1) A countable set of symbols. A finite sequence of symbols of L is called an *expression* of L;

(2) There is a subset of the expressions of L called the set of *well-formed formulas* (abbreviated 'wffs') of L;

(3) There is an effective procedure (based on *evidence-based* reasoning) to determine whether a given expression of L is a wff of L.

Moreover—reflecting the *evidence-based* perspective of this investigation as detailed in the proposed Definitions 21.3 to 21.7—we shall explicitly distinguish between a first-order language and:

- any *first-order theory* that seeks—on the basis of *evidence-based* reasoning—to assign the values 'provable/unprovable' to the well-formed formulas of the language under a *proof-theoretic logic*;
- any *first-order theory* that seeks—on the basis of *evidence-based* reasoning—to assign the values 'true/false' to the well-formed formulas of the language under a *model-theoretic logic*.

First-order language with quantifiers (we essentially follow the definitions in [**Me64**], pp.56-57): A first-order language K with quantifiers is a first-order language whose alphabet consists of:

(1) The propositional connectives '$\neg$' and '$\rightarrow$';
(2) The punctuation marks '(', ')' and ',';
(3) Denumerably many individual variables $x_1, x_2, \ldots$;
(4) A finite or denumerable non-empty set of predicate letters $A_j^n\,(n, j \geq 1)$;
(5) A finite or denumerable, possibly empty, set of function letters $f_j^n\,(n, j \geq 1)$;
(6) A finite or denumerable, possibly empty, set of individual constants $a_i\,(i \geq 1)$;

where the function letters applied to the variables and individual constants generate the *terms* as follows:

(a) Variables and individual constants are terms;
(b) If f_i^n is a function letter, and $t_1, \ldots, t_n$ are terms, then $f_i^n(t_1, \ldots, t_n)$ is a term;
(c) An expression of K is a term only if it can be shown (on the basis of *evidence-based* reasoning) to be a term on the basis of clauses (a) and (b).

Further:

(d) The predicate letters applied to terms yield the *atomic formulas*, i.e., if A_i^n is a predicate letter and $t_1, \ldots, t_n$ are terms, then $A_i^n(t_1, \ldots, t_n)$ is an atomic formula.

and:

(e) The well-formed formulas (wffs) of K are defined as follows:
  (i) Every atomic formula is a wff;
  (ii) If $\mathcal{A}$ and $\mathcal{B}$ are wffs and y is a variable, then '$\neg\mathcal{A}$', '$\mathcal{A} \rightarrow \mathcal{B}$' and '$(\forall y)\mathcal{A}$' are wffs;
  (iii) An expression of K is a wff of K only if it can be shown (on the basis of *evidence-based* reasoning) to be a wff on the basis of clauses (i) and (ii).

Moreover, we follow the convention that defines:

(f) '$\mathcal{A} \wedge \mathcal{B}$' as an abbreviation for '$\neg(\mathcal{A} \rightarrow \mathcal{B})$';

(g) '$\mathcal{A} \vee \mathcal{B}$' as an abbreviation for '$(\neg\mathcal{A}) \rightarrow \mathcal{B}$';

(h) '$\mathcal{A} \equiv \mathcal{B}$' as an abbreviation for '$(\mathcal{A} \rightarrow \mathcal{B}) \wedge (\mathcal{B} \rightarrow \mathcal{A})$';

(i) '$(\exists x)\mathcal{A}$' as an abbreviation for '$\neg((\forall x)\neg\mathcal{A})$'.

First-order theory with quantifiers (we essentially follow the definitions in [**Me64**], pp.56-57): A first-order theory S with quantifiers is a first-order language with quantifiers plus a set of rules—which we define as the *proof-theoretic logic* of S—that assigns *evidence-based* 'provability' values to the wffs of S by means of logical axioms, proper axioms, and rules of inference as follows:

I: If $\mathcal{A}, \mathcal{B}, \mathcal{C}$ are wffs of S, then the following logical axioms are designated as provable wffs of S:

(1) $\mathcal{A} \rightarrow (\mathcal{B} \rightarrow \mathcal{A})$;

(2) $(\mathcal{A} \rightarrow (\mathcal{B} \rightarrow \mathcal{C})) \rightarrow ((\mathcal{A} \rightarrow \mathcal{B}) \rightarrow (\mathcal{A} \rightarrow \mathcal{C}))$;

(3) $(\neg\mathcal{B} \rightarrow \neg\mathcal{A}) \rightarrow ((\neg\mathcal{B} \rightarrow \mathcal{A}) \rightarrow \mathcal{B})$;

(4) $(\forall x_i)\mathcal{A}(x_i) \rightarrow \mathcal{A}(t)$ if $\mathcal{A}(x_i)$ is a wff of S and t is a term of S free for x_i in $\mathcal{A}(x_i)$;

(5) $(\forall x_i)(\mathcal{A} \rightarrow \mathcal{B}) \rightarrow (\mathcal{A} \rightarrow (\forall x_i)\mathcal{B})$ if $\mathcal{A}$ is a wff of S containing no free occurences of x_i.

II: The proper axioms of S which are to be designated as provable wffs of S vary from theory to theory.

A first-order theory in which there are no proper axioms is called the first-order logic FOL.

III: The rules of inference of any first-order theory are:

(i) Modus ponens: If $\mathcal{A}$ and $\mathcal{A} \rightarrow \mathcal{B}$ are provable wffs of S, then $\mathcal{B}$ is a provable formula of S;

(ii) Generalisation: If $\mathcal{A}$ is a provable wff of S, then $(\forall x_i)\mathcal{A}$ is a provable wff of S.

IV: A wff $\mathcal{A}$ of S is provable if, and only if:

– $\mathcal{A}$ is a logical axiom of S; or

– $\mathcal{A}$ is a proper axiom of S; or

– $\mathcal{A}$ is the final wff of a finite sequence of wffs of S such that each formula of the sequence is:

  - either an axiom of S,
  - or is a provable formula of S by application of the rules of inference of S to the formulas preceding it in the sequence.

Moreover, we define a first-order theory S with quantifiers as *well-defined model-theoretically* if, and only if, it has a *well-defined model* in the sense of the proposed Definitions 21.3 to 21.7.

Hilbert's Second Problem:[1] In this investigation, we treat Hilbert's intent[2] behind the enunciation of his Second Problem as essentially seeking a finitary proof for the consistency of arithmetic when formalised in a language such as the first order Peano Arithmetic PA.

Interpretation (we essentially follow the definitions in [**Me64**], p.49): An *interpretation* of the:

- predicate letters;
- function letters; and
- individual and logical constants;

of a formal system S consists of:

- a non-empty set $\boldsymbol{D}$, called the *domain* of the interpretation;

and an *evidence-based* assignment:

- to each predicate letter A_j^n of an n-place relation in $\boldsymbol{D}$;
- to each function letter f_j^n of an n-place operation in $\boldsymbol{D}$ (i.e., a function from $\boldsymbol{D}$ into $\boldsymbol{D}$); and
- to each individual constant a_i of some fixed element of $\boldsymbol{D}$.

Given such an interpretation, variables are thought of as ranging over the set $\boldsymbol{D}$, and $\neg, \rightarrow$, and quantifiers are given their usual meaning.

Moreover, we define an interpretation as *well-defined* if, and only if, all the above assignments are *well-defined* in the sense of the proposed Definitions 21.3 to 21.7.

Model (we essentially follow the definitions in [**Me64**], p.49): An interpretation $\mathcal{I}$ defines a *model* of a formal system S if, and only if, there is a set of rules—which we define as the *model-theoretic logic* of S—that assign *evidence-based* truth values of 'satisfaction', 'truth', and 'falsity' to the formulas of S under $\mathcal{I}$ such that the axioms of S interpret as 'true' under $\mathcal{I}$, and the rules of inference of S preserve such 'truth' under $\mathcal{I}$.

Moreover, we define a model as *well-defined* if, and only if, it is defined by a *well-defined* interpretation in the sense of the proposed Definitions 21.3 to 21.7.

---

[1] "When we are engaged in investigating the foundations of a science, we must set up a system of axioms which contains an exact and complete description of the relations subsisting between the elementary ideas of that science. . . . But above all I wish to designate the following as the most important among the numerous questions which can be asked with regard to the axioms: To prove that they are not contradictory, that is, that a definite number of logical steps based upon them can never lead to contradictory results. In geometry, the proof of the compatibility of the axioms can be effected by constructing a suitable field of numbers, such that analogous relations between the numbers of this field correspond to the geometrical axioms. . . . On the other hand a direct method is needed for the proof of the compatibility of the arithmetical axioms."

. . . Excerpted from Maby Winton Newson's English translation [**Nw02**] *of Hilbert's address* [**Hi00**] *at the International Congress of Mathematicians in Paris in 1900.*

[2] Compare Curtis Franks' thesis in [**Fr09**] that Hilbert's intent behind the enunciation of his Second Problem was essentially to establish the autonomy of arithmetical truth without appeal to any debatable philosophical considerations.

ω-consistency: A formal system S is ω-consistent if, and only if, there is no S-formula $[F(x)]$ for which, first, $[\neg(\forall x)F(x)]$ is S-provable and, second, $[F(a)]$ is S-provable for any specified S-term $[a]$.

Partial recursive: Classically, a partial function F of n arguments is called partial recursive if, and only if, F can be obtained from the initial functions (zero function), projection functions, and successor function (of classical recursive function theory) by means of substitution, recursion and the classical, unrestricted, μ-operator. F is said to come from G by means of the unrestricted μ-operator, where $G(x_1, \ldots, x_n, y)$ is recursive, if, and only if, $F(x_1, \ldots, x_n) = \mu y(G(x_1, \ldots, x_n, y) = 0)$, where $\mu y(G(x_1, \ldots, x_n, y) = 0)$ is the least number k (if such exists) such that, if $0 \leq i \leq k, G(x_1, \ldots, x_n, i)$ exists and is not 0, and $G(x_1, \ldots, x_n, k) = 0$. We note that, classically, F may not be defined for certain n-tuples; in particular, for those n-tuples $(x_1, \ldots, x_n)$ for which there is no y such that $G(x_1, \ldots, x_n, y) = 0$ (cf. [**Me64**], p.120-121).

Tarski's inductive definitions: We shall assume that truth values of 'satisfaction', 'truth', and 'falsity' are assignable inductively to the compound formulas of a first-order theory S under the interpretation $\mathcal{I}_{S(\mathbb{D})}$ in terms of *only* the satisfiability of the atomic formulas of S over $\mathbb{D}$ as usual (see [**Me64**], p.51; [**Mu91**]):

- A denumerable sequence s of $\mathbb{D}$ satisfies $[\neg A]$ under $\mathcal{I}_{S(\mathbb{D})}$ if, and only if, s does not satisfy $[A]$;
- A denumerable sequence s of $\mathbb{D}$ satisfies $[A \rightarrow B]$ under $\mathcal{I}_{S(\mathbb{D})}$ if, and only if, either it is not the case that s satisfies $[A]$, or s satisfies $[B]$;
- A denumerable sequence s of $\mathbb{D}$ satisfies $[(\forall x_i)A]$ under $\mathcal{I}_{S(\mathbb{D})}$ if, and only if, specified any denumerable sequence t of $\mathbb{D}$ which differs from s in at most the i'th component, t satisfies $[A]$;
- A well-formed formula $[A]$ of $\mathbb{D}$ is true under $\mathcal{I}_{S(\mathbb{D})}$ if, and only if, specified any denumerable sequence t of $\mathbb{D}$, t satisfies $[A]$;
- A well-formed formula $[A]$ of $\mathbb{D}$ is false under $\mathcal{I}_{S(\mathbb{D})}$ if, and only if, it is not the case that $[A]$ is true under $\mathcal{I}_{S(\mathbb{D})}$.

Total: We define a number-theoretic function, or relation, as total if, and only if, it is effectively computable, or effectively decidable, respectively, for any given set of natural number values assigned to its free variables. We define a number-theoretic function, or relation, as partial otherwise. We define a partial number theoretic function, or relation, as effectively computable, or decidable, respectively, if, and only if, it is effectively computable, or decidable, respectively, for any given set of values assigned to its free variables for which it is defined (cf. [**Me64**], p.214).

***Weak* standard interpretation of PA** (cf. [**Me64**], p.107): The *weak* standard interpretation $\boldsymbol{M}$ of PA over the domain $\mathbb{N}$ of the natural numbers is the one in which the logical constants have their 'usual' interpretations in the first-order predicate logic FOL, and:

(a) The set of non-negative integers is the domain;

(b) The symbol [0] interprets as the integer 0;

(c) The symbol $[']$ interprets as the successor operation (addition of 1);

(d) The symbols [+] and [$\star$] interpret as ordinary addition and multiplication;

(e) The symbol [=] interprets as the identity relation.

Comment: In this investigation, unless explicitly specified otherwise, we do not assume that Aristotle's particularisation holds under the the standard interpretation $\boldsymbol{M}$ of PA or under any interpretation of FOL.

Reason: Contrary to what is *implicitly* suggested in standard literature and texts—Aristotle's particularisation does not form any part of Tarski's *inductive* definitions of the satisfaction, and truth, of the formulas of PA under the standard interpretation $\boldsymbol{M}$ of PA, but is an extraneous, generally *implicit*, assumption in the underlying first-order logic FOL.

Moreover, its inclusion not only makes $\boldsymbol{M}$ non-finitary (as argued by Brouwer in [**Br08**]) but, as we show (Corollary 15.11), the assumption of Aristotle's particularisation does not hold in any model of PA (and, ipso facto, of FOL)!

***Weak* standard model of PA**: The *weak* standard model of PA is the one defined by the classical standard interpretation $\boldsymbol{M}$ of PA over the domain $\mathbb{N}$ of the natural numbers.

APPENDIX B

Rosser's Rule C

(*Excerpted from Mendelson* [**Me64**]*, p.73-74, §7, Rule C* [1])

It is very common in mathematics to reason in the following way. Assume that we have proved a wf of the form $(Ex)\mathcal{A}(x)$. Then, we say, let b be an object such that $\mathcal{A}(b)$. We continue the proof, finally arriving at a formula which does not involve the arbitrarily chosen element b. ...

In general, any wf which can be proved using arbitrary acts of choice, can also be proved without such acts of choice. We shall call the rule which permits us to go from $(Ex)\mathcal{A}(x)$ to $\mathcal{A}(b)$, Rule C ("C" for "choice"). More precisely, the definition of a Rule C deduction in a first-order theory K is as follows:

$\Gamma \vdash_c \mathcal{A}$ if and only if there is a sequence of wfs $\mathcal{B}_1, \ldots, \mathcal{B}_n = \mathcal{A}$

such that the following four statements hold.

(I) For each i, either

  (i) $\mathcal{B}_i$ is an axiom of K, or

  (ii) $\mathcal{B}_i$ is in Γ, or

  (iii) $\mathcal{B}_i$ follows by MP or Gen from preceding wfs in the sequence, or

  (iv) There is a preceding wf $(Ex)\mathcal{C}(x)$ and $\mathcal{B}_i$ is $\mathcal{C}(d)$, where d is a new individual constant. (Rule C)

(II) As axioms in (I)(i), we can also use all logical axioms involving the new individual constants already introduced by applications of (I)(iv), Rule C.

(III) No application of Gen is made using a variable which is free in some $(Ex)\mathcal{C}(x)$ to which Rule C has been previously applied.

(IV) $\mathcal{A}$ contains none of the new individual constants introduced in any application of Rule C.

(*Fn.† The first formulation of a version of Rule C similar to that given here seems to be due to Rosser (*[**Ro53**]*, pp.127-130).*)

[1] But see also [**Ro53**], pp.127-130.

APPENDIX C

Acknowledgement

C.1. If I have seen a little further it is by standing on the shoulders of Giants

Prior to Isaac Newton's above tribute to René Descartes and Robert Hooke, in a letter to the latter, it was reportedly the 12th century theologian and author, John of Salisbury, who was recorded as having used an even earlier version of this humbling admission—in a treatise on logic called Metalogicon, written in Latin in 1159, the gist of which is translatable as:

> "Bernard of Chartres used to say that we are like dwarfs on the shoulders of giants, so that we can see more than they, and things at a greater distance, not by virtue of any sharpness of sight on our part, or any physical distinction, but because we are carried high and raised up by their giant size.
>
> (Dicebat Bernardus Carnotensis nos esse quasi nanos, gigantium humeris insidentes, ut possimus plura eis et remotiora videre, non utique proprii visus acumine, aut eminentia corporis, sed quia in altum subvenimur et extollimur magnitudine gigantea.)"

Contrary to a frequent interpretation of the remark:

- 'Standing on the shoulders of Giants'

as describing:

- 'Building on previous discoveries',

it seems to me that what Bernard of Chartres apparently intended was to suggest that it doesn't necessarily take a genius to see farther; only someone both humble and willing to:

- First, clamber onto the shoulders of a giant and have the self-belief to see things at first-hand as they appear from a higher perspective (achieved more by the nature of height—and the curvature of our immediate space as implicit in such an analogy—than by the nature of genius); and,
- Second, avoid trying to see things first through the eyes of the giant upon whose shoulders one stands—for the giant might indeed be a vision-blinding genius!

C.2. Challenge it

It was this latter lesson that I was incidentally taught by—and one of the few that I learnt (probably far too well for better or worse) from—one of my Giants, the late Professor Manohar S. Huzurbazaar, in my final year of graduation in 1963 at the Institute of Science, Mumbai.

The occasion: I protested that the axiom of infinity (in the set theory course that he had just begun to teach us) was not self-evident to me, as (I had heard him explain in his introductory lecture) an axiom should seem if a formal theory were to make any kind of coherent sense under interpretation.

Whilst clarifying that his actual instruction to us had not been that an axiom should necessarily seem self-evident, but only that it should be treated as self-evident, Professor Huzurbazar further agreed that the set-theoretical axiom of infinity was not really as self-evident as an axiom ideally ought to seem in order to be treated as self-evident.

To my natural response asking him if it seemed at all self-evident to him, he replied in the negative; adding, however, that he believed it to be true despite its lack of an unarguable element of self-evidence.

It was his remarkably candid response to my incredulous—and youthfully indiscreet—query as to how an unimpeachably objective person such as he (which was his defining characteristic) could hold such a subjective belief that has shaped my thinking ever since.

He said that he had *had* to believe the axiom to be true, since he could not teach us what he did with conviction if he did not have such faith!

Although I did not grasp it then, over the years I came to the realisation that committing to such a belief was the price he had willingly paid for a responsibility that he had recognised—and accepted—consciously at a very early age in his life (when he was tutoring his school going nephew, the renowned physicist Jayant V. Narlikar):

> ***Nature had endowed him with the rare gift shared by great teachers—the capacity to reach out to, and inspire, students to learn beyond their instruction!***

It was a responsibility that he bore unflinchingly and uncompromisingly, eventually becoming one of the most respected and sought after teachers (of his times in India) of Modern Algebra (now Category Theory), Set Theory and Analysis at both the graduate and post-graduate levels in the University of Mumbai.

At the time, however, Professor Huzurbazar pointedly stressed that his belief should not influence me into believing the axiom to be true, nor into holding it as self-evident.

His words—spoken softly as was his wont—were:

> Challenge it.

Although I eventually elected not to follow an academic career, Professor Huzurbazar never faltered in encouraging me to question the accepted paradigms of the day when I shared the direction of my reading and thinking (particularly on Logic and the Foundations of Mathematics) with him on the few occasions that I met him over the next twenty years.

Moreover, even if the desirable *evidence-based* nature of the most fundamental axioms of mathematics (those of the first-order Peano Arithmetic PA that form the focus of this investigation) is finally accepted as formally inconsistent with a belief in the classical 'self-evident' truth of any axiom of infinity (as suggested, for instance, by the anomaly in Goodstein's argument highlighted in §22.2, Theorem 22.3), I believe that the shades of Professor Huzurbazaar would feel more liberated than bruised by the 'fall'.

And finally, if this investigation has any underlying guiding philosophy, it derives from what was once quoted to me in our early years by another of my Giants—my late friend, erstwhile classmate, and mentor, Ashok Chadha:

> "Let not posterity judge us as having spent our lives polishing the pebbles, and tarnishing the diamonds."
> . . . *Anonymous.*

Bibliography

[AABG] Diederik Aerts, Sven Aerts, Jan Broekaert and Liane Gabora. 2000. *The Violation of Bell Inequalities in the Macroworld.* In *Foundations of Physics.*, September 2000, Volume 30, Issue 9, pp.1387-1414.
[https://doi.org/10.1023/A:1026449716544]

[Ae98] Diederik Aerts, 1998. *The hidden measurement formalism: what can be explained and where quantum paradoxes remain.* In *International Journal of Theoretical Physics*, January 1998, Volume 37, Issue 1, pp.291-304.
[https://doi.org/10.1023/A:1026670802579]

[AKS04] Manindra Agrawal, Neeraj Kayal, and Nitin Saxena. 2004. *PRIMES is in P*. In *Annals of Mathematics*. Volume 160, Issue 2, pp. 781793.
[http://annals.math.princeton.edu/2004/160-2/p12]

[And72] Philip W. Anderson,/ 1972./ *More Is Different: Broken symmetry and the nature of the hierarchical structure of science.* In *Science*, 4th August 1972, Volume 177, Number 4047, American Association for the Advancement of Science.

[Ar09] Sanjeev Arora and Boaz Barak. 2008. *Computational Complexity: A Modern Approach*. 2009. Princeton University Press, New Jersey, USA.
[http://www.cs.princeton.edu/theory/complexity/]

[AR02a] Steve Awodey and Erich H. Reck. 2002. *Completeness and Categoricity. Part I: Nineteenth-century Axiomatics to Twentieth-century Metalogic*. In *History and Philosophy of Logic*, 23 (2002), 1-30, Taylor & Francis.
[http://www.faculty.ucr.edu/ reck/Awodey%20&%20Reck-CC,%20Part%20I.pdf]

[AR02b] . . . 2002. *Completeness and Categoricity. Part II: Nineteenth-century Axiomatics to Twentieth-century Metalogic*. In *History and Philosophy of Logic*, 23 (2002), 77-94, Taylor & Francis.
[http://www.faculty.ucr.edu/ reck/Awodey%20&%20Reck-CC,%20Part%20II.pdf]

[At17] Mark van Atten. 2017. *Predicativity and parametric polymorphism of Brouwerian implication.* Revised version of papers presented at the ASL meeting in Seattle in April 2017, at the workshop '*Critical Views of Logic*' in Oslo in August 2017, at the '*Journées SND*' in Paris in September 2017 and at the meeting '*Axiomatic Thinking*' in Lisbon in October 2017.
[https://www.academia.edu/34894973/Predicativity_and_parametric_polymorphism_of_Brouwerian_implication]

[At18] . . . . 2018. *The Creating Subject, the Brouwer-Kripke Schema, and infinite proofs.* Archived at arXiv:1805.00404v1[math.HO].
[https://arxiv.org/pdf/1805.00404v1.pdf]

[AT03] Mark van Atten and Robert Tragesser. 2003. *Mysticism and Mathematics: Brouwer, Gödel, and the Common Core Thesis.* In *Essays on Gödel's Reception of Leibniz, Husserl, and Brouwer,* Chapter 10, pp.173-187. Volume author: Mark van Atten. Part of the *Logic, Epistemology, and the Unity of Science* book series (LEUS, volume 35), Springer, Cham.
https://doi.org/10.1007/978-3-319-10031-9_10

[Aw18] Jon Awbrey. 2018. *Sign Relations, Triadic Relations, Relations • 8*. Blogpost in *Inquiry into Inquiry*, July 6, 2018.

https://inquiryintoinquiry.com/2018/07/06/sign-relations-triadic-relations-relations-%e2%80%a2-8/#comments

[Ba05] Andrej Bauer. 2005. *Realizability as the connection between computable and constructive mathematics*, Proceedings of CCA 2005, CiteSeerX.
[http://citeseerx.ist.psu.edu/viewdoc/citations?doi=10.1.1.392.8358]

[Ba16] ... 2016. *Five Stages of Accepting Constructive Mathematics*, Bull. Amer. Math. Soc. (N. S.), electronically published on October 3, 2016. DOI: https://doi.org/10.1090/bull/1556.
https://doi.org/10.1090/bull/1556

[Bar88] Michael Barnsley. 1988. *Fractals Everywhere.* Academic Press, Inc. San Diego.

[BBJ03] George S. Boolos, John P. Burgess, Richard C. Jeffrey. 2003. *Computability and Logic.* 4th ed., Cambridge University Press, Cambridge.

[BCST] Itzhak Bars, Shih-Hung Chen, Paul J. Steinhardt, and Neil Turok. 2012. *Antigravity and the big crunch/big bang transition.* In *Physics Letters B*, Volume 715, Issues 1-3, (2012), pp.278281, Elsevier B. V.
https://doi.org/10.1016/j.physletb.2012.07.071

[Be59] Evert W. Beth. 1959. *The Foundations of Mathematics.* Studies in Logic and the Foundations of Mathematics. Edited by L. E. J. Brouwer, E. W. Beth, A. Heyting. 1959. North Holland Publishing Company, Amsterdam.

[BF58] Paul Bernays and Abraham A. Fraenkel. 1958. *Axiomatic Set Theory.* Studies in Logic and the Foundations of Mathematics. Edited by L. E. J. Brouwer, E. W. Beth, A. Heyting. 1959. North Holland Publishing Company, Amsterdam.

[Bi18] Errett Bishop. 19xx. *How to Compile Mathematics into Algol.* Manuscript (last modified 04/05/2018) retrieved from the Department of Computer Science, University of Birmingham, UK.
[http://www.cs.bham.ac.uk/ mhe/Bishop/Algol.pdf]

[Bl64] John Stewart Bell. 1964. *On the Einstein Podolsky Rosen Paradox*. Physics Vol 1, No. 3, pp.195-200, 1964. Reprinted in *J. S. Bell, Speakable and unspeakable in quantum mechanics*, Cambridge, 2004, p. 14-21.
[http://philoscience.unibe.ch/documents/TexteHS10/bell1964epr.pdf]

[Bo52] David Bohm. 1952. *A Suggested Interpretation of the Quantum Theory in Terms of 'Hidden Variables', I.* Physical Review 85 n.2, pp.166-179 (1952) and *A Suggested Interpretation of the Quantum Theory in Terms of 'Hidden Variables', II.* Physical Review 85 n.2, pp.180-193 (1952).

[Boh49] Neils Bohr. 1949. *Discussions with Einstein on Epistemological Problems in Atomic Physics.* In Schlipp, P. A. (Ed.). *Albert Einstein: Philosopher-Scientist.* The Library of Living Philosophers, Volume VII, 1949, Tudor Publishing Co. (1951), New York.
[http://www.marxists.org/reference/subject/philosophy/works/dk/bohr.htm]

[Bomb] E. Bombieri. *Problems of the Millennium: The Riemann Hypothesis*. Official description provided for the Clay Mathematical Institute, Cambridge, Massachusetts.
[http://www.claymath.org/sites/default/files/official_problem_description.pdf]

[Bor71] Max Born. 1971. *The BORN-EINSTEIN Letters.* The Macmillan Press Ltd., London.
https://ia800700.us.archive.org/33/items/TheBornEinsteinLetters/Born-TheBornEinsteinLetters.pdf

[Bov00] Andrey I. Bovykin. 2000. *On order-types of models of arithmetic.* Thesis submitted to the University of Birmingham for the degree of Ph.D. in the Faculty of Science, School of Mathematics and Statistics, The University of Birmingham, Edgbaston, Birmingham, B15 2TT, United Kingdom, 13 April 2000.

[BPS88] Michael F. Barnsley, Robert L. Devaney, Benoit Mandelbrot, Heinz-Otto Peitgen Dietmar Saupe, Richard F. Voss, et al. 1988. *The Science of Fractal Images.* Editors: Heinz-Otto Peitgen and Dietmar Saupe. 1988. Springer-Verlag, New York Inc., USA.

[Br08] L. E. J. Brouwer. 1908. *The Unreliability of the Logical Principles.* English translation in A. Heyting, Ed. L. E. J. Brouwer: Collected Works 1: Philosophy and Foundations of Mathematics. Amsterdam: North Holland / New York: American Elsevier (1975): pp.107-111.

[Br13] . . . 1913. *Intuitionism and Formalism.* Inaugural address at the University of Amsterdam, October 14, 1912. Translated by Professor Arnold Dresden for the Bulletin of the American Mathematical Society, Volume 20 (1913), pp.81-96. 1999. Electronically published in Bulletin (New Series) of the American Mathematical Society, Volume 37, Number 1, pp.55-64.

[Br23] . . . 1923. *On the significance of the principle of the excluded middle in mathematics, especially in function theory.* Address delivered on 21 September 1923 at the annual convention of the Deutsche Mathematiker-Vereinigung in Marburg an der Lahn. In Jean van Heijenoort. 1967. Ed. From Frege to Gödel: A source book in Mathematical Logic, 1878 - 1931. Harvard University Press, Cambridge, Massachusetts.

[Bri93] Selmer Bringsjord. 1993. *The Narrational Case Against Church's Thesis*. Easter APA meetings, Atlanta.
http://www.rpi.edu/ brings/SELPAP/CT/ct/ct.html

[Brm07] Manuel Bremer. 2007. *Varieties of Finitism.* In *Metaphysica.* October 2007, Volume 8, Issue 2, pp 131-148. Springer, Netherlands.

[Brn00] Richard P. Brent. 2000. *Recent Progress and Prospects for Integer Factorisation Algorithms.* In *Computing and Combinatorics*, Lecture Notes in Computer Science, Volume 1858, 2000, pp.3-22, Springer, New York/Heidelberg.
http://pub.math.leidenuniv.nl/ edixhovensj/public_html_rennes/cours/brent.pdf

[Bu10] Jeff Buechner. 2010. *Are the Gödel incompleteness theorems limitative results for the neurosciences*? Journal of Biological Physics 36(1):23-44, Netherlands. DOI: 10.1007/s10867-009-9160-1
https://rd.springer.com/article/10.1007%2Fs10867-009-9160-1

[Ca37] Rudolf Carnap. 1937. *The Logical Syntax of Languge.* Reprinted in 1959 from the first English translation in 1937 by Routledge and Kegan Paul Ltd., London. Littlefield, Adams & Co., New Jersey.

[Ca62] . . . 1962. *On the use of Hilbert's ε-operator in scientific theories.* In *Essays on the Foundations of Mathematics.* Edited by Y. Bar-Hillel, E. I. J. Posnanski, M. O. Rabin, and A. Robinson for The Hebrew University of Jerusalem. 1962. North Holland Publishing Company, Amsterdam: pp. 156-164.

[Ca62a] . . . 1962. *Logical Foundations of Probability.* Second Edition 1962. The University of Chicago Press, Chicago, Illinois, U.S.A.

[Cai07] Andrés Eduardo Caicedo. 2007. *Goodstein's Theorem.* Revista Colombiana de Matemáticas, Volume 41(2007)2, pp. 381-391.
http://emis.impa.br/EMIS/journals/RCM/Articulos/878.pdf

[CCS01] Cristian S. Calude, Elena Calude and Solomon Marcus. 2001. *Passages of Proof.* Workshop, Annual Conference of the Australasian Association of Philosophy (New Zealand Division), Auckland.
http://arxiv.org/pdf/math/0305213.pdf

[Ch36] Alonzo Church. 1936. *An unsolvable problem of elementary number theory.* In M. Davis (ed.). 1965. *The Undecidable.* Raven Press, New York. Reprinted from the Am. J. Math., Vol. 58, pp.345-363.

[Che42] Lord Cherwell. 1942. *Number of Primes and Probability Considerations*. In *Nature* **150**, 120-121 (25 July 1942), p. 121. doi:10.1038/150120a0
http://www.nature.com/nature/journal/v150/n3795/pdf/150120a0.pdf

[Chr97] G. Chardin. 1997. *Motivations for antigravity in General Relativity.* In *Hyperfine Interactions.* 1997. Volume 109, Issue 1-4, pp.83-94. Kluwer Academic Publishers.
https://doi.org/10.1023/A:1012688930074

[Co63] Paul J. Cohen. 1963. *The Independence of the Continuum Hypothesis I.* Proceedings of the U. S. National Academy of Sciences. 50, 1143-1148, 1963.

[Co64] ... 1964. *The Independence of the Continuum Hypothesis II.* Proceedings of the National Academy of Sciences of the United States of America, 51(1), 105-110, 1964.

[Co66] ... 1966. *Set Theory and the Continuum Hypothesis.* (Lecture notes given at Harvard University, Spring 1965) W. A. Benjamin, Inc., New York.

[Co66] ... 1966. *Set Theory and the Continuum Hypothesis.* Lecture notes given at Harvard University, Spring 1965. W. A. Benjamin, Inc., New York.

[Cook] Stephen Cook. 2000. *The P versus NP Problem*. Official description provided for the Clay Mathematical Institute, Cambridge, Massachusetts.
http://www.claymath.org/sites/default/files/pvsnp.pdf

[Cor73] John Corcoran. 1973. *Gaps Between Logical Theory and Mathematical Practice.* In M. Bunge (ed.). *The Methodological Unity of-Science*, 23-50. 1973. D. Reidel Publishing Company, P.O. Box 17, Dordrecht, Holland.
https://philpapers.org/rec/CORGBL

[Cr05] John N. Crossley. 2005. *What is Mathematical Logic?* A Survey. Address at the First Indian Conference on Logic and its Relationship with Other Disciplines held at the Indian Institute of Technology, Powai, Mumbai from January 8 to 12. Reprinted in *Logic at the Crossroads: An Interdisciplinary View - Volume I* (pp.3-18). ed. Amitabha Gupta, Rohit Parikh and Johan van Bentham. 2007. Allied Publishers Private Limited, Mumbai, India.

[Ct75] Gregory J. Chaitin. 1975. *A Theory of Program Size Formally Identical to Information Theory.* In *Journal of the ACM (JACM)* Volume 22 Issue 3, July 1975, pp. 329-340. Association for Computing Machinery, New York, NY, USA.
https://dl.acm.org/citation.cfm?id=321894

[Ct82] ... 1982. *Gödel's Theorem and Information.* International Journal of Theoretical Physics (1982) 21, pp. 941-954. Kluwer Academic Publishers-Plenum Publishers.
https://doi.org/10.1007/BF02084159

[Da82] Martin Davis. 1958. *Computability and Unsolvability.* 1982 ed. Dover Publications, Inc., New York.

[Da95] .... 1995. *Is mathematical insight algorithmic?* In *Behavioral and Brain Sciences*, 13 (4), 659-60.

[De17] Daniel C. Dennett. 2017. *A Difference That Makes a Difference.* In *Edge*, 22nd November 2017.
https://www.edge.org/conversation/daniel_c_dennett-a-difference-that-makes-a-difference

[Di70] Jean Dieudonné. 1970. *The Work of Nicholas Bourbaki.* In *The American Mathematical Monthly*, vol. 77, 1970, pp. 134-145.

[Dic52] Leonard Eugene Dickson. 1952. *History of the Theory of Numbers: Volume I.* Chelsea Publishing Company, New York, N. Y.

[Dir37] Peter Gustav Lejeune Dirichlet. 1837. *There are infinitely many prime numbers in all arithmetic progressions with first term and difference coprime.* Originally read to The Royal Prussian Academy of Sciences on the 27th of July, 1837; published in *Abhandlungen der Königlichen Preußischen Akademie der Wissenschaften zu Berlin*, 1837, 4581. *Citation source*: English translation and arXive on 24/11/2014 by Ralf Stephan.
http://arXiv:0808.1408v2

[EC89] Richard L. Epstein, Walter A. Carnielli. 1989. *Computability: Computable Functions, Logic, and the Foundations of Mathematics.* Wadsworth & Brooks, California.

[Ed03] Jim Edwards. 2003. *Reduction and Tarski's Definition of Logical Sequence.* In *Notre Dame Journal of Formal Logic*, Volume 44, Number 1, 2003.
http://projecteuclid.org/download/pdfview_1/euclid.ndjfl/1082637614

[Ei49] Albert Einstein. 1949. *Remarks to the Essays Appearing in this Collective Volume.* In Schlipp, P. A. (Ed.). —textitAlbert Einstein: Philosopher-Scientist. The Library of Living Philosophers, Volume VII, 1949, Tudor Pub. Co. (1951), New York.
http://www.marxists.org/reference/subject/philosophy/works/ge/einstein.htm

[El79a] P. D.T. A. Elliott. 1979. *Probabilistic Number Theory I.* Springer-Verlag, New York.

[El79b] P. D.T. A. Elliott. 1979. *Probabilistic Number Theory II.* Springer-Verlag, New York.

[EPR35] Albert Einstein, Boris Yakovlevich Podolsky, Nathan Rosen. 1935. *Can Quantum-Mechanical Description of Physical Reality be Considered Complete?*. Physical Review 47 (10): 777-780. Bibcode 1935PhRv…47...777E. doi:10.1103/PhysRev.47..777.

[Fe92] Solomon Feferman. 1992. *What rests on what: The proof-theoretic analysis of mathematics*. Invited lecture, 15th International Wittgenstein Symposium: *Philosophy of Mathematics*, held in Kirchberg/Wechsel, Austria, 16-23 August 1992.
http://math.stanford.edu/ feferman/papers/whatrests.pdf

[Fe97] … 1997. *The significance of Hermann Weyl's 'Das Kontinuum'.* The second of Solomon Feferman's three lectures for the conference, *Proof Theory: History and Philosophical Significance*, held at the University of Roskilde, Denmark Oct. 31-Nov. 1, 1997.
http://math.stanford.edu/ feferman/papers/DasKontinuum.pdf

[Fe99] … 1999. *Does Mathematics Need New Axioms?* In *The American Mathematical Monthly*, Vol. 106, No. 2, Feb., 1999, pp. 99-111, DOI: 10.2307/2589047.
http://www.jstor.org/stable/2589047

[Fe02] … 2002. *Predicativity.*
http://math.stanford.edu/ feferman/papers/predicativity.pdf.

[Fe06] … 2006. *Are There Absolutely Unsolvable Problems? Gödel's Dichotomy.* Philosophia Mathematica (2006) 14 (2): 134-152.
http://math.stanford.edu/ feferman/papers/dichotomy.pdf

[Fe08] … 2008. *Lieber Herr Bernays!, Lieber Herr Gödel! Gödel on finitism, constructivity and Hilbert's program.* in *Special Issue: Gödel's dialectica Interpretation* Dialectica, Volume 62, Issue 2, June 2008, pp. 245-290.
http://math.stanford.edu/ feferman/papers/bernays.pdf

[Fey85] Richard P. Feynman. 1985. *Judging Books by Their Covers* in *Surely You're Joking, Mr. Feynman! (Adventures of a curious character)*. Norton, New York.
http://www.textbookleague.org/103feyn.htm

[FFMS] Solomon Feferman, Harvey M. Friedman, Penelope Maddy and John R. Steel. 2000. *Discussion: Does Mathematics Need New Axioms?* In *The Bulletin of Symbolic Logic*, Vol. 6, No. 4 (Dec., 2000), pp. 401-446, DOI: 10.2307/420965.
http://www.jstor.org/stable/420965

[FP00] Juliet Floyd & Hilary Putnam. 2000. *A Note on Wittgenstein's 'Notorious Paragraph' about the Gödel Theorem.* In *The Journal of Philosophy* 45, 11: 624-632.

[Fr09] Curtis Franks. 2009. *The Autonomy of Mathematical Knowledge: Hilbert's Program Revisited*. Cambridge University Press, New York.

[Fu42] W. H. Furry. 1942. *Number of Primes and Probability Considerations*. In *Nature* **150**, 120-121 (25 July 1942), p. 120, doi:10.1038/150120a0
http://www.nature.com/nature/journal/v150/n3795/pdf/150120a0.pdf

[Ga10] William Gasarch. 2010. *Theorems that you simply don't believe.* Computational Complexity blog, Thursday, March 11, 2010.
http://blog.computationalcomplexity.org/2010/03/theorems-that-you-simply-dont-believe.html

[Gal18] Brian Gallagher. 2018. *Are Suicide Bombings Really Driven By Ideology.* Blogpost in *Nautilus*, May 24, 2018.

[Gam18] David Gamez. 2018.*Human and Machine Consciousness.* Open Book Publishers, Cambridge, UK.https://doi.org/10.11647/OBP.0107
https://doi.org/10.11647/OBP.0107

[Gd44] R. L. Goodstein. 1944. *On the Restricted Ordinal Theorem.* J. Symb. Logic 9, 33-41.

[Go31] Kurt Gödel. 1931. *On formally undecidable propositions of Principia Mathematica and related systems I.* Translated from the German original by Elliott Mendelson and first published in M. Davis (ed.). 1965. *The Undecidable.* Raven Press, New York. In Dover edition 2004, pp.5-38, Dover Publications Inc., Mineola, NY 1150.

[Go33] Kurt Gödel. 1933. *On Intuitionistic Arithmetic and Number Theory.* Translated by Martin Davis from the German original. In M. Davis (ed.). 1965. *The Undecidable.* Raven Press, New York.

[Go40] . . . 1940. *The Consistency of the Axiom of Choice and of the Generalised Continuum-Hypothesis with the Axioms of Set Theory.* 1940. Princeton University Press, Princeton, New Jersey.

[Go51] . . . 1951. *Some basic theorems on the foundations of mathematics and their implications.* Gibbs lecture. In *Kurt Gödel, Collected Works III*, pp.304-323. 1995. *Unpublished Essays and Lectures.* Solomon Feferman et al (ed.). Oxford University Press, New York.

[Gow02] William Timothy Gowers. 2002. *Does Mathematics Need a Philosophy?* Talk presented in 2002 before the Cambridge University Society for the Philosophy of Mathematics and Mathematical Sciences (CUSPOMMS). Published in *18 Unconventional Essays on the Nature of Mathematics.* pp.182-200. Editors: Hersh Reuben (Ed.). 2006. Springer-Verlag New York.
https://www.dpmms.cam.ac.uk/ wtg10/philosophy.html

[Gr95] Andrew Granville. 1995. *Harald Cramér and the distribution of prime numbers*. Scandinavian Actuarial Journal, Volume 1995, Issue 1, pp.12-28. DOI:10.1080/03461238.1995.10413946
http://www.dms.umontreal.ca/ andrew/PDF/cramer.pdf

[GS97] Charles M. Grinstead and J. Laurie Snell. 2003. *Introduction to Probability*, *The CHANCE Project* Version dated 4 July 2006 of the Second Revised Edition, 1997, American Mathematical Society, Rhode Island, USA.
https://math.dartmouth.edu/ prob/prob/prob.pdf

[Ha40] G. H. Hardy. 1940. *A Mathematician's Apology.* Cambridge University Press, Cambridge, UK.

[HA28] David Hilbert & Wilhelm Ackermann. 1928. *Principles of Mathematical Logic.* Translation of the second edition of the *Grundzge Der Theoretischen Logik.* 1928. Springer, Berlin. 1950. Chelsea Publishing Company, New York.

[HB34] David Hilbert and Paul Bernays. 1934. *David Hilbert and Paul Bernays: Grundlagen der Mathematik.* First bilingual German-English (third) edition of Parts A, B, and C (Preface and sections 1-7 of Volume I). Edited by Claus-Peter Wirth, Jörg Siekmann, Michael Gabbay, and Don Gabbay for *The Hilbert Bernays Project.* Commented translation by Claus-Peter Wirth. 2011. College Publications, London.
http://wirth.bplaced.net/p/hilbertbernays/demos.html

[He04] Catherine Christer-Hennix. 2004. *Some remarks on Finitistic Model Theory, Ultra-Intuitionism and the main problem of the Foundation of Mathematics.* ILLC Seminar, 2nd April 2004, Amsterdam.

[Hei71] Werner Heisenberg. 1971. *Physics and beyond; encounters and conversations.* 1971. Harper & Row, New York.

[Hi00] David Hilbert. 1900. *Mathematical Problems.* An address delivered before the International Congress of Mathematicians at Paris in 1900. The French translation by M. L. Laugel "*Sur les problmes futurs des mathmatiques*" appeared in Compte Rendu du Deuxime Congrs International des Mathmaticiens, pp. 58-114, Gauthier-Villars, Paris, 1902. Dr. Maby Winton Newson translated this address into English with the author's permission for Bulletin of the American Mathematical Society 8 (1902), 437-479. A reprint appears in *Mathematical Developments Arising from Hilbert Problems*, edited by Felix Brouder, American Mathematical Society, 1976. The original address "*Mathematische Probleme*" appeared in Gttinger Nachrichten, 1900, pp. 253-297, and in Archiv der Mathematik und Physik, (3) 1 (1901), 44-63 and 213-237. [A fuller title of the journal Gttinger Nachrichten is Nachrichten von der Knigl. Gesellschaft der Wiss.

zu Gttingen.] An HTML version of Dr. Newson's translation was prepared for the web by D. Joyce with only minor modifications, mainly, more complete references.
https://mathcs.clarku.edu/ djoyce/hilbert/problems.html#prob2

[Hi25] ... 1925. *On the Infinite.* Text of an address delivered in Mnster on 4th June 1925 at a meeting of the Westphalian Mathematical Society. In Jean van Heijenoort. 1967.Ed. *From Frege to Gödel: A source book in Mathematical Logic, 1878 - 1931*. Harvard University Press, Cambridge, Massachusetts.

[Hi27] ... 1927. *The Foundations of Mathematics.* Text of an address delivered in July 1927 at the Hamburg Mathematical Seminar. In Jean van Heijenoort. 1967.Ed. *From Frege to Gödel: A source book in Mathematical Logic, 1878 - 1931*. Harvard University Press, Cambridge, Massachusetts.

[Hi30] ... 1930. *Die Grundlegung der elementaren Zahlenlehre.* In *Mathematische Annalen.* Vol. 104 (1930), pp. 485-494.

[HL23] G.H. Hardy and J.E. Littlewood. 1923. *Some problems of 'partitio numerorum:' III: On the expression of a number as a sum of primes*, Acta Mathematica, December 1923, Volume 44, pp.1-70.
http://fuchs-braun.com/media/8cdd73c813c342f8ffff80d1ffffff0.pdf

[Ho01] Wilfrid Hodges. 2001. *Tarski's Truth Definitions.* In *Stanford Encyclopedia of Philosophy.* (Winter 2001 Edition). Edward N. Zalta (ed.). Web essay.
http://plato.stanford.edu/entries/tarski-truth/

[Hos18] Sabine Hossenfelder. 2018. *The Overproduction Crisis in Physics and Why You Should Care About It.* Blogpost in *Backreaction*.
http://backreaction.blogspot.in/2018/05/the-overproduction-crisis-in-physics.html

[Hos18a] ... 2018. *Lost in Math: How Beauty Leads Physics Astray.* Basic Books, New York.

[HP98] Petr Hájek and Pavel Pudlák. 1998. *Metamathematics of First-Order Arithmetic.* Volume 3 of *Perspectives in Mathematical Logic* Series, ISSN 0172-6641. Springer-Verlag, Berlin, 1998.

[Hr97] Reuben Hersh. 1997. *What is Mathematics, Really?* Oxford University Press Inc., New York.

[HW60] G. H. Hardy and E. M. Wright. 1960. *An Introduction to the Theory of Numbers.* 4th edition. Clarendon Press, Oxford.

[Ka91] Richard Kaye. 1991. *Models of Peano Arithmetic.* Oxford Logic Guides, 15. Oxford Science Publications. The Clarendon Press, Oxford University Press, New York, 1991.

[Ka11] ... 2011. *Tennenbaum's theorem for models of arithmetic.* In *Set Theory, Arithmetic, and Foundations of Mathematics.* Eds. Juliette Kennedy & Roman Kossak. Lecture Notes in Logic (No. 36). pp.66-79. Cambridge University Press, 2011. Cambridge Books Online.
http://dx.doi.org/10.1017/CBO9780511910616.005

[Kac59] Mark Kac. 1959. *Statistical Independence in Probability, Analysis and Number Theory.* 1959. *The Carus Mathematical Monographs: Number Twelve* The Mathematical Association of America, Second Impression, 1964.
http://www.gibbs.if.usp.br/ marchett/estocastica/MarkKac-Statistical-Independence.pdf

[KKT91] Daniel Kahneman, Jack L. Knetsch and Richard H. Thaler. 1991. *Anomalies: The Endowment Effect, Loss Aversion, and Status Quo Bias.* In *The Journal of Economic Perspectives*, 5(1), pp. 193-206, Winter 1991, American Economic Association, Pittsburgh, USA.
https://www.aeaweb.org/articles?id=10.1257/jep.5.1.193

[Kl36] Stephen Cole Kleene. 1936. *General Recursive Functions of Natural Numbers.* Math. Annalen vol. 112 (1936) pp.727-766.

[Kl52] ... 1952. *Introduction to Metamathematics.* North Holland Publishing Company, Amsterdam.

[Km59] László Kalmár. 1959. *An Argument Against the Plausibility of Church's Thesis*. In Heyting, A. (ed.) *Constructivity in Mathematics*. North-Holland, Amsterdam.

[KMFLZ] Mario Krenn, Mehul Malik, Robert Fickler, Radek Lapkiewicz, and Anton Zeilinger. 2016. *Automated Search for new Quantum Experiments*. In *Physical Review Letters*, 4 March 2016, Phys. Rev. Lett.116, 090405.
https://journals.aps.org/prl/abstract/10.1103/PhysRevLett.116.090405

[Kn63] G. T. Kneebone. 1963. *Mathematical Logic and the Foundations of Mathematics: An Introductory Survey.* D. Van Norstrand Company Limited, London.

[Ko25] Andrei Nikolaevich Kolmogorov. 1925. *On the principle of excluded middle.* In Jean van Heijenoort. 1967. Ed. *From Frege to Gödel: A source book in Mathematical Logic, 1878 - 1931.* Harvard University Press, Cambridge, Massachusetts.

[Kol56] A. N. Kolmogorov. 1933. *Foundations of the Theory of Probability.* Second English Edition. Translation edited by Nathan Morrison. 1956. Chelsea Publishing Company, New York.
http://www.socsci.uci.edu/ bskyrms/bio/readings/kolmogorov_theory_of_probability_small.pdf

[Kr16] Stanislaw Krajewski. 2016. *Theological Metaphors in Mathematics.* In *Studies in Logic, Grammar and Rhetoric. The Journal of University of Bialystok.* 44 (57) 2016. Berlin: De Gruyter.
http://logika.uwb.edu.pl/studies/index.php?page=search&vol=57

[Krp13] Saul A. Kripke. 2013. *The Church-Turing "Thesis" as a Special Corollary of Gödel's Completeness Theorem.* In *Computability: Turing, Gödel, Church and Beyond.* Eds. B. Jack Copeland, Carl J. Posy and Oron Shagrir. First paperback edition, 2015, The MIT Press, Cambridge, Massachusetts, USA.

[KS06] Roman Kossak and James H. Schmerl. 2006. *The structure of models of Peano arithmetic*. Oxford Logic Guides, 50. Oxford Science Publications. The Clarendon Press, Oxford University Press, Oxford, 2006.

[La29] Yehezkel-Edmund Landau. 1929. *Foundations of Analysis.* 1951. Translation into English by F. Steinhardt of the German-language book *Grundlagen Der Analysis* by Edund Landau. 2nd Edn. 1960. Chelsea Publishing Company, New York.

[Lam17] Timm Lampert. 2017. *Wittgenstein and Gödel: An Attempt to Make "Wittgenstein's Objection" Reasonable.* In *Philosophia Mathematica.* nkx017.
https://doi.org/10.1093/philmat/nkx017

[Li64] A. H. Lightstone. 1964. *The Axiomatic Method.* Prentice Hall, NJ.

[Ln08] Laureano Luna. 2008. *On non-standard models of Peano Arithmetic*. The Reasoner, Vol(2)2 p7.
https://www.kent.ac.uk/secl/researchcentres/reasoning/TheReasoner/vol2/TheReasoner-2(2).pdf

[Lo15] Leopold Löwenheim. 1915. *On possibilities in the calculus of relatives*. In Jean van Heijenoort. 1967. Ed. *From Frege to Gödel: A source book in Mathematical Logic, 1878 - 1931.* Harvard University Press, Cambridge, Massachusetts.

[Lob59] Martin H. Löb. 1959. *Constructive Truth.* In Heyting, A. (ed.) *Constructivity in Mathematics.* North-Holland, Amsterdam.

[LP11] H. W. Lenstra Jr. and Carl Pomerance. 2011. *Primality testing with Gaussian periods.* Archived version 20110412, Dartmouth College, US.
https://www.math.dartmouth.edu/ carlp/aks041411.pdf

[LR00] George Lakoff and Rafael E. Núñez. 2000. *Where Mathematics Comes From.* Basic Books, NY, USA.

[Lu61] John Randolph Lucas. 1961. *Minds, Machines and Gödel*. Philosophy, XXXVI, 1961, pp.112-127; reprinted in *The Modeling of Mind.* Kenneth M.Sayre and Frederick J.Crosson, eds., Notre Dame Press, 1963, pp.269-270; and *Minds and Machines*, ed. Alan Ross Anderson, Prentice-Hall, 1964, pp.43-59.
http://users.ox.ac.uk/ jrlucas/Godel/mmg.html

[Luxx] ... 19xx. *The Gödelian Argument: Turn Over the Page.*
http://users.ox.ac.uk/ jrlucas/Godel/turn.html#r-10

[Lun17] Björn Lundgren. 2017. *Does semantic information need to be truthful?* In *Synthese*, October 2017. DOI:10.1007/s11229-017-1587-5.
https://doi.org/10.1007/s11229-017-1587-5

[Lus62] Eugene C. Luschei. 1958. *The Logical Systems of Lesniewski.* Studies in Logic and the Foundations of Mathematics. Edited by L. E. J. Brouwer, E. W. Beth, A. Heyting. 1959. North Holland Publishing Company, Amsterdam.

[Ma08] Penelope Maddy. 2008. *How Applied Mathematics Became Pure.* In *The Review of Symbolic Logic*, Volume 1, Number 1, June 2008.
http://www.socsci.uci.edu/ pjmaddy/bio/how%20applied%20math%20rsl.pdf

[Ma18] ... *What do we want a foundation to do? Comparing set-theoretic, category-theoretic, and univalent approaches.* To appear in S. Centrone, D. Kant, and D. Sarikaya, eds., *Reflections on Foundations: Univalent Foundations, Set Theory and General Thoughts.*
https://www.academia.edu/36650692/What_do_we_want_a_foundation_to_do_Comparing_set-theoretic_category-theoretic_and_univalent_approaches

[Ma18a] ... *Set-theoretic Foundations.* To appear in A. Caicedo et al, eds., *Foundations of Mathematics.* (Providence, RI: AMS).
https://www.academia.edu/23459457/Set-theoretic_Foundations

[Mar54] Markov, Andrey Andreevich. *The Theory of Algorithms.* 1960. American Mathematical Society Translations, series 2, 15, 1-14.

[Md01] James J. Madden. 2001. *Book Review: Where Mathematics Comes From.* Notices of the AMS, Volume 48, Number 10, November 2001, p.1182.
http://www.ams.org/notices/200110/rev-madden.pdf

[Me64] Elliott Mendelson. 1964. *Introduction to Mathematical Logic.* Van Norstrand, Princeton.

[Me90] ... 1990. *Second Thoughts About Church's Thesis and Mathematical Proofs.* Journal of Philosophy 87.5.

[MM01] V. Wiktor Marek and and Jan Mycielski. 2001. *Foundations of Mathematics in the Twentieth Century.* The American Mathematical Monthly, Vol. 108, No. 5 (May, 2001), pp. 449-468, Mathematical Association of America, DOI: 10.2307/2695803.
http://www.jstor.org/stable/2695803

[Mn77] Mandelbrot, Benoit B. 1977. *The Fractal Geometry of Nature.* W. H. Freeman and Company, New York.

[MS05] Maria Emilia Maietti and Giovanni Sambin. 2005. *Toward a minimalist foundation for constructive mathematics.* Clarendon Press, Oxford.

[Mt09] Maria Emilia Maietti. 2009. *A minimalist two-level foundation for constructive mathematics.* Dipartimento di Matematica Pura ed Applicata, University of Padova.

[Mu91] Chetan R. Murthy. 1991. *An Evaluation Semantics for Classical Proofs.* Proceedings of Sixth IEEE Symposium on Logic in Computer Science, pp. 96-109, (also Cornell TR 91-1213), 1991.
http://www.nuprl.org/documents/Murthy/EvaluationSemantics.pdf

[Mur06] Roman Murawski. 2006. *Troubles With (The Concept Of) Truth In Mathematics.* In *Logic and Logical Philosophy*, Volume 15 (2006), 285303. DOI: 10.12775/LLP.2006.017.
https://philpapers.org/rec/MURTWT

[Mycl] Jan Mycielski. *Russells Paradox and Hilberts (much forgotten) View of Set Theory.* Unpublished draft.
http://spot.colorado.edu/ jmyciel/

[Na08] Melvyn B. Nathanson. 2008. *Desperately Seeking Mathematical Truth.* Opinion in the August 2008 *Notices of the American Mathematical Society*, Vol. 55, Issue 7.

[NG91] Michael Martin Nieto and T. Goldman. *The arguments against "antigravity" and the gravitational acceleration of antimatter.* In *Physics Letters B*, Volume 205, Issue 5,

(1991), pp.221281, Elsevier B. V.
https://doi.org/10.1016/0370-1573(91)90138-C

[Nv64] P. S. Novikov. 1964. 1964. *Elements of Mathematical Logic.* Oliver & Boyd, Edinburgh and London.

[Nw02] Maby Winton Newson. 1902. *Mathematical Problems: Lecture delivered before the International Congress of Mathematicians at Paris in 1900 by Professor David Hilbert.* In *Bulletin of the American Mathematical Society* 8 (1902), 437-479. HTML version provided at:
http://aleph0.clarku.edu/ djoyce/hilbert/problems.html

[Pa71] Rohit Parikh. 1971. *Existence and Feasibility in Arithmetic.* In *The Journal of Symbolic Logic*, Vol.36, No. 3 (Sep., 1971), pp. 494-508.

[Pa08] . . . 2008. *Sentences, Belief and Logical Omniscience, or What Does Deduction Tell Us?* The Review of Symbolic Logic, Volume 1, Number 4, December 2008, pp. 459-476.
https://doi.org/10.1017/S1755020308090059

[Pas95] Charles Parsons. 1995. *Platonism and Mathematical Intuition in Kurt Gödel's Thought.* The Bulletin of Symbolic Logic, Volume 1, Number 1, March 1995, pp. 44-74.

[Pd17] Thanu Padmanabhan. 2017. *The Universe Began With a Big Melt, Not a Big Bang.* In *Nautilus,* Issue 053, October 5, 2017.
http://nautil.us/issue/53/monsters/the-universe-began-with-a-big-melt-not-a-big-bang

[Pd17a] . . . 2017. *Do We Really Understand the Cosmos?* Comptes Rendus Physique18, 275-291 (2017).
https://arXiv.org/pdf/1611.03505.pdf

[Pe90] Roger Penrose. 1990. *The Emperor's New Mind: Concerning Computers, Minds and the Laws of Physics.* 1990, Vintage edition. Oxford University Press.

[Pe94] . . . 1994. *Shadows of the Mind: A Search for the Missing Science of Consciousness.* Oxford University Press.

[Per17] David Pereplyotchik. 2017. *The Ontology, Epistemology and Methodology of Linguistics.* Article dated December 1, 2017 in '*The Brains Blog*'.
http://philosophyofbrains.com/2017/12/01/1-ontology-epistemology-methodology-linguistics.aspx

[Pol59] George Pólya. 1959. *Heuristic Reasoning in the Theory of Numbers*. In *The American Mathematical Monthly*, Vol. 66, No. 5 (May, 1959), pp. 375-384, DOI: 10.2307/2308748.
http://www.jstor.org/stable/2308748

[Pos13] Carl J. Posy. 2013. *Computability and Constructibility.* In *Computability: Turing, Gödel, Church and Beyond.* Eds. B. Jack Copeland, Carl J. Posy and Oron Shagrir. First paperback edition, 2015, The MIT Press, Cambridge, Massachusetts, USA.

[PR86] 1986. Peitgen, Hans-Otto & Richter, Peter H. 1986. *The Beauty of Fractals.* 1986. Springer-Verlag, Berlin.

[PZ97] Marian B. PourEl & Ning Zhong. 1997. *The wave equation with computable initial data whose unique solution is nowhere computable.* In *Mathematical Logic Quarterly*, 43 (4):499-509 (1997).
http://ac.els-cdn.com/0001870881900013/1-s2.0-0001870881900013-main.pdf?_tid=2d44769e-48d4-11e7-8597-00000aacb35d&acdnat=1496546358_92434c99000e7ba958e1882fe96eb670

[Qu53] Willard Van Orman Quine. 1953. *On What There Is.* In *From a Logical Point of View.* Harvard University Press, Cambridge, Massachusetts.

[Qu63] . . . 1963. *Set Theory and its Logic.* Harvard University Press, Cambridge, Massachusetts.

[Qn17] Paula Quinon. 2017. *Can Church's Thesis be Viewed as a Carnapian Explication?*. In *ResearchGate.*
https://www.researchgate.net/publication/316938844

[Re16] Kenneth W. Regan. (2016, June 29) *Getting to the Roots of Factoring.* In *Gödel's Lost Letter and P = NP*. Web log post.
https://rjlipton.wordpress.com/2016/06/29/getting-to-the-roots-of-factoring/

[Rg87] Hartley Rogers Jr. 1987. *Theory of Recursive Functions and Effective Computability.* MIT Press, Cambridge, Massachusetts.

[Ri74] Ian Richards. 1974. *On The Incompatibility Of Two Conjectures Concerning Primes; A Discussion Of The Use Of Computers In Attacking A Theoretical Problem.* Bulletin Of The American Mathematical Society, Volume 80, Number 3, May 1974.
http://projecteuclid.org/download/pdf_1/euclid.bams/1183535510

[Ro36] J. Barkley Rosser. 1936. *Extensions of some Theorems of Gödel and Church.* In M. Davis (ed.). 1965. *The Undecidable.* Raven Press, New York. Reprinted from *The Journal of Symbolic Logic.* Vol.1. pp.87-91.

[Ro53] ... 1953. *Logic for Mathematicians.* McGraw Hill, New York.

[Rob83] Guy Robin. 1983. *Sur l'ordre maximum de la fonction somme des diviseurs.* In *Séminaire Delange-Pisot-Poitou, Théorie des nombres (1981-1982).* Progress in Mathematics. 38: 233-244.

[Rot88] Brian Rotman. 1988. *Towards a Semiotics of Mathematics.* In *Semiotica* 72 (1-2):1-36, published on-line in 1988. Also published in *18 Unconventional Essays on the Nature of Mathematics*, pp.182-200. Editors: Hersh Reuben (Ed.). 2006. Springer-Verlag New York.
https://doi.org/10.1515/semi.1988.72.1-2.1

[Rot99] ... 1999. *Technology of Mathematical Persuasion.* Web essay archived at Stanford University. Archive last modified 11/01/1999.
https://brianrotman.wordpress.com/articles/technology-of-mathematical-persuasion/

[RS17] Michael Rathjen and Wilfried Sieg. 2017. *Proof Theory.* Article dated November 14, 2017 uploaded by Wilfried Sieg to Research Gate, November 22, 2017.
https://www.researchgate.net/publication/321226347_Proof_Theory

[Ru53] Walter Rudin. 1953. *Principles of Mathematical Analysis.* McGraw Hill, New York.

[Rus19] Bertrand Russell. 1919. *Introduction to Mathematical Philosophy.* George Allen and Unwin Ltd., London.

[Rus37] ... 1937. *The Principles of Mathematics.* Second Edition, 1937. George Allen & Unwin Ltd., London.

[Sc35] Erwin Schrödinger. *Die gegenwärtige Situation in der Quantenmechanik, Naturwissenschaftern.* 23: pp. 807-812; 823-823, 844-849. (1935). English translation: John D. Trimmer, *The present situation in Quantum Mechanics*, Proceedings of the American Philosophical Society 124, pp. 323-38 (1980), reprinted in Quantum Theory and Measurement, p. 152 (1983).
http://www.tuhh.de/rzt/rzt/it/QM/cat.html#sect5

[Se49] Atle Selberg. 1949. *An Elementary Proof of Dirichlet's Theorem About Primes in an Arithmetic Progression.* Annals of Mathematics, Second Series, Vol. 50, No. 2 (Apr., 1949), pp. 297-304.
http://www.jstor.org/stable/1969454

[Sh67] Joseph R. Shoenfield. 1967. *Mathematical Logic.* Reprinted 2001. A. K. Peters Ltd., Massachusetts.

[She91] Saharon Shelah. 1991. *The Future of Set Theory*. Proceedings of the *Bar Ilan Winter School* (January 1991), Israel Mathematical Conference, vol 6. Distributed by the AMS.
http://shelah.logic.at/future.html

[Sh+11] Sheldon Goldstein et al. 2011. *Bell's Theorem*, Scholarpedia, 6(10):8378.
doi:10.4249/scholarpedia.8378

[Shr13] Gila Sher. 2013. *The Foundational Problem of Logic.* In *The Bulletin of Symbolic Logic*, Vol. 19, No. 2 (JUNE 2013), pp. 145-198. DOI:10.2178/bsl.1902010.
http://www.jstor.org/stable/41955403

[Si97] Wilfried Sieg. 1997. *Step by recursive step: Church's analysis of effective calculability.* In *Bulletin of Symbolic Logic*, Volume 3, Number 2.

[Si12] ... 2012. *Hilbert's Programs and Beyond.* 2013. Oxford University Press, New York, NY.

[Sim88] Stephen G. Simpson. 1988. *Partial Realisation of Hilbert's Program.* In *Journal of Symbolic Logic.* Volume 53, Issue 2, June 1988, pp.349-363.
https://doi.org/10.1017/S0022481200028309

[Sim06] ... 2006. *Subsystems of Second Order Arithmetic.* Preprint, 2006, Second Edition. Published in 2009 for the Association of Symbolic Logic by Cambridge University Press, USA.
http://www.personal.psu.edu/t20/sosoa/chapter1.pdf

[Sk22] Thoralf Skolem. 1922. *Some remarks on axiomatized set theory.* Text of an address delivered in Helsinki before the Fifth Congress of Scandinavian Mathematicians, 4-7 August 1922. In Jean van Heijenoort. 1967. Ed. *From Frege to Gödel: A source book in Mathematical Logic, 1878 - 1931.* Harvard University Press, Cambridge, Massachusetts.

[Sk28] ... 1928. *On Mathematical Logic.* Text of a lecture delivered on 22nd October 1928 before the Norwegian Mathematical Association. In Jean van Heijenoort. 1967. Ed. *From Frege to Gödel: A source book in Mathematical Logic, 1878 - 1931.* Harvard University Press, Cambridge, Massachusetts.

[Sk34] ... 1934. *Über die Nicht-charakterisierbarkeit der Zahlenreihe mittels endlich oder abzählbar unendlich vieler Aussagen mit ausschliesslich Zahlenvariablen.* Fundamenta Mathematicae, 23, 150-161. English version: *Peano's axioms and models of arithmetic.* In *Mathematical interpretations of formal systems.* North Holland, Amsterdam, 1955, pp.1-14.

[Sl15] Hartley Slater. 2015. *Gödel's Theorems and the ε-Calculus.* Paper presented on 10^{th} June at the Epsilon 2015 workshop on '*Hilbert's Epsilon and Tau in Logic, Informatics and Linguistics*', 10^{th} June 2015 - 12^{th} June 2015, University of Montpellier, France.
https://www.academia.edu/14450007/G%C5%8Ddels_Theorems_and_the_Epsilon_Calculus

[SL09] Michael Schmidt and Hod Lipson. 2009. *Distilling Free-Form Natural Laws from Experimental Data.* In *Science.* 03 April 2009: Vol. 324, Issue 5923, pp.81-85.
DOI: 10.1126/science.1165893

[Sm92] Raymond M. Smullyan. 1992. *Gödel's Incompleteness Theorems.* Oxford University Press, Inc., New York.

[SN01] Matthias Schirn and Karl-Georg Niebergall. 2001. *Extensions of the Finitist Point of View.* In *History and Philosophy of Logic*, Volume 22, 2001 - Issue 3, pp.135-161. Taylor and Francis. DOI: 10.1080/01445340210154295.
https://doi.org/10.1080/01445340210154295

[Sp10] Andrew Sparkes et al. 2010. *Towards Robot Scientists for autonomous scientific discovery.* In *Automated Experimentation.* 2010 2:1 (*Publication discontinued*).
https://doi.org/10.1186/1759-4499-2-1

[SP10] Andrea Scarantino and Gualtiero Piccinini. 2010. *Information Without Truth.* In *Metaphilosophy*, Vol.41, No. 3, April 2010. Included in *Putting Information First: Luciano Floridi and the Philosophy of Information.* Editor(s): Patrick Allo. Published by Blackwell Publishing Ltd, 9600 Garsington Road, Oxford OX4 2DQ, UK, and 350 Main Street, Malden, MA 02148, USA.
https://doi/book/10.1002/9781444396836

[Srn16] Jens Jakob W. H. Sørensen et al. 2016. *Exploring the quantum speed limit with computer games.* In *Nature*, 14 April 2016, volume 532, pp.210-213.
https://www.nature.com/articles/nature17620

[SRP17] S. A. Selesnick, J. P. Rawling, and Gualtiero Piccinini. 2017. *Quantum-like behavior without quantum physics I: Kinematics of neural-like systems.* In *Journal of Biological Physics,* July 2017, 43: 415, DOI: 10.1007/s10867-017-9460-9.

[St97] Ian Stewart. 1997. *Does God Play Dice?* (2nd ed.), Penguin, London.

[Ste02] Jörn Steuding. 2002. *Probabilistic Number Theory.* The Pennsylvania State University CiteSeerX Archives, doi=10.1.1.118.4755.
http://citeseerx.ist.psu.edu/viewdoc/summary?doi=10.1.1.118.4755

[Su60] Patrick Suppes. 1960. *Axiomatic Set Theory.* Van Norstrand, Princeton.

[SW17] Wilfried Sieg and Patrick Walsh. 2017. Natural Formalization: Deriving the Cantor-Bernstein Theorem in ZF. Article uploaded by Wilfried Sieg on ResearchGate on 30/11/2017.

[Ta35] Alfred Tarski. 1935. *Der Wahrheitsbegriff in den formalisierten Sprachen*. Studia Philos., Vol. 1, pp.261-405. Expanded English translation, *The concept of truth in formalized languages*. In *Logic, Semantics, Metamathematics*, papers from 1923 to 1938 (p152-278). ed. John Corcoran. 1983. Hackett Publishing Company, Indianapolis.
http://www.thatmarcusfamily.org/philosophy/Course_Websites/Readings/Tarski%20-%20The%20Concept%20of%20Truth%20in%20Formalized%20Languages.pdf

[Ta39] ... 1939. *Some Current Problems in Metamathmatics.* Edited version of a lecture given at Harvard University during 1939/40. Editors: Jan Tarski and Jan Wolenski. In *History and Philosophy of Logic*, Volume 16, 1995 - Issue 2, pp.159-168. Taylor and Francis.
https://doi.org/10.1080/01445349508837247

[Teg14] Max Tegmark. 2014. *Life Is a Braid in Spacetime: How to see yourself in a world where only math is real.* In *Nautilus: Matter/Physics*, October 1, 2015.

[Ti51] E. C. Titchmarsh. 1951. *The Theory of the Riemann Zeta-Function.* Clarendon Press, Oxford.

[Tu36] Alan Turing. 1936. *On computable numbers, with an application to the Entscheidungsproblem.* In M. Davis (ed.). 1965. *The Undecidable.* Raven Press, New York. Reprinted from the *Proceedings of the London Mathematical Society*, ser. 2. vol. 42 (1936-7), pp.230-265; corrections, Ibid, vol 43 (1937) pp. 544-546.

[Tu39] ... 1939. *Systems of logic based on ordinals.* In M. Davis (ed.). 1965. *The Undecidable.* Raven Press, New York. Reprinted from the Proceedings of the London Mathematical Society, ser. 2. vol. 45 (1939), pp.161-228.

[Tu50] Alan Turing. 1950. *Computing Machinery and Intelligence.* In *Mind*, Volume LIX, Issue 236, 1 October 1950, Pages 433-460.
https://doi.org/10.1093/mind/LIX.236.433

[Vi11] M. Villata. 2011. *CPT symmetry and antimatter gravity in general relativity.* In *EPL (Europhysics Letters)*, Volume 94, Number 2, March 2011.
https://doi.org/10.1209/0295-5075/94/20001

[Vo10] Vladimir Voevodsky. 2010. *What If Current Foundations of Mathematics Are Inconsistent?* Lecture delivered to the Schools of Mathematics and Natural Sciences, Institute for Advanced Study, Princeton during celebration of its eightieth anniversary.
https://www.ias.edu/ideas/2012/voevodsky-foundations-of-mathematics

[Wa63] Hao Wang. 1963. *A survey of Mathematical Logic.* North Holland Publishing Company, Amsterdam.

[We10] Hermann Weyl. 1910. *Definitions of Fundamental Mathematical Concepts.* English translation by Stephen Pollard of Weyl's habilitation lecture *Über die Denitionender mathematischen Grundbegrie.* In *Mathematisch-naturwissenschaftliche Blätter*, 7 (1910):93-95, 109-113.
https://www.academia.edu/36559178/Definitions_of_Fundamental_Mathematical_Concepts

[We27] ... 1927. *Comments on Hilbert's second lecture on the foundations of mathematics.* In Jean van Heijenoort. 1967. Ed. *From Frege to Gödel: A source book in Mathematical Logic, 1878 - 1931.* Harvard University Press, Cambridge, Massachusetts.

[WG03] Peter Wegner and Dina Goldin. 2003. *Computation Beyond Turing Machines.* Communications of the ACM, 46 (4) 2003.

[WG06] ... 2006. *Principles of problem solving.* In *Communications of the ACM*, July 2006, Vol. 49 No. 7, Pages 27-29, doi:10.1145/1139922.1139942.
https://cacm.acm.org/magazines/2006/7/5879-principles-of-problem-solving/abstract

[Wi78] Ludwig Wittgenstein. 1937. *Remarks on the Foundations of Mathematics.* 1978 ed., MIT Press, Cambridge.

[Wl03] Frank Waaldijk. 2003. On the Foundations of Constructive Mathematics Especially in Relation to the Theory of Continuous Functions. In *Foundations of Science*, Volume 10, Issue 3, September 2005, pp. 249-324, Springer Verlag. DOI: 10.1007/s10699-004-3065-z.
https://link.springer.com/article/10.1007/s10699-004-3065-z

[Wol15] Natalie Wolchover. 2015. *A Fight for the Soul of Science.* Blogpost in ***Quanta****magazine*, December 16, 2015.
https://www.quantamagazine.org/physicists-and-philosophers-debate-the-boundaries-of-science-20151216/

[WR10] Alfred North Whitehead and Bertrand Russell. 2010. *Principia Mathematica to* $\star$56. 1962. Paperback edition. Cambridge University Press, London, U.K.

[Ya93] Stephen Yablo. 1993. *Paradox without self-reference*. Analysis, 53(4), pp. 251-252.
http://www.mit.edu/ yablo/pwsr.pdf

[An04] Bhupinder Singh Anand. 2004. *Do Gödel's incompleteness theorems set absolute limits on the ability of the brain to express and communicate mental concepts verifiably?* Invited article in the Special Issue on *Minds, Brains and Mathematics*, NeuroQuantology, [S.l.], v.2, n.2, 2004, pp.60-100. ISSN 1303-5150. DOI: 10.14704/nq.2004.2.2.39.
http://www.neuroquantology.com/index.php/journal/article/view/39/39

[An06] ... 2006. *How Definitive is the Classical Church-Turing Thesis?* In *Communications of the ACM*, October 2006, Vol. 49 No. 10, Pages 11-13. doi: 10.1145/1164394.1164406
http://cc.bingj.com/cache.aspx?q=how+definitive+is+the+classical+church-turing+thesis+article+oct+2006communications+of+the+acme&d=5008294438903125&mkt=en-US&setlang=en-US&w=0ruv5OMK-arlgkk1t8W__bXZzWy3KzFU

[An07a] ... 2007. *The Mechanist's Challege*. The Reasoner, Vol(1)5 p5-6.
https://www.kent.ac.uk/secl/researchcentres/reasoning/TheReasoner/vol1/TheReasoner-1(5).pdf

[An07b] ... 2007. *Why we shouldn't fault Lucas and Penrose for continuing to believe in the Gödelian argument against computationalism - I*. The Reasoner, Vol(1)6 p3-4.
https://www.kent.ac.uk/secl/researchcentres/reasoning/TheReasoner/vol1/TheReasoner-1(6).pdf

[An07c] ... 2007. *Why we shouldn't fault Lucas and Penrose for continuing to believe in the Gödelian argument against computationalism - II*. The Reasoner, Vol(1)7 p2-3.
https://www.kent.ac.uk/secl/researchcentres/reasoning/TheReasoner/vol1/TheReasoner-1(7).pdf

[An12] ... 2012. *Evidence-Based Interpretations of PA*. In Proceedings of the Symposium on Computational Philosophy at the AISB/IACAP World Congress 2012-Alan Turing 2012, 2-6 July 2012, University of Birmingham, Birmingham, UK.
http://events.cs.bham.ac.uk/turing12/proceedings/04.pdf

[An13] ... 2013. *A suggested mathematical perspective for the EPR argument.* Presented on 7^{th} April at the workshop on '*Logical Quantum Structures*' at UNILOG'2013, 4^{th} World Congress and School on Universal Logic, 29^{th} March 2013 - 7^{th} April 2013, Rio de Janeiro, Brazil.
https://foundationalperspectives.files.wordpress.com/2016/07/42_resolving_epr_unilog_2013_extended_abstract.pdf

[An14a] ... 2014. *A probability-based proof that any deterministic algorithm which always computes a prime factor of n cannot be polynomial-time.* Submitted on 01/01/2015 to the *Calcutta Statistical Association Bulletin*, Kolkata, India.
http://alixcomsi.com/40_IF_not_in_P_Update.pdf

[An14b] ... 2014. *Defining prime probability analytically: A probability-based approach to estimating the prime and twin-prime counting functions* $\pi(n)$ *and* $\pi_2(n)$ *analytically.* Submitted on 12/12/2014 to the *Calcutta Statistical Association Bulletin*, Kolkata,

India.
http://alixcomsi.com/40_PNT_Update.pdf

[An15] ... 2015. *Why Hilbert's and Brouwer's interpretations of quantification are complementary and not contradictory*. Presented on 10^{th} June at the Epsilon 2015 workshop on '*Hilbert's Epsilon and Tau in Logic, Informatics and Linguistics*', 10^{th} June 2015 - 12^{th} June 2015, University of Montpellier, France.
https://sites.google.com/site/epsilon2015workshop/

[An15a] ... 2015. *Algorithmically Verifiable Logic vis à vis Algorithmically Computable Logic: Could resolving EPR need two complementary Logics?* Presented on 26'th June at the workshop on '*Emergent Computational Logics*' at UNILOG'2015, 5^{th} World Congress and School on Universal Logic, 20^{th} June 2015 - 30^{th} June 2015, Istanbul, Turkey.
https://foundationalperspectives.files.wordpress.com/2016/05/42_resolving_epr_unilog_2015_full_paper.pdf

[An16] ... 2016. *The truth assignments that differentiate human reasoning from mechanistic reasoning: The evidence-based argument for Lucas' Gödelian thesis*. In *Cognitive Systems Research*. Volume 40, December 2016, Pages 35-45.
doi:10.1016/j.cogsys. 2016.02.004.

[An18] ... 2018. *Are the Barriers that Inhibit Mathematical Models of a Cyclic Universe, which Admits Broken Symmetries, Dark Energy, and an Expanding Multiverse, Illusory?* Preprint.
https://www.academia.edu/s/5605f44056/are-the-barriers-that-inhibit-mathematical-models-of-a-cyclic-universe-which-admits-broken-symmetries-dark-energy-and-an-expanding-multiverse-illusory?source=news