RETHINKING INCONSISTENT MATHEMATICS

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This thesis is not dedicated to anyone, because I have BPD and the stuff above was hard enough to write. Instead, here is my alphabetically ordered top 7 favorite games I played during this PhD: Baba Is You, Before Your Eyes, Elsinore, Outer Wilds, Paradise Killer, Sayonara Wild Hearts, Super Metroid.

Summary

Here is what happens in this dissertation, if you don't care about spoilers.

Chapter 0 begins with a short historical introduction to the field of inconsistent mathematics, individuating six main traditions. Next, the goals and methodology of the thesis are introduced. The focus on mathematical practice is explained, and four main questions are stated:

1. What motivates inconsistent mathematics?
2. What is the role of logic in inconsistent mathematics?
3. What characterizes inconsistent mathematics as distinct from classical mathematics?
4. How does inconsistent mathematics relate to classical mathematics (e.g. is it an extension, an alternative, a revolution, etc.)?

Chapter 1 is concerned with classifying and comparing different argumentative strategies for justifying particular inconsistent practices. Sections 1.1 discusses purely mathematical reasons to care about inconsistent mathematics, noting in particular the uncontroversial value of it from a mathematical logic perspective. Section 1.2 argues that appeals to duality cannot be sufficient on their own. Section 1.3 notes some possible difficulties with appeals to an inconsistent subject matter. Sections 1.4 and 1.5 critically discuss arguments for inconsistent mathematics which justify it as a support to particular philosophical doctrines, e.g. foundationalism, logicism, formalism, or strict finitism. Section 1.6 surveys several attempts in the literature to read inconsistent mathematics directly from classical practice, and finds them unconvincing. Section 1.7 argues that claims to the effect that classical logic is invalid require some appeal to foundationalism in order to affect classical mathematics, and therefore justify inconsistent mathematics. Section 1.8 presents the so-called argument from liberation for inconsistent mathematics: like classical logic, classical mathematics

contributes to the naturalization of harmful dualisms, and inconsistent mathematics can be defended as a way to counteract this.

Chapter 2 focuses on the relationship between logic and inconsistent mathematics. Sections 2.1-2.5 discuss the pros and cons of several classes of nonclassical logics for the purposes of inconsistent mathematics. Section 2.6 discusses a potential example of inconsistent mathematics which does not make use of nonclassical logics. Section 2.7 argues that a pluralist attitude is required from a minimally naturalist perspective. Section 2.8 goes one step further, and suggests that the arguments from pure maths and from liberation are best served by a nihilist attitude. Section 2.9 balances this by introducing Juliette Kennedy's notion of formalism freeness and indicating how it may be used to make sense of the multitude of possible approaches.

Chapter 3 is a survey of the technical literature on inconsistent mathematics, with a focus on comparing assumptions, uses of logic, and attitudes towards inconsistencies. Section 3.1 is dedicated to naive set theories: after presenting the central issues and some assessment criteria, many approaches are discussed and compared. Section 3.2 moves on to the kinds of inconsistent mathematics generated by postulating inconsistent entities or predicates on top of classical structures. Section 3.3 surveys the literature on inconsistent models of classical theories. Sections 3.4 defends the inconsistency of relevant arithmetic and compares several proposals on how to understand implications with false antecedents. Section 3.5 discusses the difficult interaction between inconsistencies and group structures. Section 3.6 compares two methodologically quite diverse applications of inconsistent geometry, namely the classification of impossible pictures and the metaphysical analysis of boundaries.

Chapter 4 is concerned with the characterization of inconsistent mathematics as distinct from classical mathematics. Section 4.1 sets the scene by emphasizing the question of what makes an informal theory substantially inconsistent. Sections 4.2 and 4.3 argue that appeals to paraconsistent logics, inconsistent formalizations, or inconsistent foundations are all inadequate for the purpose of characterization, as they overshoot the target due to the possibility of inert or accidental contradictions. Section 4.4 argues that a characterization based on inconsistent concepts can only do its intended job insofar as the attribution of inconsistency is agent-dependent, which however makes the resulting characterization potentially fragile and unsubstantial. Section 4.5 solves this issue by proposing a characterization of inconsistent mathematics as an agent-dependent activity of inconsistentization. Joining this with the argument from liberation, queer incomaths is born. Section 4.6 introduces the idea of a critical maths kind in analogy with Robin Dembroff's notion of critical gender kinds.

Chapter 5 presents a toy example of how queer incomaths could consciously drive mathematical practice, proposing several ways to inconsistentize the Cantor space without starting from a choice of logic or target theory. Section 5.1 introduces several (classical) perspectives on the Cantor space; a short introduction to the topological notions at play is found in the Appendix. Section 5.2 explores two different ways to inconsistentize the identity relation on the space, while Sections 5.3 and 5.4 show how the tree-like construction of the Cantor space can be manipulated so as to naturally introduce inconsistencies in the end result. Section 5.5 uses these inconsistent versions of the Cantor space to showcase how non-mathematical concepts, in this case gender, may be liberated through classical modelling followed by inconsistentization.

Chapter 6 is an attempt to extract and put to work a practice-based classification of different conceptions of inconsistent mathematics. Section 6.1 relies on José Ferreirós's notion of Framework-Agent pair to give sufficient conditions for the inconsistency of a practice, and argues that inconsistent practices in this sense are not classical. Section 6.2 classifies inconsistent agents along three dimensions of their meta-mathematical views, namely what, how, and when should we inconsistentize. Section 6.3 discusses which conceptions of inconsistent mathematics can provide a genuine alternative to classical mathematics: dialetheic mathematics is argued to be as alternative as intuitionistic mathematics, while queer incomaths is argued to be really alternative (in Jean Paul Van Bendegem's sense) because of its rejection of stability. Section 6.4 argues that inconsistent mathematics is not on track for a Kuhn-style revolution, yet certain kinds of inconsistent agents can be understood as revolutionary. In particular, queer incomaths comes out as revolutionary because of its nihilist attitude towards logic choice, contra-classical standards of practice change, and introduction of an ethical dimension in mathematics.

Finally, Chapter 7 waxes poetic about a possible future for inconsistent mathematics. Section 7.1 ponders how queer incomaths may come to be implemented in practice, while Section 7.2 discusses what the consequences of adopting queer incomaths would be for the philosophy of mathematics at large. Some directions for future work are suggested. Section 7.3 is a hopeful goodbye.

List of published parts

An earlier version of Section 1.8 first appeared in "The liberation argument for inconsistent mathematics", published in the *Australasian Journal of Logic*. Some of the material developed in Sections 1.7, 4.5, 7.2 was also first discussed there.

Chapter 0

Introduction

Inconsistent mathematics is a tiny field of study that arose in the second half of the twentieth century. Very roughly, it is a branch of mathematics concerning itself with reasoning from or about contradictions;[1] for a variety of reasons, it has been proposed and developed by mathematicians, logicians, and philosophers alike. It distinguishes itself from *classical mathematics* insofar as the latter shuns all contradictions: they are never introduced on purpose, and when they are discovered they have to eventually be removed.[2] This dissertation is, among other things, a survey of the field; an attempt to make sense of the various conceptions floating around; and, maybe most importantly, a suggestion towards a new future.

Let me set the stage by providing a brief history of inconsistent mathematics and some simple examples of what has come out of it. While alleged historical examples abound, from the early calculus to Cantorian set theory, the origins of inconsistent mathematics as a field of study may be traced back to [Asenjo, 1954], where the first logic specifically for the purpose of studying mathematical antinomies was proposed: this was the *calculus of antinomies*, later popularized as **LP**.[3] The idea was that this logic would allow us to explore mathematical questions like "what can we say about a set that both contains and does not contain some element?", or "what can we say about a number that is strictly

[1] Attempts to be less rough will take a good chunk of the dissertation.

[2] I will use the expressions "classical mathematics" and "mainstream mathematics" interchangeably throughout most of this thesis. This terminology points at the fact that mainstream mathematical reasoning involves some typically classical inference rules (e.g. Reductio and Disjunctive Syllogism), and that the received foundation of mathematics - ZFC (Zermelo-Fraenkel + Choice) set theory - is based on classical logic. That being said, I am not assuming any *essential* connection between classical logic and current mainstream mathematics.

[3] See also [Asenjo, 1966]. The name **LP** - for Logic of Paradox - was introduced by [Priest, 1979], who independently rediscovered the logic and deployed it as a solution to logical paradoxes.

less than itself?"; taken at face value, these questions remain off-limits to classical mathematics, where such contradictory properties are banned by fiat. Investigations of this sort were carried out in [Asenjo and Tamburino, 1975], [Asenjo, 1989], and [Asenjo, 1996], constituting what Asenjo called *antinomic mathematics*.

We may identify *at least* five other main traditions of inconsistent mathematics, developing more or less independently from Asenjo's work: *paraconsistent mathematics*, starting with [da Costa, 1964]; *relevant arithmetic*, starting with [Meyer, 1976]; *dialectical* or *dialetheic mathematics*, starting with [Sylvan, 1977];[4] the study of *inconsistent models*, starting with [Meyer and Mortensen, 1984]; and the study of *impossible pictures*, starting with [Mortensen, 1997].

Paraconsistent mathematics differs from antinomic mathematics in focusing not on inconsistent theories, but rather on consistent theories which allow for inconsistent assumptions without thereby taking any of them to be actually true. While such theories can also be used to answer questions about inconsistent entities, they do not involve postulating any such entities, and so they can be more easily framed as extending the application range of classical theories. This line of research has produced paraconsistent extensions of category theory, calculus, and set theory: some examples are respectively [da Costa et al., 2004], [Carvalho, 2004], and [Carnielli and Coniglio, 2013]. Usually, the strategy is to take a classical theory, weaken its underlying logic so that contradictions do not cause too much trouble, and add enough axioms to recover any classical equivalence one might need.

Let us consider a little example of what paraconsistent mathematics has to offer. Both the universal set U - the set which contains all sets - and the Russell set R - the set containing all sets which do not contain themselves - are well known to lead to paradox. By Cantor's theorem, U is strictly smaller than its power set (i.e. the set of all its subsets); but, in virtue of being the universe, U is also a superset of its power set, hence it cannot be strictly smaller. Contradiction. Concerning R, problems arise as soon as we ask: is it the case that $R \in R$? If yes, then $R \notin R$ by definition of R; if not, then $R \in R$ by definition of R. Contradiction. These paradoxes are classically taken to entail that U and R do not and cannot exist; paraconsistent set theory needs not challenge this, but unlike classical mathematics it is able to answer with nuance the question of what would happen if they *did* exist. For example, [Arruda and Batens, 1982] showed that, under very weak logical and

[4]The term "dialetheic mathematics" has been used by Zach Weber in his introduction to [Sylvan, 2019], and I am going to stick with it when referring to this tradition, since the connection with dialectics has largely fallen out of the picture.

set-theoretical assumptions, if the Russell set exists then the universal set exists. This kind of result has little value from a classical perspective, because classical set theory claims that if the Russell set exists then *everything whatsoever* exists, so any mathematical subtlety goes out the window. Paraconsistent set theory thus provides a framework where questions about inconsistent sets can be given more informative answers.[5]

The name "paraconsistent mathematics" comes from paraconsistent logics. A logic is said to be *paraconsistent* if it rejects the classical Explosion rule: if A and not-A, then B. The intention behind such a rejection is to allow for theories that are inconsistent, i.e. entail A and not-A for some A, yet nontrivial, i.e. do not entail everything. In practice, this is hardly guaranteed by merely getting rid of Explosion, so the definition is best treated as a mere starting point.[6] **LP** is maybe the simplest example of a paraconsistent logic, as it can be straightforwardly obtained by opening up the usual semantics of classical logic to the non-exclusivity of truth and falsity.

Relevant logics are a particular breed of paraconsistent logics insisting that in every valid implication the consequent should be in some sense *relevant* to the antecedent. This is a clear break with the classical material conditional, which only takes truth values into account: for example, false propositions classically imply everything whatsoever, relevant or not. Relevant logics tend to be paraconsistent because the Explosion rule appears to be a clear example of irrelevant reasoning: intuitively, the fact that $R \in R$ and $R \notin R$ has nothing to do with whether I had coffee this morning. So it seems natural for a relevant logic to discard Explosion, and so we may expect relevant mathematics - i.e. mathematics built on some relevant logic - to allow for contradictions.[7]

Robert K. Meyer's relevant arithmetic $\mathbf{R}^\sharp$, developed in [Meyer, 2021a] and [Meyer, 2021b],[8] was an attempt to apply such logical insights to arithmetic. Should we not be able to ask what is *really* - i.e. relevantly - implied by $0 = 2$, even if we know that it is false and in fact $0 \neq 2$? $\mathbf{R}^\sharp$ delivered on this by recasting the axioms of Peano Arithmetic in a relevant logic, thus allowing for a more fine-grained distinction between different implications with false antecedents: for example, it can prove that $0 = 2 \rightarrow 0 = 4$, but not that $0 = 2 \rightarrow 0 = 1$. An additional hope was that $\mathbf{R}^\sharp$ would be a closer fit to mathematicians' actual

[5]For more on antinomic and paraconsistent mathematics, see Section 3.2.

[6]Some authors try to avoid this issue by defining paraconsistent logics as those logics which can underlie inconsistent nontrivial theories; however the resulting class is just as inhomogeneous, so I will stick with the former meaning for simplicity.

[7]For more on paraconsistent logics, see Sections 2.1-2.5.

[8]These are both unfinished manuscripts from the '70s which only recently saw (posthumous) publication.

reasoning practices, and come with a better metamathematics. Meyer's ambitions had to be redimensioned a bit when [Friedman and Meyer, 1992] showed that $\mathbf{R}^\sharp$ is in fact unable to prove some basic number-theoretic truths derivable in Peano Arithmetic. Nevertheless, variants and models of $\mathbf{R}^\sharp$ are still being discussed to this day: see e.g. [Logan and Leach-Krouse, 2021] and [Slaney, 2022].[9]

The interest in paraconsistent and relevant logics naturally spawned an interest in the kind of models that such logics allowed. This in turn led to the study of what are usually called *inconsistent models*, i.e. models which satisfy some contradiction.[10] Maybe the most famous examples of inconsistent models are so-called finite models of arithmetic. One such model was used in [Meyer, 1976] to show that $\mathbf{R}^\sharp$ is non-trivial; [Priest, 1997], [Paris and Pathmanathan, 2006], and [Tedder, 2015] discuss the matter more systematically. Here is one extremely simple example: take the standard model of arithmetic, and let every number greater than 2 be also equal to 2. This is a nontrivial **LP**-model[11] of arithmetic, in the sense that all truths of arithmetic hold in it (together with some of their negations) yet not every arithmetical sentence does: for example, it is not the case that $0 = 1$. Infinite inconsistent models have been studied as well, and they are the main topic of [Mortensen, 1995], which provides such models for many different areas of mathematics.[12]

Usually, neither paraconsistent nor relevant mathematics as presented here are committed to the truth of any contradictions: the point is to study what follows from contradictions, not to endorse them. Dialetheic mathematics is a different beast. *Dialetheism* is the doctrine that there are true contradictions, and dialetheic mathematics is meant to be the mathematics of dialetheism: a technical foundation which can not only countenance but *prove* true contradictory theorems.[13]

Maybe the most influential examples of dialetheic mathematics come from *naive set theory*, i.e. set theory as characterized by the following axioms:

- Extensionality: if two sets have the same elements then they are identical, formally $\forall x, y(\forall z(z \in x \leftrightarrow z \in y) \rightarrow x = y)$.

[9] For more on $\mathbf{R}^\sharp$, see Sections 1.5 and 3.4.

[10] A note on terminology. Models of paraconsistent logics are almost always built in a classical metalanguage, so there is a sense in which they are perfectly consistent. Because of this, there has been some opposition to the idea of calling them inconsistent models; it might be more accurate to call them *consistent* models of inconsistent theories. They are not themselves inconsistent; they are consistently representing inconsistency. With this caveat in mind, for simplicity I will stick to the standard terminology here. When inconsistent models the likes of [Badia et al., 2022] become more popular, a better terminology may have to be developed: for example, we may want to talk about classical inconsistent models vs nonclassical inconsistent models.

[11] Given a logic **L**, by **L**-model I mean a structure satisfying the axioms and rules of **L**.

[12] For more on inconsistent models, see Section 3.3.

[13] For a defense and exploration of dialetheism, see [Priest, 2006b] and [Priest, 2006a].

- (Naive) Comprehension schema: for every property ϕ there is a set of exactly those sets satisfying that property, formally $\exists y \forall x (x \in y \leftrightarrow \phi(x))$.

Famously, these axioms lead to paradox when we consider the property of not belonging to oneself, since Naive Comprehension will then generate the Russell set. Classically, this line of reasoning functions as a reductio of Naive Comprehension; but if we stick with the intuition that Naive Comprehension is true, as many dialetheists seem to think, then there seems to be no escaping the fact that the Russell set exists and truly belongs and does not belong to itself.[14]

Results on naive set theory abound, although they vary wildly depending on the adopted logic. If the logic is very weak, then it is relatively easy to find models, and therefore to show non-triviality. For example, [Weir, 2004] presents a 1-element **LP**-model: this is a set-theoretic universe with a single object satisfying *every* instance of Naive Comprehension, yet being consistently non-self-identical, so that the model is technically non-trivial. Things can get significantly more complicated when we move to stronger logics: still, [Brady, 1989] managed to prove that some deductively stronger naive set theories are in fact non-trivial (relative to ZFC),[15] while [Weber, 2010b] and [Weber, 2012] showed that such theories are strong enough to support basic ordinal and cardinal arithmetic. Going even further, [Weber, 2021a] makes some first steps towards a naive set-theoretic interpretation of a whole bunch of inconsistent mathematics, from arithmetic to topology; [Badia et al., 2022] is a first attempt to add metamathematics (i.e. model theory) to the list.[16]

What about applications? One striking instance of applied inconsistent mathematics concerns so-called *impossible pictures*: these are 2D projections of 3D geometric figures that *look* like they could not actually exist in space. In recent years, Chris Mortensen has spearheaded a project of classifying impossible pictures through the use of inconsistent mathematics, which culminated into [Mortensen, 2010]: the main idea is to take seriously the perceived inconsistency and explicitly incorporate it within the mathematical description of the pictures, for example by allowing for points x, y such that x is (perceived as being) both in front of and not in front of y.[17]

As far as I know, the term "inconsistent mathematics" was popularized as a

[14] There is, in fact, escaping. Usually it involves rejecting the Law of Excluded Middle, thus disallowing proof by cases: see for example [Brady, 2006].

[15] For a friendly recap of the proof strategy, see [Weber, 2022, ch.2].

[16] As far as I know, the naive set theories adopted in these more recent works have not yet been proven to be non-trivial, although they do appear to avoid all the usual traps. For more on naive set theories, see Section 3.1.

[17] For the history of impossible pictures in the 20th century, see [Mortensen, 2022]. For more on inconsistent treatments of them, see Section 3.6.

catch-all for the field mainly via its use as the title of [Mortensen, 1995], the first book on the topic. Of course, the terms "relevant", "dialetheic", and "dialectical" could hardly serve this purpose: much inconsistent mathematics has nothing to do with relevance or dialetheism or dialectics. [Mortensen, 2000] explicitly rejects the label "paraconsistent mathematics" on the grounds that paraconsistency *"is a property of logics rather than theories"* (p.203); more importantly, we will see that not every piece of inconsistent mathematics requires a paraconsistent logic.[18] So I will stick with "inconsistent mathematics" when I want to talk about the field in general.[19]

For all the work that has been done in inconsistent mathematics, it must be said that the field remains an exceedingly small niche, unknown not only to most philosophers but - especially - to most mathematicians. This is partly because of the very limited number of practitioners, most of which are coming from logic or philosophy; and partly because interactions with classical mathematical open problems have been almost nonexistent up to now, which makes it difficult to catch the classical mathematician's attention. In principle, this may change at any time; all it would take is one proof of, say, Riemann's Hypothesis achieved via some paraconsistent detour. Hence, nothing in this dissertation builds on the assumption that inconsistent mathematics will *never* be incorporated into the mainstream.

Goals and methodology

This is a dissertation *about* inconsistent mathematics; it is not a dissertation *in* inconsistent mathematics, insofar as it is (mostly) not a work of logic or mathematics. It is also not really about any *specific* kind of inconsistent mathematics: much has been said about particular philosophical and technical projects, in defense of this or that paraconsistent logic, but not much has been said about the field *in general*, and that is the main gap I am attempting to fill. The main questions I want to answer are the following:[20]

1. What motivates inconsistent mathematics?
2. What is the role of logic in inconsistent mathematics?
3. What characterizes inconsistent mathematics as distinct from classical mathematics?
4. How does inconsistent mathematics relate to classical mathematics (e.g. is it an extension, an alternative, a revolution, etc.)?

[18] See Sections 2.4 and 2.6.

[19] Which is not to say I subscribe to Mortensen's definition, which I will discuss in Section 4.1.

[20] They will be primarily discussed in Chapters 1, 2, 4, and 6 respectively.

All of these questions are approached from a perspective that may be taken as belonging to the philosophy of mathematical *practice*, in at least two senses. First, my starting point is the inconsistent practices emerging from the literature, and the way in which they interact with or differ from classical practices.[21] The philosophical assumptions of any given practice are taken into consideration as part of the practice, and they are in principle treated neutrally.[22] Second, I take it for granted that questions about inconsistent mathematics cannot be answered in a void, independently from what mainstream mathematics is and classical mathematicians do, especially insofar as classical mathematics appears to be (in some sense) successful, knowledge-producing, and relatively stable. This does not entail any sort of hardcore naturalism to the effect that classical mathematics cannot be questioned from the outside; rather, the point is that classical mathematics cannot be simply *dismissed*.

It will soon become clear that the literature can provide no unique answer to these questions, insofar as there is no such thing as a uniform conception of inconsistent mathematics encompassing the whole field, but rather many different and often incompatible conceptions, from both a philosophical and technical point of view; in fact, the very extension of inconsistent mathematics is itself controversial. Hence, a big part of the thesis will be concerned with classifying and discussing *possible* answers.

That being said, I will also present and defend my own answers. More specifically, I will argue:

1. that inconsistent mathematics is justified as a means of counteracting the social harms involved with the naturalization and normalization of classical mathematics;

2. that inconsistent mathematics is best served by a logical nihilism balanced by an active search for formalism freeness;

3. that inconsistent mathematics is best thought of as an activity of reinterpreting previous mathematics in inconsistent ways;

4. that inconsistent mathematics can be seen as a genuine alternative to classical mathematics, and as carrying revolutionary intent.

[21] Throughout the dissertation, by inconsistent practices I mean practices that are part of inconsistent mathematics, and by inconsistent mathematicians I mean mathematicians engaged in inconsistent practices. More precise definitions will be given in Ch.6.

[22] Which is not to say that my sympathies will be inscrutable and fail to color my arguments.

These four answers are argued for independently, yet come together in a cohesive conception of inconsistent mathematics which I call *queer incomaths*.[23]

My defense of queer incomaths is not meant to be at the expense of other conceptions of inconsistent mathematics. I claim, of course, that it is a most valuable conception and worth taking seriously; furthermore, there is a sense in which the whole of inconsistent mathematics as it exists today contributes to, and in fact may be seen as part of, queer incomaths. Still, other more restrictive conceptions could be valuable for a variety of purposes. In fact, this dissertation also aims at providing a toolkit for comparing and assessing existing practices, not to mention creating new ones; hence the various classifications and assessment criteria sprinkled throughout.

A final note. When I started working on this, there was no such thing as a comprehensive survey of inconsistent mathematics, so to provide one was both a prerequisite for my project and a valuable goal in itself. This explains why I may occasionally indulge in the discussion of some work that never ends up being called upon in my main arguments. Of course, [Weber, 2022] eventually scooped me on that goal; still, my dissertation is overall more comprehensive and the way I present the material significantly different, so there is very little overlap.

[23] The term "queer incomaths" first appeared in [Mangraviti, 2023], which focuses on the liberation aspect. The connection with queer theory will be explained in Section 4.5; "incomaths" is simply an abbreviation for "inconsistent mathematics" - it's like slang, from England, if you will.

Chapter 1

Why inconsistent mathematics?

To begin with, I want to look at the motivations which have been presented for studying, developing, or acknowledging inconsistent mathematics anew. My focus will be less on the details of any particular proposal, and more on the viability, requirements, and implicit assumptions of the general strategies. The goal is (for the most part) to clarify, not to debunk; I want to highlight what each line of argument for inconsistent mathematics relies on, and what it needs to show in order to be truly convincing. My novel argument will be presented last.

Different arguments can point to different kinds of inconsistent mathematics, which is to say to different mathematical *practices*. Following [Kitcher, 1984, ch.7], we can roughly boil a practice down to a shared language among the practitioners, a set of accepted statements and reasoning techniques, a set of open questions the practitioners are concerned with, and a set of shared metamathematical views providing an interpretation and justification of the formalism - including what the goals and standards of the practice are.[1] Practices are not fixed, but rather change over time (e.g. new concepts may be introduced, or new statements may come to be accepted upon being proven) in accord with the relevant shared views of their practitioners. A practice-based perspective will be crucial in assessing the arguments on their own terms: the success of a given practice should not be evaluated based on the goals of a different one.

I will take a fully fledged argument for an inconsistent mathematical practice X to have four distinct aspects:

1. X can achieve its goals (*Possibility*);

2. X is worth exploring (*Importance*);

[1] I will adopt a more complex definition of practice in Chapter 6, but for now this will do.

3. X is not replaceable by consistent mathematics without significant loss (*Indispensability*);

4. X involves inconsistency and is recognizable as mathematics (*Pertinence*).

I take *Importance* to be implicitly assuming *Possibility* for the sake of argument; the reason I treat *Possibility* as a separate aspect is because the vast majority of the literature[2] is concerned with suggesting projects rather than carrying them out. In such cases, assessment of *Possibility* remains largely speculative; my job will simply be to point out potential obstacles.

Often, *Importance* will be connected to the specific metamathematical commitments of the practice: for example, if X is an attempt to provide a foundation for mathematics, *Importance* will partly depend on the assumption that foundationalism is a philosophy of mathematics worth pursuing. This chapter is not the place to debate any such assumptions, and I will remain largely neutral about them. I will, however, point out where the classical standards (i.e. the metamathematical standards occurring in classical practices) do not appear to establish *Importance* on their own, insofar as this indicates the need for an independent justification of alternative standards.[3]

Indispensability is included because consistency is arguably *the* classical standard, so the argument loses a lot of its persuasive power if the appeal to inconsistent mathematics is easily disposed of. In fact, even dialetheists seem to be generally willing to concede that - other things being equal - a consistent theory is rationally preferable to an inconsistent one.[4] Of course it would be question-begging to take inconsistency to be automatically disqualifying; following [Priest, 2006a, ch.7], a fairer approach is to assume that consistency has roughly the same status as any other theoretical virtue, e.g. simplicity or fruitfulness, and that we are free to dispense with it - in fact, it is *rational* to do so - if an inconsistent theory overall fares better than its consistent alternatives.[5]

It is important to not read the *Indispensability* requirement too strictly.

[2]Including this thesis.

[3]This is the case regardless of whether classical standards are taken to be well justified or not. The point is simply that if classical standards are relied on then their justification can be delegated to mainstream philosophy of mathematics.

[4]This concession to the orthodoxy has a minimal impact in practice, since "other things being equal" is a very idealized scenario and different theoretical virtues are often interconnected. Even radical dialetheists who reject consistent solutions across the board do not argue that inconsistent solutions are better *merely* in virtue of being inconsistent; rather, the inconsistency is taken to be conducive to the satisfaction of other virtues.

[5]This is echoed, from the perspective of contemporary mathematical practice, by [Wagner, 2017]: *"Consistency turns out to be one constraint among many, not an absolute foundation"* (p.101). See Section 1.6.

First of all, the inconsistent mathematician should not be required to *prove* the impossibility of consistent translation - that is too high a bar.[6] Promising consistent alternatives have to be actually exhibited in order to serve as a foil. Second, what counts as significant loss can and will be up for debate; in general, the best strategy is to argue that there is a specification of "significant loss" which both makes X significantly more adequate than any known classical practice and goes hand in hand with the justification of *Importance*. Note however that *Indispensability* does not in general entail *Importance*: just because classical mathematics cannot do something well, it does not follow that someone should be doing it.

While failure to comply with *Pertinence* does not of course invalidate a practice, it does prevent it from being considered in a thesis on inconsistent mathematics. However, since there is no universally accepted definition of inconsistent mathematics, the meaning of *Pertinence* will have to be extrapolated by the practice itself. One could decide to rule out as "not mathematical enough" purely formal work, or exclude anything that we could not expect a nontrivial fragment of the mathematical community to ever engage with as mathematicians. Similarly, for a theory to involve inconsistency could be as simple as containing something that looks like a contradiction, or it could be as tough as being impossible to convincingly rewrite in consistent terms. Since one of my goals is to figure out what inconsistent mathematics *could* be, and I do not wish to rule out any of the literature by fiat, I am going to leave the specification open for the time being.[7] My own proposal will be given in Chapter 4.

Note that I did not explicitly require that X be justified as knowledge-producing. In principle, this seems to be neither necessary nor sufficient for *Importance*: under most conceptions of knowledge, not every knowledge is particularly worth exploring and not everything worth exploring is so due to associated knowledge. *Pertinence* may demand that what comes out of X is "mathematical knowledge", whatever that might mean; but again, I want to leave that open for now.

1.1 The argument from pure mathematics

There is a very simple line of argument for inconsistent mathematics, one that gets thrown around a lot during coffee breaks and introductory slides (mine too,

[6] Especially since classical mathematicians cannot prove the consistency of their own mathematics!

[7] Of course, my selection of pertinent literature had to involve some degree of specification. I tried to be as comprehensive as possible, and take into account everything I know of that could be reasonably construed as pertinent.

I confess). Inconsistent mathematics is different from classical mathematics; it allows us to access a whole new realm of mathematical knowledge; and that is as good a reason as any to entertain it. As [Mortensen, 1995] puts it: *"The argument from pure mathematics for studying inconsistency is the best of reasons: because it is there"* (p.8).

As in most allegedly simple arguments, there is a lot to unpack here. The argument appears to fundamentally rest on the assumption that the space of inconsistent theories is *intrinsically valuable*, i.e. we should study it because we can. This is both a trivialization of *Possibility* (since non-trivial inconsistent theories exist, we are done) and an optimistic handwaving away of *Importance* (of course it's going to be worth it!).[8] And yet it seems obvious that, for inconsistent mathematics to have a chance at being justified on pure mathematical grounds, one must show that it is sufficiently interesting *as mathematics*.[9] Not every new theory or result is equally valuable; and when it comes to publishing, mathematicians are asked to justify the importance of their work as much as any other academic. It is not clear why inconsistent mathematics, if assessed as a branch of mathematics, should be treated any different.[10]

Some standard reasons for finding a piece of mathematics valuable are a potential for applications, or a contribution to the understanding of pre-existing mathematics. There is not much philosophical literature on what makes good mathematics, and either way, to give a theoretical discussion would be somewhat beside the point; the mathematical community does not need any help assessing it, and in the end the assessment of *Importance* in the argument from pure maths goes to them. Furthermore, as [Tao, 2007] correctly points out, attempting to fix a definition of good mathematics would run the risk of being exclusionary to genuinely valuable - if exotic - work, like one could imagine inconsistent mathematics to be.

That being said, it is worth examining how the interest question evolves when we are looking at inconsistent maths in particular. One necessary (though usually not sufficient) condition for some piece of mathematics to be interesting is that it be *mathematically non-trivial*, which we may gloss as having enough of an interesting

[8]From a purely formal perspective, one might argue that this sort of *Possibility* is not that trivial after all, since it required the invention of new logical machinery. Either way, the important point here is that it is uncontroversial today.

[9]To be clear, it is not my intention to imbue "sufficiently interesting" with some sort of mathematical elitism. Many niches of mathematics are uninteresting to most mathematicians outside them, due either to ignorance or taste; and depth varies wildly among branches without thereby disqualifying any. The point is simply that any new research direction requires *some* justification.

[10]Different branches may have different internal criteria, and in this sense inconsistent mathematics could introduce its own internal criteria for good mathematics. However, before doing this it must show itself to be a valuable new branch, so the need for justification does not disappear.

structure. This is to be distinguished from *logical* non-triviality, which involves not everything being true. For example, the classical 1-element group is not a logically trivial structure because it does not satisfy any sentence contradicting the group axioms; nevertheless, if all that inconsistent mathematics has to offer is along the lines of the 1-element group, then it is not going to rock anyone's socks off. Classically, at least in the context of formal theories, inconsistency automatically leads to logical triviality; on the other hand, inconsistent maths draws a distinction between the two, which has the effect of widening the gap between logical nontriviality and mathematical nontriviality. In other words, there is a lot more logical room for mathematically trivial constructions.

An explicit attempt to single out nontrivial mathematics is Mortensen's requirement of *functionality*: uniform substitution of identicals should preserve validity of atomic formulas.[11] The proposed motivation for this is that otherwise there can be no sensible notion of computation: what is the point of knowing that $2 + 2 = 4$, if we cannot use this fact anywhere else?[12] Functionality is contrasted with *transparency*, i.e. the idea that uniform substitution of identicals should preserve validity of *all* formulas. Classically, this is equivalent to functionality; but in paraconsistent contexts they might come apart. This suggests the triviality worry might be turned on its head: here is an example of inconsistent mathematics potentially allowing for more leeway in what constraints can be rejected while preserving enough structure.

Still, the assumption of functionality is hardly enough to exclude mathematically uninteresting structures. For example, [Priest, 2017] presents a functional model of naive set theory[13] which is constituted by a classical universe together with one additional inconsistent set b such that:

- every set both belongs and does not belong to b;
- b consistently does not belong to any classical set;
- no classical set is identical to b.

All structure here is on the consistent side; the one inconsistent set is as structureless as it could possibly be, since it relates to everything in the same way. So we may want to ask of a non-triviality criterion that it ensures that the inconsistent part of a construction is structured as well, not just the consistent part.

[11] See e.g. [Mortensen, 1995, ch.1].

[12] Actually, [Mortensen, 2000] notes that full functionality may already be too strict a requirement, since a single failure need not spread to every atomic equation (this is the kind of reasoning that supported paraconsistent logics to begin with, after all).

[13] **LP**-based naive set theory, to be more specific. I will say more about such models in Ch.3.

Of course, the lack of such a criterion is hardly an obstacle to finding particular examples of non-trivial inconsistent mathematics: we will see some better candidates in Ch.3. My point here is simply that "because we can" does not in itself fulfill *Importance*: it does not follow, just from the fact that inconsistent structures exist, that mathematicians are missing out by not exploring them, because for all we know they might be mathematically uninteresting (or only interesting insofar as they contain classical ones). The argument from pure mathematics requires the exhibition of interesting examples - or, at least, of reasons why we should expect to find them.

What about *Indispensability*? One way to achieve it would be to show that some inconsistent mathematics just *cannot* be consistently reformulated. There are good reasons to doubt any such bold claim, however. First of all, most research in inconsistent mathematics is presented through the lens of a classical - and therefore consistent - formal semantics: this means that we can provide a consistent reformulation simply by treating the formal semantics as part of the mathematics, rather than as a "real" semantics. In other words, any talk of inconsistency can be replaced by talk about consistent mappings of consistent mathematical objects ("sentences") to other consistent objects ("truth values" or the like),

Now, there is some work which does not explicitly rely on classical model theory, most notably [Weber, 2021a]; furthermore, there is still the matter of whether a purely formal translation could prevent mathematically significant loss. But the history of mathematics is largely a history of apparently contradictory statements and reasonings being eventually integrated into consistent theories, with no substantial loss to speak of - hence the standard picture of mathematics as essentially cumulative. Given such a track record, how could we ever reject the possibility of consistentization a priori?

Fortunately, *Indispensability* requires nothing this strong. It suffices that some inconsistent theories appear to be overall *better* - for some mathematical purposes - than any consistent counterparts. The mere existence of such counterparts does not in itself provide a counterargument, and this is a very general consideration: *"It would be inconsistent to argue that newer mathematics are likely useless insofar as they mirror older mathematics, while, at the same time, valuing the usual methods as useful precisely because they correspond to one another"* (p.163) [Martínez, 2018]. The advantages of inconsistent theories can be argued for through the standard criteria applying to mathematical theories. As long as the relevant goals are shared between defenders of the different theories (e.g. solving some classical problem), a favourable outcome would provide both *Importance* and *Indispensability* at once.

Now, Mortensen has in fact shown how inconsistent mathematics may be able to provide new solutions to classical problems. Two notable examples from

[Mortensen, 1995, chs.7-8] concern Dirac's δ function (which classically cannot be a function),[14] and inconsistent linear systems of equations generated by a mismatch of predicted and observed outputs. These "problems" already have consistent solutions, but Mortensen defends the inconsistent approach on the grounds of *simplicity*.

Simplicity, however, is a double-edged sword: historically, consistent theories which have been developed with the goal of eliminating inconsistencies have had far more reaching consequences than dealing with the one motivating example. For example, even if Mortensen's take on the δ function was indeed considered simpler than Schwartz's classical solution (which, among other things, would require thinking of paraconsistent logics as simple), the latter spanned an entire new branch of mathematical analysis with applications all over the place. The search for consistency has proven again and again to be mathematically fruitful, and it is not obvious that simple inconsistent solutions have the same potential. Now, it is perfectly possible that Mortensen's approach could turn out one day to be incredibly fruitful; my point is simply that it is going to be hard to establish its mathematical superiority until it does.[15] Note also that a simple solution is not even necessary as a temporary fix; after all, physicists *did* work with the δ function just fine during the decades it took for a rigorization to appear.[16]

Rather than going through a simplicity comparison, one might try to argue for fruitfulness more directly. For example, it has been suggested that adopting some particular paraconsistent logic would provide a better model theory, or more effective proof searches, or in general nicer metamathematical properties than classical logic.[17] These are ongoing (or abandoned) projects, but at least there is a clear path towards *Importance* and *Indispensability*.

Focusing solely on the advantages of the underlying logic could however lead to some issues with *Pertinence*, as one might want to draw a distinction between practising inconsistent mathematics and merely acknowledging that inconsistent interpretations are possible. The situation is familiar from classical

[14] Dirac's δ function is everywhere zero except in one point, yet it has integral 1. This characterization is classically incompatible with calling δ a function; the conundrum was eventually given a rigorous solution by Laurent Schwartz, who introduced a theory of *generalized functions* (or distributions) which can encompass δ and much more. See e.g. [Halperin and Schwartz, 1952].

[15] There could be other, less strictly mathematical reasons to push for simplicity. For example, the simplicity of nonstandard analysis has been praised on *pedagogical* grounds: see e.g. [O'Donovan, 2007]. I am not aware of any similar proposal concerning inconsistent mathematics, however.

[16] On this note, one may want to argue physicists were - at the time - doing inconsistent mathematics. This is a kind of argument from practice, which I will discuss in Section 1.6.

[17] See e.g. the comparison between Peano Arithmetic and relevant arithmetic in [Meyer, 2021b] and [Friedman and Meyer, 1992].

mathematical logic. Notoriously, first-order Peano Arithmetic (PA) has infinitely many nonstandard models and no internal way to distinguish them from the standard one.[18] This however does not mean that there are nonstandard natural numbers; or, to be more precise, this does not mean that *we* are confused about which ones are the "real" natural numbers. It would be bizarre to say we are doing nonstandard arithmetic *just* because we are working with a first-order formalization of PA. Similarly, consider Robinson's nonstandard analysis:[19] the fact that real numbers are first-order equivalent to the hyperreals needs not commit us to infinitesimals or to any confusion about which one is the real analysis;[20] we only talk about nonstandard analysis when we are explicitly working with hyperreals. By the same lights, it seems a bit too quick to say that a paraconsistent formalization of a piece of classical mathematics deserves the title of inconsistent mathematics *just* in virtue of having inconsistent models. Still, if we did happen to develop an independent interest in such inconsistent models, then we would have a branch of mathematics studying formalizations of inconsistent structures, which certainly sounds good enough for *Pertinence*.

Finally, there is a relatively uncontroversial, if possibly underwhelming, way to fill in *Importance* across the board. Often, inconsistent mathematics is borne out of the question: is this logic strong enough to support some kind of mathematics?[21] This is almost always a possible framing,[22] and the value of such questions is well established. But note that the mathematical interest behind the question is not really in the inconsistent mathematics itself; rather, it concerns the expressive power of the logic in question. In this sense, we have an intra-mathematical justification of inconsistent mathematics as a *test case* for investigations in mathematical logic. Relatedly, some logicians have argued that formal semantics for a logic, which usually require a modicum of mathematics, should be formulated using that very logic - as opposed to, say, classical logic - for maximum insight.[23] This is an intra-mathematical justification of inconsistent mathematics as a *tool* for investigations in mathematical logic.

On a similar note, inconsistent mathematics can also be motivated by logic in the sense of exemplifying a logic's value. This attitude is explicitly supported by [Asenjo, 1989]: *"the future of antinomic logic [...] will be decided by the mathematical usefulness of the structures that derive from the antinomic approach"* (p.412). So, if we have reasons to believe that some particular paraconsistent

[18] See [Kaye, 1991].
[19] See [Robinson, 2016].
[20] Pun intended.
[21] See e.g. [Badia and Tedder, 2018] and [Ferguson, 2019a].
[22] Except when classical reasoning is used: see Section 2.6.
[23] See e.g. [Girard and Weber, 2015].

logic is important, we can test this by looking at its ability to do inconsistent mathematics. If the resulting mathematics is fruitful, then the logic is validated; and if it is not, then on Asenjo's terms we can conclude that the logic is not a good one, which is still a worthwhile result.[24]

1.2 The argument from duality

The argument from *duality* is a variant of the argument from pure maths that attempts to dodge having to show *Importance* by uniformly inferring it from the similarity with other accepted theories whose value has already been established. The idea is maybe best summarized by [Meyer and Mortensen, 1987]: *"And there is no reason in the world why a model that results from fruitful confusion (because it corresponds to intuitive inconsistency) is less interesting or worthy of study than one which results from fruitful ignorance (corresponding similarly to incompleteness)"* (p.9).[25]

Now, this kind of argument obviously relies on accepting incomplete mathematics as a worthwhile endeavour, but this is not controversial: after all, ZFC itself is an incomplete theory, and it has to be on Gödelian grounds. Furthermore, intuitionistic mathematics, which takes incompleteness to the next level by rejecting the unrestricted Law of Excluded Middle, is officially recognized as a branch of mathematics, if an extremely niche one.[26]

There are a few ways to spell out the apparent duality between incompleteness and inconsistency. For a start, [Mortensen, 2013a] notes that every consistent incomplete theory T closed under double negation laws can be extended to a complete inconsistent dual "theory" $T^* = \{A : \neg A \notin T\}$.[27] The intuitive idea is that every *gap* of T - every A such that neither A nor $\neg A$ belong to T - is turned into a *glut* of T^*, i.e. both A and $\neg A$ show up in T^*; while T and T^* agree on their non-gappy non-glutty fragments. Such dual theories are always nontrivial,

[24]This has a philosophical upshot if we take this kind of evidence to suffice for logical pluralism, as e.g. [Shapiro, 2014] does. See Section 2.7.

[25]A theory is said to be *incomplete* if there is some A (in the same language) such that the theory proves neither A nor $\neg A$.

[26]The MSC2020 (Mathematics Subject Classification) contains the three following entries: *"intuitionistic mathematics"* (03F55), *"constructive and recursive analysis"* (03F60), and *"other constructive mathematics"* (03F65) [Mathematical Reviews and ZbMATH, 2020]. [Shapiro, 2014, ch.3] points at three pieces of mathematics based on intuitionistic logic which can be considered legitimate on fruitfulness grounds, regardless of one's philosophical leanings: Heyting arithmetic with the intuitionistic Church's thesis, intuitionistic analysis, and Kock-Lawvere smooth infinitesimal analysis. Finally, I will note that intuitionistic set theory made it into at least one general set theory textbook [Bell, 2011].

[27]Mortensen is following [Sylvan and Plumwood, 1972] here.

and in a sense they can be surprisingly well-behaved: for example, PA* is closed under single-premise deducibility, i.e. if $A \in$ PA* and B classically follows from A then $B \in$ PA*. On the other hand, Mortensen shows that PA* has some prima facie outlandish properties as well:

- it is not closed under adjunction, i.e. it is not the case that if $A, B \in$ PA* then $A \wedge B \in$ PA*;
- it is not closed under modus ponens, i.e. it is not the case that if $A, A \rightarrow B \in$ PA* then $B \in$ PA*.

More importantly, both closures would be *trivial*.[28]

Now, none of this needs to be a dealbreaker, but it does speak to some difficulties in making sense of such theories beyond the formalism. Even granting that this looks like reasonable fodder for inconsistent mathematics, I think Mortensen owes us *some* explanation of what the point of these dual theories is if we are to entertain them: how should we interpret them, what should we do with them, what do they tell us about mathematics, etc. The mere fact that they are dual of interesting theories does not do that much lifting.

Another influential way to think of the duality between incompleteness and inconsistency is in terms of categories. Topos theory took the classical orthodoxy by surprise via the discovery that the *internal logic* of a topos is intuitionistic.[29] Since topoi can cosplay as universes of sets, it can be tempting to conclude that in some sense the underlying logic of mathematics is intuitionistic. It has been argued in [Mortensen, 1995, ch.11], and more recently in [Estrada-González, 2015], that this picture is unjustifiably one-sided: one could run the same argument using the dual notion of *complement-topos*, and conclude that the underlying logic of mathematics is paraconsistent - specifically, a *closed set logic*.[30] The upshot is that paraconsistent logic appears to be at least as important for mathematics as intuitionistic logic. As we will see in Chapter 2, this is far from the only example of duality between glutty logics and gappy logics; the difference lies in the alleged mathematical significance of the logic of topoi, which is given by the idea that

[28] The same goes for every T^* such that T is consistent, incomplete, and closed under classical logical consequence.

[29] A *topos* is a category which has finite limits, is cartesian closed, and has a subobject classifier. I will not use any of these words again. For an introduction, see [Goldblatt, 2014].

[30] Closed set logic is also called, for obvious reasons, dual intuitionistic logic. The original name comes from the fact that, given an appropriate topological semantics, the logic is complete w.r.t. all families of *closed* sets across all topological spaces. Dually, intuitionistic logic is complete w.r.t. all families of *open* sets: see [Tarski, 1983].

topos theory may act as a foundation for mathematics.[31]

Now, the first problem with the argument from duality is that the importance of moving from an incomplete perspective to an inconsistent perspective should be motivated. If the two perspectives are in some sense symmetrical, what is the point of switching? Do we not already have access to everything we want by sticking to the far better understood viewpoint? [Mortensen, 2009a] seems to suggest that inconsistent maths is inherently more worthwhile because - unlike incomplete maths - it extends rather than restrict: *"the duals of incomplete theories are inconsistent, and they include classical consistent complete theories as subtheories, and consistent incomplete theories as sub-sub-theories. Thus inconsistent mathematics supports a principle of tolerance about what counts as mathematics"* (p.645). However, this is merely a point about theory acceptability *in principle*; one can agree (as I do) with Mortensen that the space of theories should not be logically restricted in the way some intuitionists and classicists want it to be, while at the same time remaining skeptical that there is something to be gained by looking at inconsistent theories on top of the ones we already have. An inclusive attitude is - by itself - compatible with inconsistent mathematics not being worth anyone's time.[32]

Even more worryingly, it is not at all obvious that duality transmits as much as required from the argument: as [Detlefsen, 2014] puts it, *"Being evident, interesting or practically important are not properties that dualization may generally be expected to preserve"* (p.18).[33] No matter what the interpretation of PA^* ends up being, it is hard to believe it could be as interesting or practically important (let alone evident) as *Peano Arithmetic*; and even if it was, this does not follow for free from the duality, and rather needs to be directly argued for.[34] More generally, one might try to argue that one particular way of cashing in the duality between incompleteness and inconsistency does reliably transmit these properties; but I struggle to imagine any way of doing this other than independently motivating, developing, and interpreting the dual mathematics in question. This amounts to mathematically assessing said mathematics, duality being at best a

[31] This phenomenon suggests some ways to use topos theory as an argument for inconsistent mathematics distinct from an argument from duality. First, one could try and leverage the fact that topos theory is a good foundation for mathematics, and that paraconsistency is part and parcel of its logic: this is an argument from foundations. Alternatively, one might argue that complement-topoi show that paraconsistency is already there, buried in classical mathematics: this is an argument from practice. Both kinds of arguments will be discussed in later sections.

[32] I will go back to the connection between inconsistent mathematics and tolerance in Section 2.8.

[33] See also [Button and Walsh, 2018, ch.5].

[34] There is of course a trivial sense in which these properties are inherited for free, but it has nothing to do with the duality: it's just that PA^* includes PA. This is hardly a sufficient sense anyway: surely we do not consider every set-theoretic extension of a theory to be as valuable as the starting theory!

starting point, not itself a justification. So the appeal to duality does not take us any further than the argument from pure mathematics.

1.3 The argument from subject matter

Mathematics is not purely internally motivated: a lot of the time, it takes its inspiration from the outside world, or at least from other sciences. Accordingly, another very common line of argument for inconsistent mathematics goes as follows. Classical mathematics deals - and can only deal - with that which is consistent; inconsistent mathematics is the only way to properly treat any genuinely inconsistent *subject matter*. As [Sylvan, 2019] poetically puts it: *"there are whole mathematical cities that have been closed off and partially abandoned because of the outbreak of isolated contradictions, notably theories of the very small, infinitesimals, and theories of the very large, Cantor's set theory. Admittedly there have been modern restorations of apparently consistent suburbs of these theories, but the life of these cities has vanished [...]"* (p.63).

The most radical versions of this argument run through the dialetheist literature, from the just cited [Sylvan, 2019] through [Priest, 2006b] to most notably [Weber, 2021a]. Suppose that there are true contradictions in the world - or at least, in our "best" description of the world. Then it seems like we need inconsistent mathematics to talk truthfully about the world. This is what [Mortensen, 1995] calls the *"ontological justification"*, i.e. *"the paraconsistent claim that a contradiction is true or might be true, backed up by one's favorite arguments from semantics or physics"* (p.9). Prima facie, this certainly seems to clear *Importance*: of course we are interested in the best way to describe the world!

Now, the existence of true contradictions does not by itself entail that these contradictions should be included in *every* mathematical theory: someone who thinks the only examples are the classical semantic paradoxes, as argued e.g. in [Beall, 2009], will be at most interested in an inconsistent theory of semantics. Classical mathematics needs not be rejected at all, but rather might be preserved as that part of mathematics which studies consistent things. By the lights of this argument, the scope of inconsistent mathematics is somewhat limited to the location of the contradictions; this does not mean that it needs to be, but any extension will have to be independently justified.

Establishing *Possibility* does not require that an appropriate subject matter exists in a strong sense of existence. One merely has to show that we can *conceive* of some phenomena which inconsistent mathematics would be best suited to capture. Such phenomena may have not occurred or been discovered yet, or maybe they could not happen in our universe at all; this does not prevent us from studying them, although of course *Importance* might be a bit of a harder sell. This is one of the strategies that was employed in the 19th century to argue against the aprioricity of Euclidean geometry, most famously by [Helmholtz, 1876]; similarly, [Priest, 2003] discusses an imaginary situation where an inconsistent arithmetic would be naturally applicable.

The argument from subject matter can also be run under the assumption that some things are merely *perceived* as inconsistent, in the sense that a given experience can lead us to obtain - and be unable to reconcile - incompatible pieces of information: following [Mortensen, 1995], we may call this variant *"the epistemological justification: the argument that any information system with more than one source of information must permit the possibility of conflict between its sources"* (p.9). For example, [Mortensen, 2010] suggests that inconsistent mathematics is necessary to faithfully model certain "gaps" in our cognitive apparatus, most notably when it comes to impossible pictures. Other examples could be our perception of time[35] and motion.[36] In principle defending such applications does not require a commitment to dialetheism, although this may depend on how close to perception one takes truth to be.

Regardless of metaphysical commitments, the argument from subject matter is not as straightforward as it may sound. First of all, it can be difficult to argue that something *really is* inconsistent: it seems to be always open, especially when dealing with abstract concepts, to counterargue that our grasp is confused or incomplete instead, and what we should really do is reconsider our understanding of things until consistency is achieved. In a different but functionally equivalent move, one might accept that something is inconsistent and conclude that what we should do is look for a consistent *replacement*. Moves like this are what, historically, mathematics has always done: for example, the concept of infinitesimal was abandoned in the 19th century because of the trouble involved in treating it within a consistent theory, but the reformulation of analysis in terms of limits was sufficient to recover all of the relevant results and techniques, and not much of a loss was felt. A similar story can be told about the naive conception of set and its replacement by the iterative picture of ZFC.[37] But if the new concepts - by design - do their job just fine, why should we care about the old ones? The argument from subject matter still needs to answer this question, because not every concept is worth exploring, and focusing on obsolete formulations may well be a failure of *Importance*.

Even if we somehow agree that a certain subject matter is inconsistent *and* is

[35] See [Priest, 2006b, ch.15], [Shores, 2016].

[36] See [Priest, 2006b, ch.12], [Mortensen, 2013b].

[37] [Scharp and Shapiro, 2017] discuss both episodes under this light.

worth studying, *Indispensability* remains to be shown: is inconsistent mathematics really the best way to deal with it? This is a salient question because there already are consistent mathematical theories of prima facie inconsistent entities: e.g. [Hinnion, 2003], [Brady, 2006], and [Weir, 2015] all discuss (apparently) consistent naive set theories, while the nonstandard analysis of [Robinson, 2016] provides a consistent treatment of infinitesimals.[38] Despite what Sylvan may have thought, there seems to be plenty of life going on in the neighborhood. So why should we prefer the inconsistent option?

Mortensen (and others) often appeal to *faithfulness*: inconsistent descriptions of inconsistent phenomena are to be favored in virtue of being more accurate. But faithfulness to our intuition of things - let alone our original, confused conception of abstract things like infinitesimals - has rarely been much of a concern for mathematics. More generally, science is full of examples where faithfulness is set aside for the sake of having a more manageable and enlightening theory: this is most notable in the common practice of idealization, e.g. in the theory of ideal gases.[39] While some degree of faithfulness is of course a requirement for the adequacy of a description, the insistence on faithfulness at all costs may be seen as a failure of *Pertinence*; an argument for why we should want to be faithful to an inconsistency in any given case is generally needed.[40]

A dual upshot to the above discussion actually helps the argument from subject matter. Since it is not necessary for a good theory to have the same consistency status as the subject matter that inspires it, there is after all no need to argue that the subject matter is inconsistent for *Pertinence* to be satisfied; rather, one can (and should) argue directly that an inconsistent theory of it would be more valuable.[41]

[38] One might try and counter that some of these theories are in some sense inconsistent enough to count as inconsistent mathematics, in virtue of their talking about intuitively inconsistent objects. I will come back to this in Ch. 4.

[39] Maybe the most famous discussion of this is [Cartwright, 1983]. See also the move from truth to *conformation* in [Longino, 2002, ch.5].

[40] For example, in the case of impossible pictures, Mortensen connects faithfulness to *explanatory power* concerning the feeling of impossibility caused by staring at them.

[41] Such a theory may commit us to the inconsistency of a *different* subject matter. For example, we can imagine a theory of ideal inconsistent sets being proposed as the best way to study the consistent sets of the "real" universe; this is analogous to the theory of ideal gases being justified by the subject matter of real gases. [McCullough-Benner, 2020] shows how unwanted ontological commitments may be dealt with in the case of inconsistent theories being used to describe a consistent subject matter.

1.4 The argument from foundations

The more radical versions of the argument from subject matter have often gone hand in hand with the suggestion that inconsistent mathematics is a better *foundation* than its rivals. Foundation for what, exactly, varies from proposal to proposal. What I am going to call the *radical* argument from foundations argues that (some piece of) inconsistent mathematics would provide a better foundation for *everything*, from philosophy to physics: this was the proposal of [Sylvan, 2019]. On the other hand, the *modest* argument suggests that it can ground some particular discipline, say mathematics or metaphysics.[42]

Depending on one's view of the relationship of a field X to other fields, the modest argument for X may entail the radical argument; but in principle they are distinct.[43] It is important to distinguish the two arguments because their assessment rests on different rules. In order for something to be a better foundation for X, it needs to either better fulfil some of the foundational rules that current foundations for X cover, or convincingly defend the importance of a new role it is best suited to fit. On the other hand, the effectiveness of the radical argument may be judged from any discipline, depending on the role that mathematics is taken to have in the proposed reorganization of knowledge, and more holistic considerations jump in.

I don't have much to say about the radical argument: clearly inconsistent mathematics would be more than justified by having such a holistic impact, much like classical mathematics is now. But it is going to be a hard sell. Aside from the fact that *Possibility* is incredibly far on the horizon - there has barely been any interaction between inconsistent mathematics and natural or social sciences - it seems almost impossible to gain enough support for such a radical change without first finding some strong reasons for entertaining the project within the

[42]Note that there is a difference between taking some inconsistent mathematics to have foundational status, and arguing that paraconsistency should be in some sense foundational. For example, [Carnielli and Coniglio, 2016] (following [Bueno and da Costa, 2007]) argue for a paraconsistency-based paradigm shift in the way we understand science and its philosophy, without thereby taking any inconsistent mathematical theory to itself be a foundation for anything. In fact, the position is compatible with not being much of a mathematical foundationalist at all: the view of e.g. paraconsistent set theory taken by these authors veers towards instrumentalism, as suggested by the fact that no *particular* set theory or logic is singled out as ideal. Of course this is not to say that these views are unable to justify inconsistent mathematics; just that they do not constitute what I am calling an argument from foundations.

[43]It is not sufficient, for such an entailment, to argue that X is the foundation for other disciplines: one should also show that the inconsistency involved in X is actually inherited from the fields it grounds. For example, suppose someone argued that mathematics is such a foundation, and that it needs an inconsistent foundation to properly ground our understanding of the Russell set; it may still be the case that all other disciplines could make do with just the consistent sets, in which case it seems overkill to suggest that *they* need an inconsistent foundation.

many different disciplines themselves.[44] Only with enough local evidence - and therefore, it seems to me, only with the help of other arguments - can the radical argument be convincing. I will hence focus on the modest version for the rest of this section.

The first thing to note is that, at least for mathematics, the modest argument cannot get off the ground (i.e. satisfy *Possibility*) without *classical recapture*: most if not all of classical mathematics should fall under the new foundation.[45] As [Mortensen, 2009a] puts it, *"revisionists neglect the central question of the philosophy of mathematics: what is mathematics? This is hardly to be answered adequately by declaring those parts of mathematics that the theorists don't like, not to be mathematics at all"*. Foundations which reject classical mathematics are no longer foundations for all of mathematics, and so they cannot *just* be sold as a better foundation; rather, one needs to independently argue that this "new" mathematics is also a worthwhile enterprise. One could of course maintain that it is worthwhile precisely in virtue of having a certain kind of foundation which classical mathematics lacks, but this cannot be the *only* reason: we would not want to grant *Importance* to the singleton mathematics consisting of the sentence "$1 + 1 = 2$" in virtue of it being easily grounded in experience. The comparison between revisionist mathematics and classical mathematics is - by definition of "revisionist" - not strictly linear, so it will take more than an appeal to foundational advantages to settle it.

Another important observation is that appealing to a wish to study inconsistent structures is not sufficient for an argument from foundations. First of all, this just pushes the question back: why should we study those inconsistent structures? More importantly, it is not obvious that inconsistent structures would require an inconsistent foundation: much like non-well-founded structures can be modeled within ZFC despite the fact that all ZFC sets are well-founded, naive sets can be modeled within ZFC despite the fact that all ZFC sets are consistent.[46]

With these general preliminaries out of the way, let us look at the most common instance of argument from foundations, which attempts to show that naive set theory is a preferable foundation for mathematics to ZFC. This is usually based on the idea that the naive comprehension schema is true of the concept of set: ZFC is inadequate because it rejects this truth.[47] It is useful here to adopt, following [Incurvati, 2020], a distinction between the *concept* of set, which can be thought of as the more or less universally accepted - if incomplete - core of

[44] This seems to be the strategy of e.g. [Sylvan, 2019] and [Priest, 2006b].

[45] This needs not be understood too literally. Arguably, it suffices that classical mathematics be meaningfully translatable into the new foundation.

[46] See respectively [Aczel, 1988, ch.3] and [Brady, 1989].

[47] See e.g. [Priest, 2006b, ch.2].

the meaning of "set", and the many *conceptions* of set, which are distinct and possibly incompatible ways in which the concept can be specified. In this sense, naive comprehension is not part of the concept of set, since it is rejected by mathematics at large. What we have is a clash of conceptions: ZFC, which is based on the *iterative* conception of set, is the - or at least one - accepted foundation of contemporary mathematics; while some dialetheists argue for a new foundation based on the *naive* conception of set.

Now, a motivated preference for the naive conception does not automatically entail that naive set theory would be a better foundation for mathematics, in the sense of playing any of the roles a foundation is currently required to play in mathematical practice; rather, one has to show specifically that the naive conception is the best conception *for the sake of mathematics*. Mathematics is not the study of the concept of set,[48] so there would be no need to prioritize a foundation that relies on the naive conception even if we believed it to be "correct", clearer, philosophically most satisfying, whatever; different conceptions can coexist, and be better for different purposes. To the extent that the concept of set is used to ground mathematics, it will be the mathematics that determines the best conception for the job; and (alleged) lack of conceptual clarity is not always an obstacle.[49] To be clear, this does not entail that mathematics should also determine which set theory is more adequate as a study of the concept of set, or as a foundation for some other field; that, however, is a different matter.

This is why the argument in [Weber, 2021a] for a foundation accounting for the "correct" concepts of set and boundary is first and foremost a modest argument for *metaphysics*, not for mathematics; a connection is only established through the controversial assumption that the two fields should have a common foundation. One may be tempted to argue this as follows: a proper foundation for metaphysics needs some mathematics X; all mathematics needs a unique foundation; hence, any proper foundation for mathematics should incorporate X. But this does not at all get to the intended conclusion. It doesn't follow that a proper foundation for mathematics should relate to X *in the same way* X occurs in a proper foundation for metaphysics. It is perfectly coherent to think that the naive conception should provide the core of a foundation for metaphysics, yet still hold naive set theory to be perfectly grounded qua mathematics via whatever translation into a classical foundation. The fact that the translation might be awkward or not lossless is

[48] Even hardcore set-theoretic reductionists would have to concede that mathematics can at best be *reduced* to a study of the concept of set, but not *identified* with it.

[49] [Kitcher, 1984] discusses this kind of phenomenon at length in the context of mathematicians rationally and largely unproblematically working with a confused concept of infinitesimal for over a century - not to mention, as Gottlob Frege kept pointing out against deaf ears, a confused concept of number.

in principle irrelevant here: this is the case when reducing the vast majority of mathematics to set theory, yet this is not usually taken by foundationalists to affect its value as a foundation.[50]

So, let us ask directly: is the naive conception best for mathematics? [Maddy, 2019] divides the roles of contemporary mainstream foundational proposals as follows:

- ZFC set theory provides Risk Assessment for inconsistency, a Generous Arena containing all the entities that we might need and where different theories can easily be compared, a Shared Standard of legitimacy (if a theory can be represented in ZFC, it is valid), and a Meta-Mathematical Corral (meta-mathematical notions can be studied within ZFC, via model theory).
- Category theory (together with model theory, according to [Baldwin, 2020]) provides Essential Guidance for mathematical research.
- Univalent foundations are working towards an ideal framework for automated Proof Checking.

Now, ZFC already seemingly suffices as a Generous Arena for all kinds of inconsistent objects,[51] and as a Meta-Mathematical Corral for paraconsistent logics; one might argue that an inconsistent foundation is required to support a *better* theory of inconsistent objects or metamathematics, but then we are just going back to the arguments from Sections 1.3 and 1.1 respectively. Similarly, Essential Guidance appears to be a role that could only be better fulfilled by inconsistent foundations if we had already accepted some inconsistent mathematics, so such a justification would need to rely on supplementary arguments.[52] There is no evidence that any kind of mathematics (inconsistent or otherwise) would fail to be representable in ZFC, so a broader Shared Standard seems to be unnecessary. And as to Proof Checking, I am not aware of any developments in that direction.[53]

It is maybe worth spending a few words on Risk Assessment, to clear some possible confusion. ZFC provides Risk Assessment in the sense that we can

[50] There is also no reason to assume naive set theory would make awkward reductions any less awkward.

[51] Again, complaints about what gets lost in translation are beside the point here, since this happens to plenty of classical mathematics as well, and there is no reason to think naive set theory would make this better.

[52] Incidentally, I think naive set theory has provided anything *but* guidance to inconsistent mathematics, given how its particular quirks require so much extra attention compared to any other branch. Still, something like the paraconsistent category theory of [da Costa et al., 2004] might do the job, in analogy with the classical case.

[53] Although some suggestive remarks can be found in [Friedman and Meyer, 1992].

tell how risky a theory is by looking at the strength of additional set-theoretic axioms required to entertain it. No one is seriously doubting the consistency of ZFC; in fact, no one is doubting the consistency of many of the additional axioms either; but there is a clear hierarchy of axioms of increasing consistency strength (i.e. inconsistency risk).[54] Now, first of all, it is not clear whether naive set theory can recover such a hierarchy: in fact it seems to trivialize it, given that the existence of many large cardinals can be derived almost immediately from naive comprehension.[55] Second, and most important, naive set theory does not appear to be any better than ZFC at proving consistency of theories. An important point here is that the demand for consistency would not simply be replaced by an easier demand for mere nontriviality. Even in nonclassical contexts, there is obvious value in knowing where we can safely reason classically; besides, it is an established mathematical question to find out the consistency strength of a theory, and allowing for inconsistencies neither makes the question less interesting nor is of any help towards answering it. If a classical theory is discovered to be inconsistent, this is not wasted work just because technically the theory is trivialized; rather, the fact that the theory is inconsistent becomes a theorem. Nothing is thus gained, from this perspective, in adopting naive set theory.

Thus, a defense of naive set theory as best foundation for mathematics appears to require either the support of independent arguments for inconsistent mathematics, or an auxiliary argument for a new kind of foundational role in mathematics, whatever that may be.[56] One might be tempted to avoid this by directly attacking the current foundations instead. If the iterative conception is found to be sufficiently inadequate, this may give us a reason to take naive set theory seriously: after all, set-theoretic language is ubiquitous, and *some* conception of set would be helpful in making sense of things at an advanced level, especially in lack of a general consistency proof. That being said, the dialetheist's arguments against the iterative conception - as expressed e.g. in [Priest, 2006b, ch.2] - do not undermine the success of ZFC in playing its intended foundational

[54] See e.g. [Kanamori, 2008].

[55] See [Weber, 2012].

[56] This is where an alliance with the argument from invalidity may come in handy, as I will discuss later.

roles, and so they do nothing to spur a *rejection* of the current foundations.[57] This does not mean that ZFC fulfils all possible foundational roles that we might want to have fulfilled; my point is just that an attack on the current foundations based entirely on the introduction of a new requirement can be dismissed straight away by simply rejecting the idea that the current foundations are concerned with that requirement. This is arguably why category theory failed to displace set theory: while they can both be argued to fulfill a foundational role, they fulfill *different* foundational roles. *If* the new role is considered worthwhile, then the new foundation may live happily alongside the current ones; and this brings us back to the point that the importance of such a role must be positively defended if *Importance* is to be fulfilled.

To conclude this section I should note that *Indispensability* is still an issue: the proposer of inconsistent foundations should argue that consistent reformulations come with a loss of foundational value. For example, if the argument is that we need a foundation based on the naive conception, it needs to be shown that consistent naive set theories on the market are not the best for the job.

1.5 The arguments from philosophy of mathematics

We have seen that inconsistent mathematics can be presented as a contribution to particular philosophical doctrines, like dialetheism (leading to the argument from subject matter) and foundationalism (leading to the argument from foundations). On similar lines, it has been suggested that inconsistent mathematics could be a way to achieve certain projects arising from the *philosophy of mathematics*. For such lines of argument, *Importance* is of course dependent on whether one buys the underlying philosophies to begin with, so I am going to leave it aside; the main issue here is *Possibility*, i.e. whether inconsistent mathematics can actually do its intended job.[58]

[57] See also [Incurvati, 2020, ch.3] for some rebuttals. One argument that might be thought to be an exception claims that the iterative conception cannot deal with category theory, since it cannot represent entities like the category of all categories and the like. But this is just one more anti-naturalist argument in disguise, because as a matter of fact there *are* set-theoretic strategies to deal with this and do whatever mathematical thing a category theorist might need to do, much like there are set-theoretic strategies to discuss set-theoretic universes of all sets (see e.g. [Burgess, 2015, ch.4]). It is far from obvious that philosophical misgivings about the meaning of "all" have any sort of negative impact on the practice - again, this is something that should be directly argued. It should also be mentioned that absolutely zero evidence has been given that naive set theories can reconstruct category theory.

[58] *Pertinence* may also depend on whether we consider certain kinds of philosophically motivated work to be mathematics. But that has nothing to do with inconsistent mathematics in particular, so I leave it aside.

Maybe the most obvious candidate is Gottlob Frege's logicism, which was originally struck down by the discovery of Russell's paradox within his naive set theory. As [Mortensen, 2009a] puts it, *"Set-theoretic foundationalism might survive, and [...] logicism with it, if the alleged contradictions caused by an unrestricted comprehension principle were restricted to regions where little or no damage to mathematics ensues"* (p.632).

A naive set theory supporting a logicist project needs to satisfy three demands:

1. The underlying logic must be properly justified (otherwise the epistemic advantage of grounding mathematics in logic is lost).

2. Classical mathematics must be fully recaptured in one form or another (otherwise we have merely shown that *some* mathematics is logic).[59]

3. Said recapture must be expressible in a purely logical way.

I do not think it is terribly controversial to say that, at this point in time, no inconsistent theory satisfying all these demands has been provided.[60] Furthermore, for the sake of *Indispensability*, one would still have to argue that consistent alternatives are worse.

[Priest, 2006b, ch.10] suggests that inconsistent mathematics, through naive set theory, should be seen as the saviour of *anti-realist conventionalism*, rather than logicism. This is because naive set theory, unlike classical set theory, admits a nominalist-friendly substitutional semantics. Since all of mathematics can be expressed in the language of set theory, this makes mathematical truth analytic - true in virtue of the meaning of the axioms - and the appeal to mysterious mathematical objects is avoided. Furthermore, because naive set theory can express its own semantics, the standard counterargument to the effect that substitutional semantics is a mere retreat to problematic linguistic entities fails.

While the demands on naive set theory posed by this line of argument are slightly different from the logicist demands, the main objection stands unchanged: it is as yet unclear whether such a naive set theory really exists, mainly because of serious issues with achieving a goal-adequate classical recapture. It is not only difficult to adequately reduce the truth of classical mathematics to that of naive set theory; it is also difficult to adequately *express* classical mathematics in the language of naive set theory.[61]

[59]Various strands of neologicism might be happy to weaken this requirement, e.g. the *natural logicism* of [Tennant, 2014]. Such local projects should have an even easier time avoiding inconsistency.

[60]Personally, I stan the Normalized Naive Set Theory of [Istre, 2017], although up to now only intuitionist recapture has been proven, and his conception of logic would likely ruffle some feathers. More on this in Ch. 3.

[61]For more on this, see Section 3.1.

Another ambition of some inconsistent mathematicians has been to salvage the so-called *Hilbert's program*, in the sense of showing the reliability of mathematics by proving its consistency via finitary (and therefore obviously reliable) means.[62] The main line of research on this focuses on relevant arithmetic $\mathbf{R}^\sharp$, and it is most clearly expressed in [Meyer, 2021b]. The starting observation is that, thanks to its inconsistent models, $\mathbf{R}^\sharp$ can be shown to be nontrivial with finitary means: more specifically, for every distinct n, m there are finite models showing that $\mathbf{R}^\sharp$ cannot prove both $n = m$ and $n \neq m$ (in fact, it proves the correct one in each pair).[63] As [Mortensen, 2009a] puts it, *"calculation is untouched by contradiction in relevant arithmetic"* (p.637). Assuming $\mathbf{R}^\sharp$ is able to capture finitary reasoning, the result may be glossed as $\mathbf{R}^\sharp$ being able to prove its own non-triviality; note that PA could never do this on pains of proving its own consistency, which is impossible by Gödel's second incompleteness theorem.[64] This suggests that not all hope for Hilbert's program is lost: while Gödel's theorem still prevents $\mathbf{R}^\sharp$ from proving itself (or PA, for that matter) consistent, this is not an obstacle to it proving itself reliable *enough*, the possibility of inconsistency being quarantined to the far corners of formal arithmetic while the working mathematician can sleep safe and sound.

Unfortunately, as mentioned in Chapter 0, [Friedman and Meyer, 1992] showed that there are truths of PA which $\mathbf{R}^\sharp$ cannot prove, so the nontriviality of $\mathbf{R}^\sharp$ cannot ensure the reliability of PA. The first reaction could be to dismiss such truths as "beyond the scope of informal mathematics", much like one might want to dismiss the classically unprovable Gödel sentence; however, the counterexample that was found is actually an elementary theorem which could be found in any introduction to number theory.[65] Besides, even if we had a good reason to dismiss the counterexample, the fact remains that if $\mathbf{R}^\sharp$ cannot prove the consistency of PA then it also cannot prove that the usual practices - which at the very least appear to include many proof techniques specific to PA - are reliable. This seems to be an intolerable distortion of Hilbert's program unless we join forces with the view that $\mathbf{R}^\sharp$ is the *correct* formalization of informal arithmetic, a view which however was argued in [Weber, 2021b] to be quite implausible in view of the many distinctions added by $\mathbf{R}^\sharp$ which appear nowhere in the practice. Still, the

[62] There is still a lot of discussion on how to best make sense of Hilbert's program. [Detlefsen, 2013] and [Franks, 2009] both reject (for very different reasons) the idea that proving consistency of PA was actually that important for the sake of the program.

[63] There can be, however, no finitary proof of the unprovability of $0 \neq 0$. This is because, by Theorem 20 (p.378), the unprovability of $0 \neq 0$ is equivalent to the consistency of $\mathbf{R}^\sharp$.

[64] See e.g. [Smith, 2013].

[65] Namely, $\exists y \forall z \neg(y \equiv z^2 \mod p)$ for every odd prime p, which is classically equivalent to $\forall x \exists y \forall z \forall a \forall b\, a(2x+1) + b(y - z^2) = 1$.

search remains open for nearby systems that could do what $\mathbf{R}^\sharp$ could not: see e.g. [Logan and Leach-Krouse, 2021].

Finally, let us consider Jean Paul Van Bendegem's proposal of using inconsistent mathematics to ground *strict finitism*, i.e. the view that there are only finitely many mathematical entities.[66] As presented in [Van Bendegem, 1994], the project involves finding a "natural" finite model for every important mathematical theory. However, classically, every complete first-order theory with an infinite model has no finite models.[67] As we have just seen in the case of $\mathbf{R}^\sharp$, paraconsistent logics are a way to overcome this; furthermore, *"the whole idea of consistency proofs started with Hilbert's problem how to control the introduction of ideal elements in a mathematical theory. [...] As long as everything was finite, there was no problem. Hence, as all models are all finite to start with, consistency is not of prime importance any more. Rather triviality is the key issue. [...] It therefore seems unavoidable that strict finitism should go hand in hand with paraconsistent logic"* (p.33). The salient construction is as follows: one takes an infinite structure, finds a "natural" congruence relation with finitely many equivalence classes, and then *identifies* all those elements in the same class. The resulting model will be finite, but it will also be inconsistent: in the simple case of arithmetic, the idea is that *"[i]t is both true and false that the largest number is equal to itself"* (p.34).[68]

Feasibility of the proposal aside, *Indispensability* and *Pertinence* seem somewhat at odds with each other. Van Bendegem's finite models could just as well be studied as *consistent* structures by simply not treating the congruence relation in the construction as true identity. On this perspective, Van Bendegem's finite models are consistent finite quotients, which classical mathematics is perfectly able to manage. Now, this is probably not very satisfying for strict finitist purposes - for one thing, the quotient will not usually be a model of the theory - so this is not strictly speaking a failure of *Indispensability*; but the scarce mathematical upshot of focusing on the inconsistent version may suggest a failure of *Pertinence*.

1.6 The arguments from practice

All the arguments we have seen until now propose inconsistent mathematics as a *new* kind of mathematics, which is valuable insofar as it lets us do things that classical mathematics could not (or not as well). In this section I will discuss several claims to the effect that inconsistent mathematics can in some sense be

[66] Note that this is stronger than simply rejecting the existence of infinite sets - hence the "strict".

[67] This is because such a theory must satisfy the first-order sentence "there exist more than n objects" for every finite n.

[68] These are examples of so-called collapsed models, on which more will be said in Ch.3.

found already in mainstream *practice*, and we just need to recognize it.

The first such argument is historical. The claim that inconsistencies have played - and keep playing - a role in the history of mathematics and science is hardly controversial: in fact, inconsistency in the form of *paradox*, i.e. the surprising appearance of a prima facie contradiction, has been argued by philosophers and mathematicians alike to be a central feature of the development of mathematics.[69] The question is what exactly to make of this. Some have tried to argue that inconsistent mathematics is just what we get if we rationally reconstruct certain scientific practices, both old and new. Of course the controversial claim here is that this is the case for some scientific practices that were *not* simply abandoned or revised following the discovery of a contradiction, otherwise examples abound with no reconstruction needed. We should also distinguish this from the claim, defended e.g. in [Meheus, 2002], that some sort of paraconsistent reasoning was adopted at times where there was no clear way to decide between incompatible theories. The argument I am considering here rather suggests that some accepted theories were *intrinsically* inconsistent and known to be so. [Priest and Sylvan, 1989a] point at naive set theory, the early calculus, even parts of quantum mechanics; and in fact go as far as to claim that *"many other branches of mathematics - perhaps most - were inconsistent in their early versions"* (p.369).[70]

Let us focus on the early calculus, which is probably the most famous example. Infinitesimals were notoriously difficult to interpret (as extensively discussed in [Berkeley, 1754]), and there was no clear way to incorporate the new methods into an axiomatic theory. [Colyvan, 2008] appears to take these difficulties, which eventually led to infinitesimals being discarded altogether as a concept by the end of the 19th century, as evidence of inconsistency: *"There were rules about how these inconsistent mathematical objects, infinitesimals, were to be used. [...] Such rules about what is legitimate and what is not require motivation beyond what does and what does not lead to trouble. [...] for over a hundred years mathematicians and physicists worked with what would seem to be an inconsistent theory of calculus"* (p.28).

Extensive criticism of this kind of move can be found in [Vickers, 2013]. The starting point is *theory eliminativism*, i.e. the rejection of theories as a main unit of historical analysis. This has two main advantages: it sidesteps distracting debates about what a theory is or how to demarcate one, and it forces the historian of science to actually pinpoint and make sense of any alleged inconsistency within the details of the practice rather than yelling "inconsistent theory!" at the first sign

[69] A classical philosophical discussion is [Lakatos, 2015], but see also [Byers, 2010] and [Kleiner and Movshovitz-Hadar, 1994] for some direct testimony from mathematicians.

[70] Compare [McKubre-Jordens and Weber, 2017], where a paraconsistent reconstruction of the measurement of the area of the circle is suggested to show that consistency *was* assumed.

of confusion and calling it a day.[71]

Vickers's rebuttal of inconsistent reconstructions of science goes through a lot of the usual alleged cases; again I am going to focus on the early calculus. The argument here is that it is misguided to take the early calculus to be an inconsistent theory simply because it was never conceived of as a theory, but rather as a set of algorithmic procedures; furthermore, the criteria of application of said procedures were perfectly consistent. Appeals to a conception of the calculus as a set of inconsistent sentences together with a formal logic are not only anachronistic but also utterly inadequate at capturing the practice, especially since *"many mathematicians didn't even pretend they believed in infinitesimals to help them work through proofs"* (p.187). Berkeley's attack should be read as an attack on the many failed attempts at *interpreting* the calculus, rather than on the validity of the practice itself. The explicit contradictions that were occasionally derived, like the same divergent series summing up to different values, were the result of explorations outside the boundaries of the established range of application, and the question of which option (if any) was the correct generalization was left open; as [Kitcher, 1984, ch.8] points out, reasonings outside the safe boundaries were only accepted insofar as they lead to independently verifiable truths, but they were not accepted as proofs per se, which suggests that no contradictions were accepted as true.

Now, it is not my intention to try and settle the debate on how to best understand the early calculus. But what I think the discussion shows is that in general there is no immediate route from the appearance of a contradiction in mathematics, or even from the derivability of a contradiction, to the existence of an accepted inconsistent mathematical theory. Practice can be represented and formalized in many different ways, mathematicians are not a hive mind, and commitments can be partial, vague and dynamic. *Indispensability* - intended as the nonexistence of a convincing consistent reconstruction - is thus a very hard sell, even if *Possibility* - the existence of an inconsistent reconstruction - is not. Furthermore, if the goal of historical accuracy is ditched altogether - as [Brown and Priest, 2004] appear to do, by explicitly distinguishing their "rational reconstruction" from historical reconstructions - then *Importance* is, to me at least, a complete mystery. History can, to be sure, be a valuable inspiration for new mathematics; but the resulting

[71] The idea that theories are inadequate as a unit of historical analysis is hardly novel. [Kuhn, 1970] famously proposed a general notion of paradigm to replace that of theory, and [Kitcher, 1984] followed it up with a more specific notion of mathematical practice. While both notions can arguably still be seen as encompassing theories, they may also be used to dispel superficial claims of inconsistency - as in fact [Kitcher, 1984, ch.10] does. I will go back to this in Ch.6.

work does not automatically gain value by being so inspired.[72]

Another way to read inconsistent mathematics off the practice might be to take as constitutive certain ambiguities or analogies of the sort that show up very commonly in informal mathematical reasoning.[73] Whenever mathematical ideas are transferred via ambiguity or metaphor from one domain to another, this inevitably generates some kind of inconsistency, since not *everything* will make sense in the new context.[74] In particular, *"we can actually work with a sign that is not subject to a single definition, and is interpreted in incoherent ways"* (p.101) [Wagner, 2017].

Wagner's main example concerns the ambiguous role of x in a series $\sum a_n x^n$. In the context of studying generating functions, x can be:

- a *variable*, in which case the series is a function whose convergence domain must be specified;
- an *undetermined constant*, in which case the series is also an undetermined constant and may not be defined when infinite;
- a *placeholder* (like the x in $\lambda x.x$), in which case the series is a purely formal expression and operations involving infinite sums of the coefficients a_n may be undefined.

In practice the perspective is constantly switched, even if technically x cannot be all these things at once on pains of inconsistency.

Now, this is usually not taken to have much of a philosophical upshot because of the idea that the practice *could* in principle be made rigorous. It is not; but mathematicians have good reasons to assume it could be, they have a general idea of how that would work, and that general idea is usually enough to avoid going astray, which for most purposes makes it unnecessary to actually carry out

[72][Heyninck et al., 2018] charitably suggest that such rational reconstructions, even if they do not offer a plausible reconstruction of actual reasoning, may still have value as a sort of non-triviality proof: by providing a model, they *"assert the possibility of the early infinitesimalists having employed a theory that was inconsistent yet not trivial, instead of considering their mathematical practice as incoherent and trivial"*. This seems to me to get the order completely wrong: *of course* the practice was not incoherent and trivial; a helpful model should help us understand *why*!

[73]I am not aware of anyone having explicitly made this argument in print for mathematics - although the way [Priest and Sylvan, 1989a] and [Priest and Sylvan, 1989b] take roughly every mathematical and philosophical theory ever developed to be inconsistent may suggest something along these lines. Either way, it will be good to address it since it is very relevant to the discussion on what it means for mathematics to be inconsistent.

[74][Lakoff and Núñez, 2000] is the classical (and notorious) theory of how this might work on a cognitive level. See also the discussion of productive ambiguity in [Grosholz, 2007].

a rigorization process.[75] Furthermore, as already mentioned, certain strands of unrigorous reasoning might be completely safe insofar as they are merely used to *discover* results that can then be independently verified.[76] Either way, no contradiction is actually derived. That being said, it may still be the case that while these practices could always be consistently *reconstructed*, they could not be faithfully captured by a consistent formalization in the classical sense.

So, to turn this into an argument from practice, there seem to be two options:

1. take the informal, ambiguous state of things to already count as inconsistent mathematics;

2. insist on the usefulness of a faithful formalization of some globally inconsistent slice of practice.

If we accept (1), then inconsistent mathematics is just mathematics as it has always been done. No new mathematical theories are thereby suggested, and no change in practice seems to be required by such a revelation; at best this contributes to a philosophical plea for paying more attention to informal practice - hardly a position unique to the inconsistent mathematician!

On the other hand, (2) seems to have no basis whatsoever in practice. First of all, *Possibility* is dubious: it is far from obvious that this is the kind of thing that *can* be faithfully formalized, involving as it does all the fluidity and ambiguity that formalizations are usually intended to remove. Furthermore, what grounds are there to think that it *would* be better to formalize the ambiguity as inconsistent, especially given the historical roots of formalization as something meant to *reduce* the threat of inconsistencies? On one hand, insisting on faithfulness solely for the sake of providing a more accurate model of practice looks like a failure of *Importance*: it is not at all clear how this level of formal precision would improve our understanding of practice. On the other hand, if the idea is that such a formalization would be a useful intermediate step for further mathematical work - the kind of hope that spurred the development of model theory - then it should be defended on mathematical grounds: this reduces to the argument from pure maths, where the inconsistent mathematician is expected to show the mathematical fruits of inconsistent formalizations.

While for Wagner informal practice is naturally inconsistent, he seems to accept the standard view that one of the goals of formalization is to get rid of the

[75]Note that this is different from the situation in e.g. the early calculus, where it was far from obvious how to approach the matter of rigorization and there were frequent debates about the validity of certain reasonings.

[76]This does not make such reasoning superfluous, as verification techniques often require having already a conjectured answer among the infinitely many possibilities. One standard example is proving by induction the sum of a series.

inconsistencies. On the other hand, [Priest, 2006b, ch.3] takes the inconsistency of practice to *follow* from the inconsistency of formal mathematics. Roughly, his argument goes as follows:

1. The totality of informal mathematical proof procedures can be fully formalized within a recursive theory T.

2. Gödel's first incompleteness theorem applies to T, i.e. T does not prove the Gödel sentence g.

3. But g is informally provable, and thus provable in T.

4. Thus, the notion of mathematical provability is inherently inconsistent.

The most thorough - and, I think, quite convincing - rebuttal comes from [Tanswell, 2016]. Tanswell argues that, first of all, Priest's argument is incomplete: since there cannot be a unique formalization of practice, the argument must require quantification over all *adequate* formalizations, and so a criterion of adequacy must be specified. Furthermore, the possibility of any such formalization seems suspect: it seems impossible for a single theory to properly capture the specificity of different areas, there are serious issues with the formalization of diagrammatic proofs, and it is unclear how branches relying on different logics could ever be represented faithfully on the basis of a single logic. Finally, and most damningly, Priest's argument is changing the subject: to prove that the notion of *informal* proof is inherently inconsistent, he starts by formalizing it. Yet informal proof might be inherently informal or incomplete, which would prevent Gödel's theorem from applying.

1.7 The argument from invalidity

Next, I want to discuss the idea that inconsistent mathematics could be seen as a solution to the faults of classical logic. Much paraconsistent literature has argued that classical logic is *invalid*, i.e. fails to be a correct theory of valid reasoning: see e.g. [Sylvan et al., 1982] and [Priest, 2006b]. This could lead to the idea that classical mathematics is invalid, or at least that it cannot be considered valid until it has ben recaptured in a valid logic. If this is so, maybe we need inconsistent mathematics to replace it.

Now, first of all, *Pertinence* does not seem to follow, because a choice of logic rarely forces a contradiction. The vast majority of paraconsistent logics preserve the consistency of a set of axioms, so that any classical theory would remain consistent under a change of logic.[77] Inconsistent models may become available depending on the logic, but - as I have already argued - this is not much of a reason to call something inconsistent mathematics unless the inconsistent models themselves become the object of study; and we can at least *conceive* of a branch of mathematics that is not interested in model theory.

More importantly, most paraconsistent logicians - and in particular, many defenders of inconsistent mathematics - also defend some kind of classical recapture to the effect that classical reasoning is just fine in classical mathematical contexts. One very direct way to achieve this is by showing that classical mathematics is derivable within inconsistent mathematics.[78] More generally, one may treat classical reasoning as *defeasible*: roughly, if we have no reason not to expect a contradiction, then it is rational to reason classically.[79] This appears to validate classical mathematics because no one expects a contradiction to suddenly turn up in, say, PA or ZFC. In fact, it is not even clear how such an expectation could genuinely arise in mathematics: the history of mathematics is full chock of unexpected contradictions, but they were always taken to be a sign that revision was necessary, or at least that the theory wasn't fully understood yet. At no point did the appearance of contradictions convince the community that contradictions were something to be definitely accepted.

Classical recapture appears to stop the argument from invalidity dead in its tracks. Even if classical logic is invalid, as long as it remains legitimate in (classical) mathematics there is no apparent need to change anything in mathematics, and the argument is essentially reduced to a form of the argument from pure mathematics: why not just *try* mathematics based on a different kind of logic? The fact that the logic in question is valid does not, by itself, suggest *Importance*, i.e. that we should build mathematics on that logic; at best, it suggests *Possibility*.

To make this a bit more urgent, one could try and argue that inconsistent mathematics is required in order to *justify* classical mathematics: sure, maybe we can reason classically after all, but we still need inconsistent mathematics to show it. In general, this requires no inconsistent mathematics at all: it suffices to show that the proposed logic reduces to classical logic in consistent contexts. But it might be that such a reduction does not hold in general; or, one might require justification to also eliminate the risk that classical mathematical theories might be inconsistent after all. Either way, it might be necessary to show directly how certain mathematical assumptions give us the ability to recover classical

[77] There are exceptions, e.g. so-called inconsistent logics. For reasons I will explain in Section 4.2, I do not think the use of such logics suffices to call something inconsistent mathematics anyway.

[78] See e.g. [Carnielli and Coniglio, 2013].

[79] See e.g. [Beall, 2013a] and [Mares, 2004, ch.10].

mathematics within inconsistent mathematics.[80] This is essentially an argument from foundations, where the new foundational role - justification - is argued for by pointing at the general invalidity of classical logic.[81] It also resembles the argument from logicism, insofar as inconsistent mathematics thus motivated could be set aside the second it is done justifying classical mathematics, at which point we could happily *go back* to classical mathematics.

Can the appeal to foundationalism be avoided? Suppose that the goal of classical recapture is rejected altogether, and classical mathematics is judged to be an illegitimate practice in virtue of its use of classical logic. Then the argument from invalidity might go through on its own: we need a replacement, and inconsistent mathematics could be it. However, I am not aware of any paraconsistent attempt to spell out what exactly is wrong with classical *mathematical* reasoning, beyond an attack on classical reasoning in general. This leaves it open why we should not assume that (classical) mathematics is a special case, given the overwhelming agreement on the validity of mainstream mathematical reasoning. It simply does not follow, from the fact that some classical arguments are invalid, that classical mathematicians are reasoning incorrectly.

As an example of how invalidity charges can fail to apply to mathematics, consider the relevantist attack on the material conditional's inability to ensure that antecedent and consequent are relevant to each other. [Mares, 2004, ch.1] brings up as an example of irrelevant non-sequitur the classically valid deduction of Fermat's Last Theorem (which is true) from "the sky is blue". Mares correctly observes that this argument would never be accepted as a proof of FLT, or turned into a theorem. But if anything, that just shows that mathematicians are perfectly able to distinguish between those classical arguments that are useful and those that are not. The argument here is completely non-informative because we cannot prove its validity without already knowing that FLT holds, thus defeating the purpose of the proof.[82] Mathematics has plenty of internal criteria to determine the value of a proof, and classical validity is hardly sufficient; and even if it was argued that relevant validity is closer to a necessary and sufficient condition - which, as already discussed, seems highly questionable - this still wouldn't do anything to show that

[80] One striking example that comes to mind, although it does not involve a paraconsistent logic, is the recovery of classical mathematics within quantum mathematics (based on so-called quantum logic), as described by [Dunn, 1980]. The limitations of quantum logic are simply *derived away* as soon as the Peano axioms or the set theory axioms are introduced.

[81] Which is not to say the invalidity of classical logic is in itself sufficient for requiring a new foundation. The idea that mathematics needs *this* kind of foundation at all is itself a big philosophical commitment.

[82] Mares himself recognizes that this is not really a mathematical concern: in fact, [Mares, 2004, ch.10] argues that classical logic is admissible in (classical) mathematics.

the way classical mathematicians reason in practice is faulty.[83]

Note also that, in order to attack the validity of classical mathematics, it is not enough to argue that classical logic delivers the wrong judgements when it comes to, say, inconsistent objects, because classical mathematics is *not* countenancing the existence of inconsistent objects (in the sense in which it would trivialize them, anyway) in the universes under consideration; and the same goes for any sort of nonclassical objects. This is only slightly less tautological than it sounds: my point is not that mathematicians are entitled to reason classically about classical structures, but rather that they are entitled to reason *as they do* about classical structures. It doesn't matter whether classical logic is a perfect match to informal mathematical reasoning or not; what matters is that it is not leading classical mathematicians astray when it comes to the structures they are studying, because the structures they are studying are *intentionally* classical. But if the invalidity of classical logic has no effect on classical practices, then it cannot be used to justify their replacement. Again, it may well justify their *extension* by joining forces with foundationalism; but that requires classical recapture.

[Weber, 2021a] nevertheless worries that classical logic can lead us to false mathematical results: *"wherever a theorem is usually proved using disjunctive syllogism or other classically-only valid inference, there should be an alternative proof—perhaps still needing to be discovered—that leads to the same or similar result using only paraconsistently valid inferences. In the event that there is no such alternative proof, then the theorem is essentially classical and, depending on the case, may not be correct"* (pp.105-106). Correct with respect to what? Again, it seems to be a truism that classical theorems are correct with respect to classical structures. But classical mathematicians *are* studying classical structures. Maybe they are not studying the "real" universe, whatever that means; but this has nothing to do with the validity of their reasoning, unless we throw charity down the drain and understand them as studying something else. The idea that classical mathematics would be undermined by failing to provide a true description of the universe is simply out of touch with how mathematics has been practised in the last century; truth can always be relativized to a structure or formal system, which in turn can be locally justified on fruitfulness grounds, so neither the physical world nor any alleged a priori intuition have the power to delegitimize any piece of pure

[83][Meyer and Mortensen, 1987] seem to accept this, and merely claim for a replacement of the *formalization* of practice: *"As a recipe for reconstructing mathematical reason, there always was a good deal wrong with classical logic–if only because intuitive reason is subject to those relevant constraints that its truth-functional regimentation ignores"* (p.17). But of course this only makes sense if we have classical recapture (at least at the informal level), and in fact - as we have already seen - both authors support it.

mathematics.[84]

Now, for all that I've said in this section, one could of course argue that classical mathematicians are nevertheless guilty of omission: maybe they *should* be looking at something which classically would be trivialized, and their adoption of classical logic is incorrectly convincing them otherwise. However, that is *not* an argument from invalidity, since invalidity is doing nothing here to justify *why* they should. What kind of argument this is depends on the reasons given for studying this something: if it is for purely mathematical reasons, it is an argument from pure mathematics; if it is because of worldly applications, it is an argument from subject matter; and so on. This is not to say that such an argument cannot lead to the conclusion that classical logic is invalid; however, said invalidity carries no independent force in the argument.[85]

1.8 The argument from liberation

So, it seems that there is no easy route from the invalidity of classical logic to the invalidity of classical mathematics. However, there may be other issues with classical logic which are inherited by classical mathematics, and suggest a move towards inconsistent mathematics. The argument from *liberation* proposes that the issues in question are social.

As a starting point, consider Val Plumwood's charge that classical logic contributes to the naturalization of *dualisms*, which are defined as a particular kind of dichotomy underlying most forms of systemic oppression.[86] Paradigmatic examples are what she singles out as the central dualisms of Western thought: man/woman, mind/body, civilized/primitive, and human/nature. *"The master perspectives expressed in dualistic forms of rationality are systematically distorted in ways which make them unable to recognise the other, to acknowledge dependency on the contribution of the other, who is constructed as part of a lower*

[84]For more on how we got here and the contemporary status of mathematics, see [Maddy, 2011, ch.1]. See also [Fletcher, 2017] for an analogous argument against the identification of geometry with self-evident metaphysics of space. Of course, some classical mathematicians are old-school Platonists taking their theorems to describe the one true mathematical universe. But this is inconsequential: Platonists can be correct in their everyday reasoning, yet mistaken about the Platonist reading of their work. Insofar as there is any notion of absolute truth still kicking, it is tentatively inferred from the success of the practice, not the other way around; nothing about the practice changes if we give up on it, as evidenced from the fact that there are many other mainstream philosophies of mathematics floating around.

[85]Invalidity is also not a necessary condition: some logical pluralists are happy to understand validity as context-relative, e.g. [Shapiro, 2014].

[86]The terms "dichotomy" and "dualism" are not always used this way in the literature, but I'm going to stick with Plumwood's usage here.

order alien to the centre. These forms of rationality are unable to acknowledge the other as one who is essential and unique, non-interchangeable and non-replaceable. The other cannot be recognised as an independent centre of needs and ends, and therefore as a centre of resistance and limitation which is not infinitely manipulable. This provides the cultural grounding for an ideological structure which justifies many different forms of oppression, including male-centredness, Euro-centredness, ethno-centredness, human-centredness, and many more" [Plumwood, 1993b, p.453].[87]

Plumwood was certainly not the first feminist theorist to recognize the oppressive upshot of the dominant readings of rationality. Most famously, [Nye, 1990] argued that the very idea of formal logic is intrinsically antithetical to feminist aims. Plumwood's response is that Nye is playing into the master's hands by incorrectly identifying formal logic with *classical* logic, ignoring the possibility that nonclassical logics may provide less oppressive forms of rationality which may then be adopted for feminist purposes.[88]

Plumwood identifies five central structural features of dualism, which are reflected by classical logic when $\neg p$ is interpreted as "the other of p".

1. Incorporation: the other is defined in relation to the master, as a lack or negativity. In particular, p fully controls its other $\neg p$.

2. Hyperseparation: differences between master and other are maximized, while shared qualities are minimized. Explosion ensures that p and its other are kept *"at a maximum distance, so that they can never be brought together (even in thought)"* [Plumwood, 1993b, p.455].

3. Backgrounding: the other's essential contribution or reality is denied. The classical conditional allows for the suppression of true premises, thus making it possible to hide the other's contribution to the conclusion.[89]

4. Instrumentalism: the other is objectified and conceived as means to the master's ends. This is allowed by the fact that *"[...] any truth can be substituted for any other truth while preserving implicational properties"* (p.455).

[87]For more on the pernicious role of dualisms in Western philosophy, see [Lloyd, 1993], [Plumwood, 1993a], and [Prokhovnik, 2002].

[88]More feminist arguments against Nye's rejection of logic can be found e.g. in [Ayim, 1995] and [Hass, 1999]. More discussion of the interaction between formal logic and feminist theory can be found e.g. in [Falmagne and Hass, 2002] and [Russell, 2023].

[89]For example, classically it is the case that $(p \wedge q \to r) \to (p \to (q \to r))$. So if p is true it is classically acceptable to say that $q \to r$.

5. Homogenisation: differences among the dominated are disregarded, usually through stereotyping. This is facilitated by truth-functionality being the only criterion of identity.[90]

Plumwood suggests that *"[d]ualisms are not universal features of human thought, but conceptual responses to and foundations for social domination"* (p.444). Other modes of thought - even *rational* thought - are possible, but the naturalization of classical logic as the standard for rationality has contributed (and continues to contribute) to the naturalization of dualisms through naturalization of their logical structure, which in turn makes domination look natural: *"The 'naturalness' of classical logic is the 'naturalness' of domination, of concepts of otherness framed in terms of the perspective of the master"* (p.454). Classical logic is then not neutral, and its choice was not merely mandated by some notion of "objective rationality", but rather serves the purposes of the master: in fact, the usual notions of objectivity and rationality are themselves deeply complicit in the Western history of oppression.[91]

Note that the claim here is not that dichotomies expressed in classical logic are to be *identified* with dualisms. I agree with [Russell, 2020] that there does not appear to be any dualism between, say, odd and even numbers; but I think Plumwood would agree as well. This is because dualisms are not merely formal: they are a *concrete* relation of dominance where one side is treated as inferior.[92] The claim is that classical dichotomies and dualisms share an underlying logical structure, so insofar as we reason about dichotomies under the default assumption that they work classically, we both make it very easy for dualisms to form and very hard for them to be challenged. It is besides the point that formally speaking, say, $\neg A$ and A could be switched by double negation laws,[93] because a hierarchy can be superimposed in practice: the role of classical logic is simply to fix a certain kind of relationship between A and $\neg A$ which allows for - and arguably facilitates - that superimposition. If anything, the symmetry between A and $\neg A$ represents the fact that dualisms would be damaging even if the hierarchy was reversed, a fact which underlies Plumwood's rejection of feminist strategies of "uncritical reversal".[94]

That being said, it is also important to note that dualisms are not damaging *only*

[90]See also [Ferguson, 2023] for discussion of homogenization in connection with classical negation and the Law of Excluded Middle.

[91]The effects of this are still felt today, of course: see e.g. [Oliver, 1991]. Readers on the lower side of a dualism (or worse, essentially incompatible with one) may also think of all the times their lived experience was dismissed on "rational" and "objective" grounds.

[92]Plumwood also claims that *"classical logic is the closest **approximation** to the dualistic structure"* (p.454, emphasis mine), leaving the door open to some formal disconnect.

[93]This objection is raised by [Garavaso, 2016].

[94]See [Plumwood, 1993a, ch.1].

due to their hierarchical aspect. Rather, their logical structure can be damaging on its own. To see this, let us look at the man/woman dualism in a bit more detail. In the model presented in [Dembroff, 2019], four axes of the dominant, dualistic Western view of gender are distinguished: biological (man = male, woman = female), hierarchical (man > woman), teleological (one's features are - or ought to be - determined by gender), and binary. The latter refers to the fact that *"the genders men and women are binary, discrete, immutable, exclusive, and exhaustive"* (p.15); it is arguably the most crucial axis, insofar as it *"provides the conceptual framework that constrains and conjoins the content of the biological and teleological axes"* (p.15). This is exactly what classical logic provides, simply by letting woman = not man; in particular, the possibility to exist outside the binary is denied. This causes harm to all those who in practice do not fit the binary, even without contribution from the hierarchical axis; the harm lies in the erasure of their lived experience (or the legitimacy thereof), and would hardly be solved by allowing every gender outlier to adopt the "man" (dominant) label.[95] The connection between classical logic and identity erasure is explicitly acknowledged by [Eckert and Donahue, 2020]:[96] *"classical logic may not be an ideal logic for LGBTQI theorizing, since we want to take seriously people's claims about their gender identity, which combine, adjust or altogether deny the gender binary. If debate and discussion of gender identity takes classical logic as default, the structure of argumentative space ends up (already) binary in character. Activists should be especially wary to give up their home ground of relevant default"* (p.440).

The recognition of nonclassical logics provides a conceptual way out of the grip of dualisms, by showing their logical structure can be questioned. Desiderata on a non-dualistic logic may include some sort of non-explosive negation, in order to provide a non-exclusionary concept of difference; an implication that does not suppress true premises, in order to avoid backgrounding; and a more fine-grained notion of equivalence, to avoid instrumentalism and homogenisation. It is worth noting that *"[t]hese desiderata make good sense even if we were to view logic as neutral but [...] able to be weaponized (a less radical view than Plumwood's)"* [Eckert and Donahue, 2020, p.442]. Plumwood's own suggestion is to look at *weak*

[95] This kind of erasure is not just a theoretical harm. One very material consequence is that all kinds of infrastructures are just not built with these people in mind. Another is that the boundaries of the binary are policed quite harshly, in ways that range from societal pressures to outright violence. And that's not even touching on the psychological harm.

[96] See also [Eckert, 202x] and [Ferguson, 2023].

relevant logics,[97] but there have been other proposals.[98] It is not important to my point here that dualisms are understood specifically in the Plumwood way, or that her particular solution is deemed ideal: what matters is that some dualisms are recognized as harmful and that some typical features of classical logic - in particular, Explosion - are essentially involved.

Can this sort of criticism of classical logic be turned into an argument against classical mathematics, and in favor of inconsistent mathematics? Of course, the standard view is that mathematics is at least in principle removed from social or practical concerns, concerned only with an abstract agent-independent universe whose rules are not for us to decide. "Man" and "woman" are not mathematical entities; as problematic as classical laws may be when applied to worldly concepts, they may just *be* the laws governing (classical) mathematics, or at the very least be inoffensive in that context. In order for Plumwood's critique to trickle down to mathematics, it is then important to show directly that classical mathematics's use of classical logic does in fact contribute to the naturalization of dualisms. Fortunately (so to speak) some suggestions of this sort can already be found in the literature, once we start thinking about it in Plumwoodian terms.

First, mathematics is generally presented as universal and necessary in just the same way logic is - and just as illegitimately. Here is [Burton, 1995] making this point: *"Mathematics tends to be taught with a heavy reliance upon written texts which removes its conjectural nature, presenting it as inert information which should not be questioned. [...] Language is pre-digested in the text, assuming that meaning is communicated and is non-negotiable"* (p.276). In fact, *"the dominance of a Eurocentric (and male) mathematical hegemony [...] has created a judgmental situation within the discipline whereby, for example, deciding what constitutes powerful mathematics, or when a proof proves and what form a rigorous argument takes, is dictated and reinforced by those in influential positions"* (p.279).[99]

This is not only analogous to the naturalization of classical logic, but goes hand in hand with it: starting in the 20th century the hegemony of mainstream

[97] See Section 2.3.

[98] For example, [Ferguson, 2023] argues that relevance might be too strong a constraint, and semi-relevant logics like **RM** may also work; meanwhile, [Eichler, 2018] notes that Native American logics have a long history of being deeply non-dualistic. Not all proposals for a feminist logic have focused on the problem of dualisms: see e.g. [Hart, 1993] and [Olkowski, 2002].

[99] Burton's feminist epistemology is partly inspired by social constructivism, which takes objectivity to be inextricably tied to social factors: it is this feature that makes mathematics so susceptible to gatekeeping from the dominant class. For a book-length defense of social constructivism in mathematics, see [Ernest, 1998]. My argument here does not depend on accepting social constructivism; it suffices that social factors can influence the development and formulation of mathematics, and that alternative (in the very weak sense of deviating from the mainstream) mathematics can exist. These are fairly uncontroversial assumptions.

Western maths - the rational field par excellence - is directly connected with the hegemony of the classical logic which is said to provide its foundations and basic language.[100] Because of the commonplace cumulative view of mathematics, this retroactively identifies mathematics (qua collection of necessary truths) with classical mathematics in the contemporary sense. Nowadays, any piece of nonstandard mathematics (e.g. nonstandard analysis, or non-well-founded set theory) tends to be either reassimilated into canon by classical translation, or written away as a mere formal system which can be accounted for by classical metamathematics.[101] Nonclassical logics may be recognized as a (classical) mathematical object of study, but they are by and large not intended as something we do mathematics *with*, and any suggestions to the contrary (e.g. the constructive analysis of [Bishop and Bridges, 2012]) have gained little support. In logic as in mathematics, we have a naturalization of certain dominant perspectives, often to the extent that genuine alternatives disappear altogether. As [Bloor, 1991] famously put it: *"One of the reasons why there appears to be no alternative to our mathematics is because we routinely disallow it. We push the possibility aside, rendering it invisible or defining it as error or as nonmathematics"* (p.180).

Not only the necessity, but the *neutrality* of mathematics has been questioned as well. For example, [Ernest, 2018] argues that ascribing ethics-freeness to mathematics is dangerous because of the way mathematics educates to binary, instrumental thinking. *"Thus a training in mathematics is also a training in accepting that complex problems can be solved unambiguously with clear-cut right or wrong answers, with solution methods that lead to unique correct solutions. Within the domain of pure mathematical reasoning, problems, methods and solutions may be value-free and ethically neutral. [...] But carrying these beliefs beyond mathematics to the more complex and ambiguous problems of the human world leads to a false sense of certainty, and encourages an instrumental and technical approach to daily problems"* (p.197). Of course, one does not *need* to carry these beliefs beyond mathematics; but insofar as pure mathematical reasoning

[100]The reader who got their perspective skewed by spending too much time around nonclassical logicians is invited to consult, as a paradigmatic example, the Princeton Companion to Mathematics [Gowers et al., 2008]. The *"language and grammar of mathematics"* is built out of classical connectives (Sect. I.2); *"ZFC is currently accepted as the standard formal system in which to develop mathematics"* (Sect. IV.22); "logic" refers to "classical logic" throughout (most notably, in Sect. IV.23); and in over a thousand pages there is not a single mention of nonclassical logics, or of any piece of mathematics based on nonclassical logics (save for *historical* references to intuitionism).

[101]The continuum of nonstandard analysis contradicts the classical one due to the presence of infinitesimals, but it can also be construed as a classical *extension* of the classical continuum [Robinson, 2016]. Non-well-founded set theory has axioms that contradict ZFC, yet it can also be straightforwardly interpreted as the study of a substructure of the classical universe [Aczel, 1988, Ch.3].

is praised within Western societies as the highest form of rationality, its influence cannot be underestimated. So the problem is not restricted to applied mathematics; rather, *"mathematics through its actions on the mind is already implicated in some potentially harmful outcomes even before it is deliberately applied in social, scientific and technological applications"* (p.206).

Here Ernest is talking about mathematics in general, but once again we can Plumwood this up and note that the instrumental thinking associated with mathematics can be connected with the use of material implication, which in turn is connected with the overwhelmingly popular picture of mathematics being reducible to the extensional - to classical set theory and truth-functionality.[102] The focus on clear-cut right or wrong answers is also supported by classical mathematics both in virtue of its alleged necessity, *and* in virtue of its standardization of Boolean negation.[103]

Going beyond pure mathematics, obviously dualisms appear in mathematics whenever they are that to which mathematics is applied; and since dualisms are everywhere in Western societies, we can expect dualisms to appear in applied mathematics a lot. Consider for example the discussion in [Wagner, 2017] of the *marriage problem*, which involves finding an algorithm to match people according to their preferences in a stable way, i.e. such that in the end there is no pair of individuals preferring each other to their assigned spouses. The original solution to the problem - which, of course, took heterosexuality, monogamy, and a strict gender binary for granted - is the so-called Gale-Shapley algorithm: *"“[...] every boy proposes to his highest preference and every girl refuses all but her best proposal,” keeping her favorite suitor on hold. Each rejected boy continues to propose to his next highest preferences, and each girl continues refusing all but her highest preference among the boys who actually propose to her at any given time, possibly rejecting a boy whose proposal she had previously kept on hold. “This goes on until no changes [new proposals] occur; then every girl marries her only proposer she has not yet refused”"* (p.114).[104] Besides being blatantly inspired by and reinforcing gender stereotypes, this solution is male-optimal, and was

[102]The reduction was harshly criticized by logicians in the relevant school: see e.g. [Meyer and Sylvan, 1977]. Feminist philosophers of science have also argued for the inadequacy of this reductionism to represent what is actually going on in, say, biology: see e.g. [Nye, 2002].

[103]The issue is not one of having only two truth values, merely the way in which they are cashed out. First of all, the usual many-valued logics - even when paraconsistent - support backgrounding and instrumentalism in much the same way classical logic does, because of the truth-functional conditional; furthermore, the classical binary is always lurking in the form of the dichotomy between designated and undesignated values. Meanwhile, as we will see in Section 2.3, the kind of logics suggested by Plumwood - weak relevant logics - have a two-valued semantics, but the relationship between True and False there is not dualistic.

[104]The in-quote citations are from [Bollobás, 1998].

noted only several years later by [McVitie and Wilson, 1971] to be also female-pessimal.[105] Furthermore, the motivating interpretation of the problem was mostly abandoned once formal generalizations started contradicting any of the stereotyped assumptions.

Putting our Plumwoodian glasses on, we can see that the historical treatment of the marriage problem can be taken to be problematic in virtue of both initially reflecting the man-woman dualism, *and* refusing to question it even when the mathematics itself presented the opportunity. The formal presentation takes men and women to be hyperseparated: the group - which, again, is with false generality introduced as a group of *any* people - is divided into A and (classical) not-A. There is no situation in which such a division is not exclusive, or in which domain and range of the preference function intersect, etc. Yet the suppression of such situations is generated from the formal division only because of the classical negation involved. The decision to include any deviant situations out of a taste for generalizations only comes later; but by that time, rather than risk challenging the dualism, the interpretation is dropped altogether.

Conversely, the man/woman dualism is essentially used to express a certain abstract situation: as a mere dichotomy, it would fail to carve the possibility space in the intended way. The dualism also pervades the solution: women are homogenized through stereotyping, being all cast in the same passive role which eventually leads to engagement without consent, and instrumentalized by the male-optimality of the solution. This all suggests that dualisms do affect the choice of which mathematics is developed, and therefore they have a part in what is taken to be mainstream mathematics, namely *classical* mathematics. Since classical mathematics is itself naturalized, this leads to mathematics itself painting those originating dualisms as even more natural, and so on.[106]

To recap: classical mathematics is naturalized to the point of excluding all possible alternatives by fiat, it is inspired by and supportive of dualisms, and it educates to the very kind of thinking that makes dualisms look inevitable. Given all these considerations, it seems fair to say that if classical logic is problematic on grounds of naturalizing dualisms, then so is classical mathematics. We can then

[105]This means that *"no stable matching exists, where any man marries a woman whom he prefers over the one assigned by the Gale-Shapley algorithm; on the other hand, no stable matching exists that marries any woman to a man less desirable to her than the one assigned by the Gale-Shapley algorithm"* [Wagner, 2017, p.117].

[106]In fact, [Wagner, 2017, ch.4] goes even further in arguing that not only do societal biases influence mathematics, but they occasionally do so by hindering creativity and progress. Consider for example the *ménage problem*, which asks for the number of ways people can seat at a table so that noone is seated next to their partner. It took decades to find a straightforward proof: [Bogart and Doyle, 1986] conjecture that the reason for such a late discovery is that it required contradicting the assumption that women be seated first.

follow [Plumwood, 1993b] in demanding, even in mathematics, *"the development of alternative accounts of rationality, otherness and difference [...] so that modes of reasoning which treat the other in terms of domination can no longer pass without question as normal and natural"* (p.459). But inconsistent mathematics can provide just that: it can counteract the naturalization of dualisms through both inconsistent practices, which explicitly contradict standard assumptions and thus undermine their absoluteness, and the use of paraconsistent logics, which represent less dualistic ways of thinking insofar as they reject Explosion.

Let us see how our four requirements are met. *Possibility* follows from the availability of less dualistic logics and inconsistent interpretations, together with the fact that we can make use of them in mathematics. Almost every example of inconsistent mathematics in the literature can be seen as showing this, regardless of whether they were developed with liberation in mind; still, to drive the point home, a toy example of inconsistent mathematics developed with liberation in mind will be presented in Ch.5. One may wonder whether *Possibility* should not require something stronger, namely that liberation should be *fully achievable*, in the sense of forever purging mathematics from the harmful consequences described above. I find this goal both utopian and unnecessary: making society *better* is a valid goal regardless of whether we can ever make it perfect.[107]

Once the problematic status of classical mathematics is acknowledged, *Importance* should be obvious: systemic oppression is bad, fighting it is important. Of course there can still be disagreement on how important it is to fight it on this front, but a simple attitude of "every step counts" will do the job. *Pertinence* holds insofar as inconsistency is singled out as a tool for denaturalization, and denaturalization is required specifically in the context of mathematics; this does not of course prevent similar arguments from justifying the use of different tools as well, or denaturalizing even more fields.[108] Finally, *Indispensability* is achieved as a matter of degree: paraconsistent logics are usually less dualistic and contradictions are a strong mark of subversiveness, so inconsistent practices come out as usually more effective at denaturalization compared to other nonstandard practices.[109]

Indispensability is probably the hardest sell here, so let me say a few more

[107] There is of course the important question of whether certain small improvements now may endanger the possibility of larger improvements down the line, but at least for the time being I do not see any reason why this would even be a risk here. I will say a bit more about the big picture in Ch.7.

[108] Metaphysics certainly comes to mind, although that is a story for another thesis.

[109] Admittedly, this way of putting it makes the question of *which* inconsistent practices are the best at denaturalization particularly salient. I will not tackle this question here. Instead, in Sections 2.8 and 4.5 I will extend the argument from liberation from an argument for particular inconsistent practices to an argument for a general *attitude* towards logic and inconsistency.

words on it. Classical mathematics may of course try to solve the problem on its own by keeping an open mind to alternative classical formulations. However, this is not the same as allowing for inconsistent interpretations. We can only open up a problem to alternative formulations *if we are already countenancing the possibilities in question*; this could be prevented by either prejudice or a mere lack of imagination, both of which are going to be widespread if the non-existence of those possibilities has already been naturalized. On the other hand, dropping the consistency assumption simply lets the new possibilities arise on their own, from the mathematics, so to speak. So even if we accept that classical mathematics may in principle recover any given inconsistent interpretation by extending the space of classical possibilities, this does not undermine the need for inconsistent mathematics so construed.

1.9 Conclusion

Let us take stock. Inconsistent mathematics has a fairly uncontroversial logico-mathematical motivation: it is a test of expressiveness for paraconsistent logics, and a tool for better understanding them. When we leave the realm of logic, pure inconsistent mathematics may be justified on grounds of intra-mathematical fruitfulness, but to do so one needs to show that inconsistent reconstructions and nonclassical ways of reasoning actually have something to offer to mathematics. This does not come for free: fancy talk about extending the domain of mathematics, or about explicating the structure of the inconsistent, needs to be backed by evidence which goes far beyond a mere proof of logical nontriviality, especially given the much broader space of possibility for mathematical triviality. Duality considerations do not automatically provide such evidence unless they come with a clear explanation of the value of the duality in question.

True or even apparent contradictions may suggest the use of inconsistent mathematics locally, within the context where such contradictions appear or are likely to appear. Two difficulties are that it seems always possible to offer consistent alternatives, and that faithfulness to a subject matter is hardly an indefeasible value. Justification beyond the mere presence of an inconsistency needs to be provided for why an inconsistent treatment is worth sticking with in any given case.

Proposals for an inconsistent foundation of mathematics have yet to show that they can overtake any of the current foundations when it comes to their respective foundational roles; new roles have to be actively argued for. Classical recapture is still a significant problem for both foundationalism and other projects from the philosophy of mathematics; in its absence, other arguments need to be brought in to explain why a different, incompatible mathematics is worth exploring.

Arguments to the effect that inconsistent mathematics is in some sense already there in standard practice were found to be unconvincing: literal formal descriptions of practice are of dubious utility, there is little reason to assume that any historical or contemporary mainstream practice could not be reconstructed so as to avoid inconsistent commitments, and Gödel's incompleteness theorem cannot be meaningfully applied to informal proof in order to show its inconsistency.

Most alleged defects of classical logic, such as fallacies of irrelevance and the like, might in principle be ignored in the context of classical mathematics insofar as no justification is given for why this would lead to problems when reasoning within classical structures. However, classical logic's dangerous naturalization of dualisms has been shown to carry over to classical maths, so inconsistent mathematics might be justified as a way of counteracting it through the use of inconsistent reinterpretations and less dualistic logics.

Chapter 2

Reasoning with inconsistencies

Classical logic (henceforth, **CL**) does not tolerate contradictions. Classically, from a contradiction, everything follows; that is *Explosion*, the rule $A, \neg A \vdash B$.[1] A contradiction is a sign we made a mistake somewhere. Inconsistency equals triviality. So inconsistent mathematics comes with a quest for what it means to reason in an inconsistent context. Plenty of theories come from the literature on belief revision, but that is not what inconsistent mathematics needs. Here we are countenancing not just the idea that contradictions may turn up in our mathematical reasoning, which is obvious and commonplace: even without subscribing to grand fallibilist philosophies of mathematics, just think of reductio proofs, or simple mistakes! Rather, the idea is that some contradictions might be theorems, or that some nontrivial theories may rest on contradictory assumptions.

A logic is paraconsistent if Explosion fails. Clearly, paraconsistent logics are the prime candidate for underlying inconsistent mathematics. The adoption of a nonclassical logic does not, however, commit inconsistent mathematicians to a rejection of classical mathematics. First, the idea of a one true logic of mathematics is controversial: in fact, an influential position is to take the observable plurality of mathematical practices grounded in different logics to entail logical pluralism.[2] In this sense, the possibility of inconsistent mathematics needs not disturb classical mathematics at all. Second, inconsistent mathematics may be conceived as an *extension* of classical mathematics, i.e. it might recover classical mathematics as a special subcase. This leads to the already discussed problem of *classical recapture*, and its manageability very much depends on the kind of logic we are countenancing.

[1] In this chapter I will follow the convention of saving $\neg$ for explosive negations, while using $\sim$ for nonexplosive negations. Furthermore, I will use $\vdash$ to denote derivability of a formula from a set of formulas.

[2] See [Shapiro, 2014], [Kouri Kissel, 2018], and [Caret, 2021].

Of course, reasoning is not all about logic (at least in the logician's sense). What distinguishes the reasoning strategies of an inconsistent mathematician from that of a standard mathematician? What are the differences in their tactics and goals? To simply say that they follow the rules of an inconsistency-tolerant formal system seems exceedingly reductive, much like an analogous answer would be for classical mathematics. First of all, this stance makes it sound like inconsistent mathematics is a mere switch of formal systems; and most of the projects discussed in the previous chapter do not seem to force (or even suggest) such an ultra-formalist perspective. Furthermore, it is generally accepted that there is a substantial distinction to be made between formal proof and mathematical proof: in fact, some have even argued that the gap can be quite insurmountable.[3] This is partly because (most?) mathematical reasoning may not even be essentially *logical* in nature: for example, reasoning that is visual, diagrammatic, or analogical may be difficult to reduce to logic in a sense which logicians would be happy with.[4] But even regardless of logical status, the point is that an account of informal reasoning does not magically fall out of a given formal system, so if inconsistent mathematics involves new ways of reasoning then something should be said about the informal level as well. To my knowledge this question has not really been addressed explicitly in the literature on inconsistent mathematics, although - as we will see - some of the proposals that have been put forward for adequate formal systems do already go beyond what is usually considered logic.

In this chapter I will discuss some of the logics underlying existing systems of inconsistent mathematics, and some of their most notorious difficulties in capturing mathematical reasoning.[5] It is not my goal to be comprehensive, partly because

[3]See e.g. [Rav, 2007], [Leitgeb, 2009], [Larvor, 2012], and [Tanswell, 2015].

[4]See [De Toffoli and Giardino, 2015] for a striking example from low-dimensional topology. Whether or not such reasoning has as strong a justificatory power as standard logical reasoning is, I believe, not particularly relevant. In practice it is considered good enough to prove mathematical theorems, and that is all that we need here.

[5]In this chapter (and, in fact, in this entire thesis) I will only consider *first-order* logics. Practitioners of abstract model theory have long argued that, since classical first-order logic is clearly inadequate for capturing many fundamental mathematical concepts (e.g. finiteness and completeness), we should open our minds to *extended* logics instead, for example by adding generalized quantifiers, or allowing for infinitary formulas (see [Barwise and Feferman, 2017]). Following this line of thought it seems reasonable to say that, in order to access inconsistent mathematical concepts, we cannot merely dumb down classical first-order logic, but also need to extend it. The fact that the logical diversion happens within the classical fragment is irrelevant; the point is that the resulting logic could be inadequate *regardless* of its stance towards inconsistency, and therefore will not be able to provide sufficient insight into inconsistent mathematics. Furthermore, the logical structure of such extensions could be impacted, making the extension step nontrivial and therefore an interesting matter to discuss even independently of applications. I will set the topic aside simply because I know of virtually no work in this direction, although see [Hazen and Pelletier, 2018] for some (purely logical) discussion of second-order **LP**.

there are simply too many paraconsistent logics to even dream of it, and partly because I believe that any discussion about the "best" or "correct" logic is a massive red herring (in this context, at least) which does nothing but hide the substance and impair the development of inconsistent mathematics. In the last few sections I will argue that this plurality of available logics should be embraced whole-heartedly, and I will suggest a more fruitful way to deal with it than having them fight each other for supremacy. I will largely stick to the strictly logical level in this chapter, while in the next chapter I will move on to the mathematical level.

2.1 LP and friends

LP, the Logic of Paradox, is maybe the paraconsistent logic per excellence. It has a very simple semantical characterization: starting from the usual Tarskian semantics for **CL**, it can be obtained by simply rejecting the functionality of truth-value assignments. This amounts to allowing for *gluts*, i.e. for sentences that are simultaneously both true and false in the same model.[6]

It is often helpful for technical reasons to present **LP** as a 3-valued logic with values t (just True), f (just False), and b (True and False). Both t and b are designated, and the consequence relation is defined as preservation of designated values (which, intuitively, can still be understood as truth-preservation). The truth-tables for the main connectives are as follows:

A	$\sim A$
t	f
b	b
f	t

$A \wedge B$	t	b	f
t	t	b	f
b	b	b	f
f	f	f	f

$A \vee B$	t	b	f
t	t	t	t
b	t	b	b
f	t	b	f

$A \supset B$	t	b	f
t	t	b	f
b	t	b	b
f	t	t	t

$A \equiv B$	t	b	f
t	t	b	f
b	b	b	b
f	f	b	t

If we impose the order f $<$ b $<$ t on the set of truth-values, we can see that $\wedge$ and $\vee$ are just lattice operators;[7] quantifiers $\forall$ and $\exists$ can then be interpreted as infinitary $\wedge$ and $\vee$ respectively.

An interesting feature of **LP** is that it shares its set of tautologies with **CL**, so we can only distinguish it by looking at valid inference rules. This exemplifies the

[6] A syntactical characterization can be found e.g. in [Priest, 2002].

[7] This means that $x \wedge y := \max\{z : z \leq x, y\}$ and $x \vee y := \min\{z : z \geq x, y\}$.

fact that validating the classical principle of non-contradiction $\sim(A\wedge\sim A)$ is not enough to prevent a logic from tolerating inconsistencies; in the case of **LP**, all it does is ensure that every contradiction is false, without excluding the possibility that some of them may also be true. The Explosion rule $A,\sim A\vdash B$ fails, making **LP** a paraconsistent$^{\mathrm{tm}}$ logic; but many other classical inference rules are preserved. In fact, **LP** is what [Ferguson, 2012] calls a *De Morgan logic*, meaning that it validates all the De Morgan laws; it also validates double negation introduction and elimination.[8]

LP is very generous in terms of models. In fact, *every* set of sentences has at least one **LP**-model: just make every atomic formula both true and false! However (one might say: therefore), when it comes to deductive power, **LP** is inadequate for even the simplest reasoning, let alone mathematical reasoning. Formally, the main problem is that no logical premise can validate the use of modus ponens: the truth of A and $A\supset B$ does not exclude the non-truth of B, because A and $A\supset B$ may also be false; and the language contains no way to express that a formula is just true. For the same reason Disjunctive Syllogism fails, i.e. it is not the case that $(A\vee B),\sim A\vdash B$. The upshot is that, while **LP** provides a very general logical universe where inconsistent mathematics might live, it (by itself) gives almost no information on what reasoning within inconsistent mathematics might look like.[9]

Related to this is **LP**'s lack of expressive power. Classical first-order languages are famously unable to fix an interpretation (up to isomorphism) for many important mathematical theories: in fact, this is the case for every theory with infinite models, since by the Löwenheim-Skolem theorems we can then always find a model of different size.[10] In **LP**, however, this phenomenon is extended to *all* theories, and the nonstandardness becomes much more serious: every theory has finite models,[11] and there are axiomatic theories with nontrivial models making all of their axioms *false* (and also true). The main issue is, again, that theories cannot constrain any sentence to be just true, yet very little can be deduced from sentences which may be both true and false. Furthermore, unlike in the classical case, moving to second-order languages does not help one bit.

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[8]Formally:

- $\sim(A\wedge B)\dashv\vdash\sim A\vee\sim B$
- $\sim(A\vee B)\dashv\vdash\sim A\wedge\sim B$
- $A\dashv\vdash\sim\sim A$

It is not uncommon for nonclassical logics to reject some of these laws: for example, intuitionistic logic rejects $\sim\sim A\vdash A$ and $\sim(A\wedge B)\vdash\sim A\vee\sim B$.

[9]*"A willingness to welcome all worlds builds none"* [Goodman, 1978, p.21].

[10]See any introduction to classical model theory, e.g. [Hodges, 1993].

[11]By the Collapsing Lemma, to be discussed in Section 3.1.

Now, what we can do is classify the **LP**-models of a theory from the outside (albeit the classification would not be expressible in **LP**), and then start thinking about particular models. Precisely because **LP** is so weak, most **LP**-models will have room for a stronger underlying logical structure than what can be expressed in **LP**. However, **CL** will be unavailable to capture said structure because of Explosion (unless the model was classical to begin with), so what we need is an intermediate logic, sitting somewhere between **LP** and **CL**. We can then see **LP** as a starting point from which to discover these new intermediate logics, in the same way classical logic provides the bedrock on which all extended logics underlying classical mathematical structures are built on.[12]

Some of these intermediate logics are obtained by replacing **LP**'s material conditional with a nonmaterial one supporting modus ponens. It is important to note that such a "replacement" is really an addition and a change in perspective, since the material conditional remains definable as $A \supset B := \sim A \vee B$. Of course we then need to actually use the new conditional in axioms and definitions, otherwise nothing much has changed.[13] One problem with this strategy is that, because **LP** cannot validate the Contraposition meta-rule on pains of collapse into **CL**, it is impossible to have a conditional satisfying modus ponens while preserving both the Contraposition rule and the Deduction Theorem.[14] Thus, some sacrifices have to be made. Here I will focus on two examples that have been particularly popular in the literature in inconsistent mathematics.

The logic **RM3** extends **LP** with the following conditional (and biconditional):

$A \to B$	t	b	f
t	t	f	f
b	t	b	f
f	t	t	t

$A \equiv B$	t	b	f
t	t	f	f
b	f	b	f
f	f	f	t

As can be seen from the truth tables, this conditional is contraposable and satisfies modus ponens; however, the Deduction Theorem fails because implications with just true antecedents and true-and-false consequents are false. Note that the associated biconditional indicates equivalence of truth values.[15]

[12] One could also look at *contraclassical* expansions of **LP**. We will see an example in the next section.

[13] At least if the expansion is conservative, i.e. does not lead to new theorems in the original fragment.

[14] The Deduction Theorem says that, if $A \vdash B$, then $\vdash A \to B$. Contraposition is the rule $A \to B \vdash \sim B \to \sim A$. Modus ponens ensures that, if $\vdash \sim B \to \sim A$, then $\sim B \vdash \sim A$. These properties taken together imply the Contraposition meta-rule: if $A \vdash B$, then $\sim B \vdash \sim A$. Since in **LP** everything entails a theorem, adding the Contraposition meta-rule collapses t and b, and so takes us back to **CL**.

[15] For more about **RM3**, see [Anderson and Belnap, 1975, pp.470-471].

Let us see how **RM3** improves on **LP** in a toy example. Consider the axioms of a preorder:

1. $\forall x\, x \leq x$
2. $\forall x, y, z(x \leq y \wedge y \leq z \rightarrow x \leq z)$

If $\rightarrow$ was the material conditional, this would be pretty much where the content of the theory ends, save for a bunch of iterated conjunctions and De Morgan / double negation reformulations: saying that the axioms (or any extra assumptions) hold does not prevent them from being contradictory, in which case modus pones becomes unusable. For example, we do not get to conclude $0 \leq 2$ from $0 \leq 1 \wedge 1 \leq 2$, since (2) is compatible with $0 \leq 1 \wedge 1 \leq 2 \rightarrow 0 \leq 2$ being both true and false.[16] We could, of course, simply stipulate that some of the axioms are just true, which lets us derive a couple things more: this is the *shrieking* strategy suggested in [Beall, 2013b].[17] However, the fact remains that no significant conclusion can be drawn from potentially contradictory axioms, thus making the inconsistent part of the theory essentially inert. Besides, if we are willing to stipulate consistency, it might be more fruitful to expand the object language so that we can express this stipulation: that way we could actually study the interaction between classical and nonclassical fragments of the theory. This is what the logics in the next section try to do.

Meanwhile, if the conditional in these axioms is the **RM3** one, we can pretty much follow the classical reasoning to the letter: even if $0 \leq 1 \wedge 1 \leq 2$ is a potential contradiction, we can still use (2) to conclude from it that $0 \leq 2$ is (at least) true. Low bar, but still. On the negative side, failure of the Deduction Theorem means that not every valid inference will be expressible as a theorem. For example, it is not the case that $a \leq a \rightarrow b \leq b$ is a theorem.[18] This appears to require a significant revolution in the language of mathematics: many theorems, which are normally stated as implications, would have to be reformulated as inference rules.

This is a good time to note that, if we were willing to reformulate the theory *axioms* as inference rules, then we could have gone further with **LP** as well. After all, it is obviously the case that, if A follows from a theory T and "B follows

[16] This is not to say there could be no reason to look at nonclassical preorders where some particular classical property does not hold. The problem here is that basically *no* property is fixed by the axioms beyond their statements.

[17] Actually, Beall suggests shrieking *predicates*: this means postulating that, if there is anything satisfying both P and $\neg P$, then triviality follows. If all (primitive) predicates of a subclassical theory are shrieked, then the theory is either consistent or trivial.

[18] Take a model $\{a, b\}$ where $a \leq a$ and $a \leq b$ are just true, while $b \leq b$ and $b \leq a$ are true and false.

from A" is an axiom of T, then B follows from T.[19] The problem with such a move - aside from the fact that it cannot be expressed in the object language - is that it often ends up being *too* effective, undermining the advantages that a lack of expressiveness can bring in avoiding triviality. The counterpoint, best argued for by [Restall, 2013], is that excessive reliance on this lack of expressiveness is a bit of a "cheat": the motivation behind certain axioms usually involves their inferential meaning, so throwing too much of that meaning away appears to undermine the whole project. Have we really axiomatized preorders if we cannot get a single application of transitivity to go through?

The logic **A3**, sometimes also called **LP**$^{\supset}$, takes a different approach. Its conditional is defined as follows:

$$x \to y = \begin{cases} y & x \in \{\mathsf{t}, \mathsf{b}\} \\ \mathsf{t} & \text{otherwise.} \end{cases}$$

This generates the following truth-tables:

$A \to B$	t	b	f
t	t	b	f
b	t	b	f
f	t	t	t

$A \equiv B$	t	b	f
t	t	b	f
b	b	b	f
f	f	f	t

This conditional satisfies both modus ponens and the Deduction Theorem.[20] The biconditional no longer expresses equivalence of truth value, but it does express having the same designated status. What might raise an eyebrow is that this conditional is not contraposable, further exacerbating the already worrisome lack of a Contraposition meta-rule: after all, proof by contraposition is quite omnipresent in mathematics! One way to deal with this might be to rely on the fact that **A3** and **RM3** are interdefinable in order to use *both* conditionals, depending on context.[21] In particular, one might use the **RM3** conditional to express those axioms whose application is required in both direct and contraposed form, and stick to **A3** for all the others so as to maximize the amount of theorems. One example of this is the **A3** axiomatization of finite cyclic models in [Tedder, 2015]: one of the axioms states precisely that the (bi)conditional in the axiom $x = y \leftrightarrow x' = y'$ can be read as the **RM3** one, while the others remain non-contraposable.

On the model-theoretic side, the field is still too underdeveloped to say much about the kinds of structures that these logics can support. **RM3**-models of various theories are presented in [Mortensen, 1995], but the constructions largely consist

[19] This has obviously been disputed, as we will see in a few sections.

[20] See [Asenjo and Tamburino, 1975].

[21] $A \to_{RM3} B = (A \to_{A3} B) \wedge ({\sim}B \to_{A3} {\sim}A)$, while $A \to_{A3} B = (A \to_{RM3} B) \vee B$.

in taking a classical model and uniformly turning the true into contradictory across the board. This approach is certainly helpful to get nontriviality results, but not so much to gain any actual insight into the mathematical possibilities offered by the logic. More outlandish models have been found and will be discussed in Ch.3, but what would be really needed is a more *systematic* model theory, going beyond piecemeal modelization and rather focusing on classification and comparison of models and theories.[22]

Before moving on to different kinds of logics, a word about *gaps*, i.e. formulas to which no truth value is assigned. Dually to **LP**, Kleene's strong logic **K3** is obtained by taking classical logic and allowing for gaps (instead of gluts). The acceptance of gaps is usually connected with rejecting the Law of Excluded Middle (LEM): $A \vee \neg A$. **K3** is not paraconsistent, so I am not going to say much about it, except for the fact that there are ways to exploit the duality between gaps and gluts to transfer technical results between **K3** and **LP**: for example, if we look at the three-valued semantics, the only difference between the two logics is whether the third value is designated or not.

FDE is the logic obtained by allowing for both gaps and gluts. Being even weaker than **LP** (and **K3**), **FDE** appears to inherit all of the aforementioned difficulties in capturing mathematical reasoning. However, it should be noted that **FDE** can be extended in ways incompatible with **LP**, so starting from there can open new possibilities: for example, **N4** - a well-studied paraconsistent extension of **FDE** by an intuitionistic conditional[23] - could be used by inconsistent mathematicians with constructivist sympathies, and in fact some inconsistent models were already presented in [Nelson, 1959]. The option can also be attractive to someone who puts gaps and gluts on a similar level, given the apparent symmetry. Still, to avoid muddying the waters I will focus mostly on logics adopting LEM. We can worry about the interaction between constructive mathematics and inconsistent mathematics when the latter has been properly established.[24]

[22][Ferguson, 2012] is a first step in this direction, providing - among other things - a suitable definition of ultraproduct and showing that **LP**-style collapses commute with ultraproducts.

[23]Or, more simply, the logic obtained from **RM3** by dropping LEM.

[24]It should be said however that gaps might end up being quite invaluable for the formulation of certain inconsistent theories. For example, here is [Mortensen, 1995] on the possibility of an inconsistent infinitesimal calculus: *"the only way to establish validity of the paraconsistent point of view is to demonstrate the existence of a rich and interesting inconsistent mathematics. [...] Combining inconsistency with incompleteness would seem to be the right way to go here"* (pp.11-12).

2.2 Logics of formal inconsistency

The logics in the previous section were lacking in expressive power: while they have a lot of models, they have very few means to actually pin them down, which in turn can severely limit what we can deduce from a theory. This *could* be blamed on the inability of the object language to actually express that a formula behaves consistently; in which case, the obvious solution is to introduce an apparatus that does just that.

A *consistency operator*, usually denoted by $\circ$, is a unary operator which, when applied to any formula, henceforth validates Explosion for that formula: formally $\circ A, A, \sim A \vdash B$. A *logic of formal inconsistency* or *LFI* is a paraconsistent logic with a nontrivial consistency operator.[25] These logics are usually significantly more expressive than **LP** and its friends, which are unable to express anything resembling a Boolean negation. LFIs are also much easier to sell as *extensions* of classical logic, since a safe domain for classical reasoning can often be singled out using the consistency operator. So they may appear to be a much better bet for an expansionary conception of inconsistent mathematics.

It is not hard to get an LFI based on **LP**. Simply add a consistency operator with the following truth-table:

A	$\circ A$
t	t
b	f
f	t

The resulting logic, called **LFI1**, is usually taken to have as a main conditional the **A3** one, which is definable via the consistency operator: just let $A \to B := (\sim A \wedge \circ A) \vee B$.[26] We can also define a classical negation $\neg A := \sim A \wedge \circ A$. Such connectives allow for a great deal of classical reasoning, as long as the premises are stated to be consistent. However, to make all premises consistent would be somewhat moot: might as well not have introduced the possibility of inconsistency at all! Rather, the way towards inconsistent mathematics might be to use the greater expressiveness in order to add additional axioms governing the relationship between consistency and inconsistency in a

[25] Nontriviality means that $\circ A, A \vdash B$ and $\circ A, \sim A \vdash B$ should fail for some A, B. The main reference on LFIs is [Carnielli and Coniglio, 2016].

[26] **LFI1** is definitionally equivalent to the logic **CLuNs**, which takes as primitive the falsum constant $\bot$ and the **A3** conditional instead of $\circ$. There we have $\circ A := A \wedge \sim A \to \bot$.

given structure: hence complex - and new - mathematical theories might arise.[27]

We can go even further. One way to cash in expressiveness is *functional completeness*, i.e. the ability to define every possible truth-functional logical connective from within the logic. None of the paraconsistent logics discussed until now have this property, despite the fact that classical logic does. This can lead to situations where the only way we have to express certain logical operations is through the help of some external classical machinery. The connexive logic **dLP**, discussed in [Omori, 2016], is a functionally complete extension of **LFI1** by the following connective:[28]

$$x \to y = \begin{cases} y & x \in \{\mathsf{t}, \mathsf{b}\} \\ \mathsf{b} & \text{otherwise.} \end{cases}$$

This generates the following truth-tables:

$A \to B$	t	b	f
t	t	b	f
b	t	b	f
f	b	b	b

$A \equiv B$	t	b	f
t	t	b	f
b	b	b	f
f	f	f	b

dLP is an *inconsistent logic*, meaning that it takes some formula and its negation to both be theorems: this is the case e.g. for $(A \wedge \sim A) \to \sim A$ and $\sim((A \wedge \sim A) \to \sim A)$. Since the latter is the negation of a classical theorem, this is also our first example of a *contraclassical* logic that has been proposed for inconsistent mathematics. If this sounds excessively radical, it is worth pointing out that this kind of contraclassicality needs not significantly contradict any classical mathematics. For example, [Ferguson, 2019a] shows that arithmetic based on the contraclassical connexive logic **C** has similar models as **N4**-based arithmetic, which is subclassical: the only differences concern the falsity (but not the truth!) of some implications.[29] Similarly, the presence of purely logical contradictions is, at least in principle, quite irrelevant in the economy of any mathematical theory; after

[27] Going in a different direction, one of the original motivations for LFIs was to protect us from the possibility that our current theories might turn out to be inconsistent. The idea is that, with the help of LFIs, we may not have to worry about having to throw the whole formalization away just because of some isolated contradiction in a corner. In principle such a project needs no inconsistent mathematics at all, except possibly during transition periods where any unexpected inconsistencies would have to be analysed in order to figure out how to best get rid of them.

[28] A logic is said to be *connexive* if it satisfies the so-called Aristotle and Boethius theses. For a general overview of connexive logics, i.e. logics with a connexive conditional, see [Francez, 2021]. The idea of a connexive mathematics is not completely new: for example, [McCall, 1967] proposed a connexive class theory.

[29] See Section 3.3.

all, precisely because they are purely logical, they are not specific to any theory and so impact is not guaranteed. To further support this reading, there is a sense in which the (at least countable) model theory of **dLP** is equivalent to classical model theory: for any theory, its space of countable relational models ends up having the same topological structure as that of classical logic.[30]

[Carnielli and Coniglio, 2016] single out the propositional logic **mbC** as the "basic" LFI: this is obtained by adding LEM and the axiom $\circ A \to (A \to ({\sim} A \to B))$ to the positive fragment of propositional **CL**. Immediately we can define a falsum constant $\bot := A \wedge ({\sim} A \wedge \circ A)$ and a classical negation $\neg A := A \to \bot$, which behave in more or less the expected way. This logic is basic in the sense that most LFIs are obtained as axiomatic expansions of it, not in the sense that it is especially pleasant on its own: most notably, while we can provide a two-valued matrix semantics, neither $\sim$nor $\circ$ come out as deterministic.[31]

There has been a fair amount of work on a nondeterministic Tarskian model theory for **QmbC**, i.e. first-order **mbC**:[32] see e.g. [Carnielli et al., 2014], [Mendonça and Carnielli, 2018], and [Ferguson, 2020]. The advantage of such an approach is that it can allow for recovery of classical model-theoretic results by (roughly) applying those same results to the deterministic fragment and checking that the outcome still does the job. Again, the focus on **QmbC** comes from the hope that any results may be extended to many of its axiomatic expansions. Of course, even if such an attempt at model theory were to fail, this needs not prevent **QmbC** from being a useful logic for mathematics. First of all, other semantics have been discussed by [Coniglio et al., 2020], and they might turn out to be more fruitful. And besides, LFIs are much more suited for deductive work than **LP** and friends, so the need for a systematic model theory is much less pressing.

A final note. Much like **LP** and friends, LFIs also have duals under the name of LFUs, logics of formal undeterminedness.[33] Analogues of **FDE**, able to express both consistency and determinacy, can be found e.g. in [Carnielli and Rodrigues, 2019] and [Omori, 2020]. I will only mention here

[30] This is also the case for **LFI1**, a fact which may be taken to suggest that functional completeness is overkill. In both cases the result is a consequence of the following uniqueness theorem: any two nonempty zero-dimensional compact metrizable spaces with no isolated points are homeomorphic (see [Kechris, 2012, Thm 7.4]). All of these properties have an immediate logical analogue in the space of relational structures: in particular, zero-dimensionality follows from the definability of Boolean negation.

[31] To be more precise: ${\sim} A$ is always true if A is false, but may be either true or false if A is true; $\circ A$ is always false if both A and ${\sim} A$ are true, but may be either true or false in all other cases. One way to make at least $\circ$ deterministic is to add the axiom $\circ A \vee (A \wedge {\sim} A)$, thus obtaining the logic **mbCciw**. For more on non-deterministic semantics, see [Avron and Zamansky, 2010].

[32] The **Q** stands for "quantified".

[33] See [Marcos, 2005].

the logic **BS4**, which is obtained by extending **FDE** with a falsum constant $\bot$ and the **A3** conditional. A consistency operator can then be defined as $\circ A := A \wedge \sim A \to \bot$. This logic has been put to mathematical work by [Oddsson, 2021]: I will come back to this in Ch.3.

2.3 Relevant logics

The history of relevant logics has much to do with inconsistent mathematics, despite the fact that the management of inconsistencies was not one of the original motivations. The main idea from [Anderson and Belnap, 1975] is that the conditional of a logic, insofar as it expresses logical entailment or implication, should be such that antecedent and consequent are always *relevant* to each other: if B is irrelevant to A, then $A \to B$ should not be valid, regardless of the logical form and truth-status of A and B.

At the propositional level, one way to enforce relevance is by demanding that antecedent and consequent have at least one propositional variable in common: this is (a version of) the so-called *variable sharing property*. A truth-functional conditional will usually flunk this requirement: most notably, the classical material conditional delivers valid oddities like $A \supset (B \vee \neg B)$. This is not a quirk of classical logic specifically: for example, since **LP** and **CL** have the same tautologies, **LP**'s conditional is just as irrelevant. These are sometimes called the "paradoxes" of material implication, and the main goal of relevant logics is to get rid of them. In particular, it seems that $A \wedge \sim A \to B$ should be rejected: this usually comes with a rejection of Explosion, and thus we get paraconsistency.[34]

A relevant conditional tends to express a very strong connection, as opposed to e.g. the **LP** conditional, which is a paradigm of weakness.[35] If the language contains something like a falsum constant $\bot$, then something closer to a classical negation may be defined as $\neg A := A \to \bot$. In fact, even without $\bot$ in the language, we may still find some appropriate formula to play the role depending on the theory under study: for example, in relevant arithmetics it is commonplace to take $0 = 1$ as $\bot$. So relevant logics can be fairly expressive.[36] In particular, this

[34]This does not mean that every logic that has been called relevant is paraconsistent: a counterexample would be Ackermann's $\mathbf{\Pi}'$ [Anderson et al., 2017, ch.8].

[35]On this note, adding relevant-like conditionals to **LP** is one strategy to fix its lacking expressive power. A minimal example of this is the logic **BX** discussed in [Priest, 2002], which is obtained by adding LEM to the weak relevant logic **B**.

[36]Absurdity does not need to coincide with logical triviality for this strategy to make sense: it suffices to pick a formula that unquestionably leads to "bad" models. For example, [Slaney, 2022] shows that even in strong relevant arithmetics there are nontrivial models of $0 = 1$, but in all such models *every* equation holds.

can be used to fix the consistency of a sentence: simply postulate $A \wedge \sim A \rightarrow \bot$. Making an axiom true on pains of absurdity is an object language example of shrieking, and it can be used to exclude unwanted possibilities.

Probably the most famous - and one of the strongest - relevant logic is **R**. Relevant logics are usually easier to present axiomatically, so here we go. Axioms for **RQ** (i.e. **R** with quantifiers), with $A \vee B$ defined as $\sim(\sim A \wedge \sim B)$:

$\rightarrow$1. $A \rightarrow A$

$\rightarrow$2. $(A \rightarrow B) \rightarrow ((B \rightarrow C) \rightarrow (A \rightarrow C))$

$\rightarrow$3. $A \rightarrow ((A \rightarrow B) \rightarrow B)$

$\rightarrow$4. $(A \rightarrow (A \rightarrow B)) \rightarrow (A \rightarrow B)$

$\wedge$1. $A \wedge B \rightarrow A$

$\wedge$2. $A \wedge B \rightarrow B$

$\wedge$3. $(A \rightarrow B) \wedge (A \rightarrow C) \rightarrow (A \rightarrow (B \wedge C))$

$\wedge\vee$. $A \wedge (B \vee C) \rightarrow (A \wedge B) \vee C$

$\sim$1. $(A \rightarrow \sim B) \rightarrow (B \rightarrow \sim A)$

$\sim$2. $\sim\sim A \rightarrow A$

$\forall$1. $\forall x A \rightarrow A^x_t$[37]

$\forall$2. $\forall x(A \vee B) \rightarrow (A \vee \forall x B)$, where x is not free in A[38]

Rules (where $\Gamma \Rrightarrow B$ means that, if everything in Γ is valid, so is B):

1. $A, A \rightarrow B \Rrightarrow B$
2. $A, B \Rrightarrow A \wedge B$
3. $A \rightarrow B \Rrightarrow A \rightarrow \forall x B$, where x is not free in A

There can be a lot of variability in the adequacy of different relevant logics to mathematical reasoning, so let me just go through a few common themes with **R** in mind. First of all, it should be noted that relevance comes with some apparent sacrifices. For example, consider the Weakening axiom $A \rightarrow (B \rightarrow A)$.[39] Not

[37] A^x_y denotes the formula obtained from A by replacing every occurence of x with y.
[38] This axiom is dropped in the weaker first-order relevant logic **QR**.
[39] This is also called Thinning in some literature.

only does it fail the variable sharing property; if we add it to **R**, we immediately get classical logic back. However, in **R** it is the case that $A \vdash B$ *just means* $\vdash A \to B$. So rejecting Weakening leaves us unable to subsume trivial instances of a consequent under said consequent. For example, when classically proving a theorem of the form $B \to \forall n A(n)$ it often happens that B and $A(0)$ have nothing to do with each other, and $A(0)$ just happens to be obviously true; but then without Weakening it seems like we cannot get the induction started.

More worryingly, Weakening follows by the Deduction Theorem: if $\Gamma, A \vdash B$ then $\Gamma \vdash A \to B$, and monotonicity: if $\Gamma \vdash B$ then $\Gamma, A \vdash B$. To many logicians - and mathematicians - this generates a difficult choice. One way to get around the issue, suggested by [Weber, 2021a], is by having *two* primitive conditionals: one takes care of relevance matters, and the other satisfies Weakening and the Deduction Theorem.[40] But the most common relevantist approach, found e.g. in [Meyer, 2021a], is to accept the apparent loss of the Deduction Theorem, but argue that it can in fact be recovered in the following form: if $A \circ B \vdash C$, then $A \vdash B \to C$. Here, $\circ$ is a conjunction distinct from $\wedge$ and called *fusion*: in **R** it can be defined as $A \circ B := \sim(A \to \sim B)$.[41] While it is the case that $A \wedge B \to A \circ B$, the opposite does not hold; the intuitive idea is that $\circ$ represents a tighter way to group premises, thus requiring that each of them be (relevantly) used in proving anything from the bunch. The downside of fusion is that monotonicity is lost: it is not the case that if $A \vdash B$ then $A \circ C \vdash B$. Since monotonicity and the Deduction Theorem hold with respect to different ways to group premises, Weakening does not follow.

One of the biggest obstacles in relevantly capturing standard mathematical reasoning concerns the so-called problem of restricted quantification, as discussed in [Beall et al., 2006]. Classically, we express that all As are Bs by the sentence $\forall x(Ax \to Bx)$. This is however problematic in most relevant logics. If $\to$ here is the material conditional, then to conclude Bx we would need (a generalization of) Disjunctive Syllogism, which must be invalid on pains of getting Explosion back. If $\to$ is a relevant conditional, we come to yet more trouble as for example we do not get to conclude from the fact that all numbers have a successor that all even numbers have a successor. Furthermore, even if the logic validates LEM, it is not the case that either all As are Bs or some A is not B. Being unable to capture such sentences is quite a problem, since the vast majority of mathematical theorems is expected to have this form. One proposed solution is to add an enthymematic conditional specifically for the purpose of restricted quantification. This can be done via the use of some constant t standing in for the conjunction of all true

[40] See next section for an example.

[41] Note that in classical logic this defines $A \wedge B$.

theorems: t is introduced via the rule $A \dashv\vdash t \to A$, and the conditional is defined as $A \mapsto_1 B := A \wedge t \to B$.[42] Another solution, defended in [Beall, 2011], is to simply define a new conditional as $A \mapsto_2 B := (A \to B) \vee B$, although the effectiveness of this proposal very much depends on the logic in question.

It is quite easy to build particular models for **R** by using the fact that **RM3** is an extension of **R**, and so **RM3**-models are **R**-models.[43] However, complete formal semantics for **R** (let alone **RQ**) can get quite complicated and obscure. I want to however sketch the main idea, since it will turn out to be quite crucial to my understanding of relevant mathematics as inconsistent. The central notion is that of a *Routley-Meyer frame*, which generalizes Kripke frames for intuitionistic and modal logics by having a *ternary*, rather than binary, accessibility relation.[44] A Routley-Meyer frame is a 5-tuple $\langle W, N, R, *, \sqsubseteq \rangle$ such that:

- W is a set of worlds;[45]
- $N \subseteq W$ is a nonempty subset of designated *normal* worlds;
- $\sqsubseteq$ is a partial order on W such that $u \sqsubseteq v$ if and only if there is $g \in N$ such that $Rguv$;
- if $u \sqsubseteq v$ and $u \in N$, then $v \in N$;
- if $Rwuv$, $w' \sqsubseteq w$, $u' \sqsubseteq u$, and $v \sqsubseteq v'$, then $Ru'v'w'$;
- $w^{**} = w$, and if $u \sqsubseteq v$ then $v^* \sqsubseteq u^*$.

A *Routley-Meyer model* is a Routley-Meyer frame with an evalution $\models$ subject to the following constraints:

- If $u \sqsubseteq v$ and $u \models p$, then $v \models p$ for every atomic p;
- $w \models A \wedge B$ iff $w \models A$ and $w \models B$;
- $w \models A \vee B$ iff $w \models A$ or $w \models B$;
- $w \models \sim A$ iff $w^* \models A$;
- $w \models A \to B$ iff, for every u, v such that $Rwuv$, if $u \models A$ then $v \models B$.

[42] See [Weber, 2010a] for a discussion of this solution in the context of naive set theory.

[43] Adding the Mingle axiom $A \to (A \to A)$ to **R**, we get the (semi-relevant) logic **RM**. Adding $A \vee (A \to B)$ to **RM** we get **RM3**.

[44] See [Sylvan et al., 1982, ch.4] for more details on what follows. For extended discussion of the semantics of **R** specifically, see [Mares, 2004], which also says a bit about quantification. If you *really* want to know more than a bit, see [Logan, 2019].

[45] This is a metaphor. The word "situations" is sometimes used as well.

Theoremhood is then defined as truth at all normal worlds in all models, while logical consequence is defined as truth-preservation across all worlds. Routley-Meyer frames are sound and complete w.r.t the basic relevant logic **B**; stronger logics can be obtained by adding conditions on the accessibility relation R.[46] For our purposes, the important thing to remember here is that there is no need for arbitrary worlds in W to share truths with the normal worlds: in particular, *theorems* can fail at arbitrary worlds. This is, for example, how these logics can allow for incomplete situations despite often satisfying the LEM.[47]

A systematic model theory for relevant logics is not hopeless: see [Badia, 2017]. Some preliminary results suggest, however, that classifying theories would be *significantly* more complex. An example of this is the status of quantifier elimination,[48] which is shown by [Badia and Tedder, 2018] to fail in **RQ** (and many weaker logics) for most paradigmatic mathematical theories. To get it back, one needs the Weakening axiom; but we already saw that this is usually a no-go.[49] Quantifier elimination is one of the main tools of classical model theory: it is invaluable to prove completeness of theories, and to classify definable relations and elementary extensions. This is not a purely formal matter: the upshot of quantifier elimination is a *feasible* description of what properties certain structures have, and how they relate to each other. Without quantifier elimination, there is no guarantee that such a description can be achieved. I am sympathetic to the optimistic reading of the situation: *"a logical approach maintaining relevance opens a view which, as Meyer and Mortensen claimed, "cannot impoverish insight into the nature of mathematical structures, but rather can only enrich it""* (p.153). However, this needs to be backed up by the rise of tools which can actually deal with this added complexity, and still produce general enough results. A loss of generality is only really an enrichment if new generalities can be created from its ashes.

There are many reasons why one might want to focus on logics much weaker than **R**. One motivation is to enforce stronger relevance conditions. For example, *depth-relevant* logics like [Brady, 2006]'s **DJQ** demand that the shared

[46]This is the "general" Routley-Meyer semantics. The *reduced* semantics takes N to be a singleton, and was shown to be sufficient for plenty of relevant logics in [Slaney, 1987]. The *simplified* semantics, which drops the starting conditions on R in exchange for a more convoluted satisfaction condition for the conditional, is discussed in [Priest and Sylvan, 1992] and [Restall, 1993].

[47]This kind of phenomenon is reflected in the existence of so-called *non-regular* theories which do not contain all theorems of the logic. Theory closure is here understood as closure under adjunction and valid implications, rather than closure under logical consequence: see [Anderson and Belnap, 1975, §25.2.1].

[48]A theory T is said to have quantifier elimination if every formula is provably equivalent (in T) to a quantifier-free one.

[49]For what is worth, Weakening can be added without classical collapse to **B**.

propositional variable be at the same level of conditional nesting: this excludes e.g. $A \wedge (A \rightarrow B) \rightarrow B$. Even more radically, we could ask for the consequent to not contain any more propositional variables than the antecedent: this would rule out $A \rightarrow A \vee B$. But, as [Burgess, 2009, ch.5] points out, this is used all the time in mathematics for the sake of subsumption under a generalization: for example, in order to prove by induction $\forall n(A(n) \vee B(n))$ we usually require a proof of either $A(0)$ or $B(0)$. On the other hand, if we asked for the consequent to not contain *fewer* propositional variables than the antecedent, we would miss out on $A \wedge B \rightarrow A$. Most relevantists would not dare go this far.[50]

[Plumwood, 1967] provides a different line of argument against **R**. The idea is that, insofar as $A \rightarrow B$ expresses the fact that A is *sufficient* for B, premises should never be *suppressed*: suppression occurs when an implication *"allows the omission in a certain premiss set for a certain conclusion of some proposition which is in fact needed to make the argument from these premisses to that conclusion valid, i.e. to make the premiss set given sufficient for that conclusion"*. This idea is violated by **R** through its validation of Exported Syllogism: $(A \rightarrow B) \rightarrow ((B \rightarrow C) \rightarrow (A \rightarrow C))$.[51] This lets us prove $(B \rightarrow B) \rightarrow (A \rightarrow B)$ whenever $A \rightarrow B$ is true; but clearly the fact that B entails itself has nothing to do with whether it is entailed by A. The contribution of $A \rightarrow B$ is required, yet can be suppressed. [Plumwood, 1993b] goes even further in arguing against suppression by linking it to the dualistic feature of *"backgrounding, in which the contribution of the other to the outcome is relied upon but denied or ignored"* (p.455). If we take seriously the argument from liberation from Section 1.8, this strongly counts against **R** (or any logic from the previous sections, for that matter) being ideal for inconsistent mathematics.[52]

There are also more technical reasons why one might want to consider weaker relevant logics. For one thing, **R** and its close neighbors were famously proven undecidable by [Urquhart, 1984], which some logicians take to be quite untoward for a propositional logic.[53] Furthermore, there has been a whole industry of attempts to develop a naive set theory with a relevant conditional; however, **R** is far too strong for this kind of job, as we will see in Section 3.1. One of the logics

[50] This is the *connexivist* strategy, discussed and rejected in [Sylvan et al., 1982, Sect 2.4].

[51] This should be distinguished from Conjunctive Syllogism, which is not taken to involve suppression: $(A \rightarrow B) \wedge (B \rightarrow C) \rightarrow (A \rightarrow C)$.

[52] While the informal point is clear enough, there are some difficulties in formalizing suppression-freedom in a way that actually does what it is supposed to: see especially [Øgaard, 2020], which argues against Exported Syllogism being suppressive in any significant way.

[53] A logic (and, more generally, a theory) is said to be decidable if its set of theorems is, i.e. if there is an algorithm that can tell us, for every formula in the language, if it is a theorem of the logic or not. Classical propositional logic is decidable, using truth tables; classical first-order logic is not, and a proof can be found in [Turing, 1936].

of choice used to be **DKQ**, i.e. **DJQ** + LEM: the resulting set theory was proven nontrivial by [Brady, 1989]. I spare the reader a quite painful axiomatization.[54]

2.4 Substructural logics

There is a sense in which many of the logics discussed until now are not that radical after all. They may come with different axioms or rules, but the underlying structure of deduction is not really changed from classical logic. But what if inconsistencies required more than a mere change of axioms?

A logic is said to be *substructural* if it gives up on some Tarskian features of the consequence relation; or, in proof-theoretic terms, if it rejects some structural rule of sequent calculus.[55] Structural properties are often implicitly assumed by the logical framework, rather than being part of a particular choice of principles; in this sense, they can appear to be constitutive of logical reasoning. One could wonder if mathematics would even be intelligible without some of them. We already saw that there is a sense in which **R** is non-monotonic, namely with respect to premises grouped with fusion (rather than extensional conjunction); monotonicity is a structural property, so this makes **R** substructural in a sense. Here I am going to discuss two more suggestions that have repeatedly made their appearance in the literature on inconsistent mathematics: dropping transitivity, and dropping contraction.

Let us start with transitivity. A common formulation is this: if $\Gamma \vdash A$ for every $A \in \Delta$ and $\Delta \vdash C$, then $\Gamma, \Delta \vdash C$.[56] It is easy to imagine this as utterly indispensable. Prima facie, a lack of transitivity threatens to bring down the whole edifice of mathematics: how can we prove anything, if we can never join two steps together? But this is a misrepresentation of the proposals in question. What the nontransitive approaches have in common is that transitivity is *usually* fine, which is why in classical mathematics we never have to worry about it; but it is not fine in some very special situations, for example those involving inconsistencies. The proposal is to drop *unrestricted* transitivity; but this is replaced by some sort of *restricted* transitivity, coming with a story about where and why exactly transitivity should fail.

[54] One can be found in the Appendix of [Weber, 2012]. Weber actually uses a slightly stronger logic, obtained by adding the Counterexample rule $A, \sim B \vdash \sim (A \rightarrow B)$.

[55] A consequence relation is said to be *Tarskian* if it is reflexive, transitive, and monotonic. The standard reference for substructural logics is [Restall, 2002]. It should be noted that the substructurality of a logic depends on its presentation: for example, the Routley-Meyer semantics for relevant logics are usually taken to generate a Tarskian consequence relation, despite the proof theory being substructural.

[56] This is far from the only way to understand transitivity: see [Ripley, 2018] to stare into the abyss.

One way to implement this in a natural deduction context is to maintain that *normal* deductions are the only truly valid proofs, so transitivity must fail whenever its application would provide a non-normal proof.[57] This is somewhat inoffensive when we have a normalization theorem stating that *every* proof is normalizable, and this is in fact the case for Gentzen's classical and intuitionistic systems; however the normalization theorem can fail if new inference rules are added. The idea is then to use the non-normal status of triviality proofs in the extended system as a uniform excuse to dispense with them. Note that this does not undermine transitivity for normalizable fragments, so the logic remains (in principle) quite strong.

The same idea can be carried out in a sequent calculus framework, where the analogue of transitivity is the so-called Cut rule. The role of the normalization theorem is then played by Gentzen's cut elimination theorem, which states that the Cut rule is redundant in the standard sequent presentation of classical logic. A consequence of this is that one loses nothing by not including the Cut rule, but rather gains the possibility to add rules which contradict it. This is exactly the way [Ripley, 2015] obtains nontransitive *extensions* of classical logic. Again, classical recapture can then be achieved by proving cut elimination theorems for given fragments.

Dropping *only* unrestricted transitivity while keeping a logical base which contains Explosion leads to a very particular sort of inconsistent mathematics. Even if contradictions may be proved, there are no further theorems that we can prove *from* the contradictions, because by design contradictions cannot be cut (or normalized away) and therefore they are not allowed to function as lemmas. Still, we are free to explore the consequences of each provable conjunct on its own; and *both* branches belong to the theory, even if we cannot use results from one branch to develop the other. [Istre, 2017] explains the situation as follows: *"Inconsistencies are [...] places where we are allowed to make assumptions. Perhaps these are places where our theories are in fact overdetermined due to the innate complexity at those points"* (p.160). I will discuss Istre's nontransitive set theory in Section 3.1.

Let us move on to our second example, Contraction. Here is one striking reason why we may want to avoid it. Following [Beall and Murzi, 2013], suppose that we have a language expressive enough to contain a validity predicate on premise-conclusion pairs, and let $C \leftrightarrow \mathrm{Val}('C','\bot')$. Suppose C: then, by definition of C and modus ponens, we get $\bot$. But this means $\mathrm{Val}('C','\bot')$, which again by

[57] A natural deduction proof is *normal* if, roughly speaking, it makes no unnecessary detours. A system is *normalizable* if every theorem of the system has a normal derivation in that system. For details on this and cut elimination, see e.g. [Mancosu et al., 2021]. The idea of taking normality as a criterion for validity was put to mathematical work by [Tennant, 2017] and [Istre, 2017].

definition of C gives us C, and therefore $\bot$ by modus ponens. So absurdity is a theorem, with no assumptions whatsoever! This may not sound like much of an issue for mathematics; however, the worry is that a truly unrestricted naive set theory would be able to code such a paradox within itself.

If we are not willing to blame modus ponens, one alternative is to pay attention to the number of times we used C in the proof. Strictly speaking, the first half of the proof did not give us $\mathrm{Val}('C',' \bot')$; rather, it gave us $\mathrm{Val}('C'\&'C',' \bot')$. The paradox is then solved by dropping (structural) Contraction, which states that if $A, A \vdash B$ then $A \vdash B$. Sure enough, this is a way to obtain a nontrivial, yet still very expressive, formulation of naive comprehension, as seen for example in [Terui, 2004]'s Light Affine Set Theory (LAST), which relies on the contraction-free logic **BCK**. The idea was then picked up by [Badia and Weber, 2019], who added the relevant **DKQ** conditional to the mix. The resulting logic is called **subDLQ** (substructural dialetheic logic with quantifiers).

Since we will see quite a bit of Weber's mathematical work based on **subDLQ**, I include here an axiomatization from [Weber, 2021a, ch.4]. Note that $\rightarrow$ is the relevant conditional and $\Rightarrow$ is the **BCK** one. Axioms:

1. $A \rightarrow A$
2. $(A \rightarrow B)\&(B \rightarrow C) \rightarrow (A \rightarrow C)$
3. $(A \rightarrow B) \rightarrow (\sim B \rightarrow \sim A)$
4. $A\&B \rightarrow A$
5. $A\&B \rightarrow B\&A$
6. $A\&(B\&C) \rightarrow (A\&B)\&C$
7. $A \rightarrow A \vee B$
8. $B \rightarrow A \vee B$
9. $A \vee \sim A$
10. $\sim\sim A \leftrightarrow A$
11. $A \vee B \leftrightarrow \sim(\sim A \& \sim B)$
12. $A\&B \leftrightarrow \sim(\sim A \vee \sim B)$
13. $A\&(B \vee C) \leftrightarrow (A\&B) \vee (A\&C)$
14. $A \vee (B\&C) \leftrightarrow (A \vee B)\&(A \vee C)$
15. $\forall x A \leftrightarrow \sim \exists x \sim A$
16. $\exists x A \leftrightarrow \sim \forall x \sim A$
17. $\forall x A \rightarrow A^x_t$
18. $(A \rightarrow B) \Rightarrow (A \Rightarrow B)$
19. $\sim(A \Rightarrow B) \Rightarrow \sim(A \rightarrow B)$
20. $A\& \sim B \Rightarrow \sim(A \Rightarrow B)$
21. $A \Rightarrow (B \Rightarrow A\&B)$
22. $(A \Rightarrow (B \Rightarrow C)) \Rightarrow (A\&B \Rightarrow C)$
23. $(A \Rightarrow C) \Rightarrow ((A \Rightarrow B) \Rightarrow (A \Rightarrow C))$
24. $(A \Rightarrow (B \Rightarrow C)) \Rightarrow (B \Rightarrow (A \Rightarrow C))$
25. $A \Rightarrow (B \Rightarrow A)$
26. $(A \Rightarrow C) \Rightarrow ((B \Rightarrow C) \Rightarrow (A \vee B \Rightarrow C))$
27. $\forall x(A \vee B) \rightarrow (A \vee \forall x B)$, x not free in A
28. $\forall x(A \Rightarrow B) \Rightarrow (\exists y A^x_y \Rightarrow B)$, x not free in B
29. $\forall x(B \Rightarrow A) \Rightarrow (B \Rightarrow \forall y A^x_y)$, x not free in B
30. $\forall x(A(x)\&B(x)) \Rightarrow \forall x A(x)\&\forall x B(x)$

Rules:

- if A and $A \Rightarrow B$, then B
- if A then $\forall x A$
- if $x = y$ then $A(x) \to A(y)$
- if $A \leftrightarrow B$ then $C(A) \leftrightarrow C(B)$

Dropping Contraction naturally comes with an *intensional* conjunction (here denoted by $\&$): repeating a sentence is not necessarily equivalent to only stating it once. More poetically: repetition brings one closer to the corresponding state of affairs. What does this mean for mathematics? Without Contraction, we have to keep track of how many times we use each hypothesis in the proof of a theorem: falling short will (in principle) invalidate it. Therefore, Weber's mathematics contains such wonderful definitions as "*x is an adherent to X iff: either $x \in \sup X \cup \inf X$; or else for every $\epsilon \in \mathbb{R}$, if $\epsilon > 0$ and (repeating) $\epsilon > 0$, then [...]*", anticipating that in applying this definition the positivity of ϵ will have to be used twice [Weber, 2021a, p.256]. This certainly *looks* weird, but on the other hand one can imagine the potential insights spurred by being forced to pay this much attention to hypothesis use.[58]

Thanks to an extensive use of excluded middle, **subDLQ** can support a form of reductio, proof by cases and some limited instances of Contraction: most significantly, Contraction on *theorems* turns out to be valid, so they can be used in a proof as many times as we want as long as we state them just as many times in the premise. Still, the strong requirement of premise tracking is bound to make the logic quite a hard sell to mathematicians. Maybe some more general classical recapture result for contraction use could help clarifying things: in analogy with the nontransitive approaches, one would hope for Contraction to be - in some sense - fine "in most cases", thus justifying our intuition of its validity. There seems to be quite the gap from Curry sentences to theorems! It may also be that a fixed, sufficiently large number n of repetitions would be redundant for most proofs, and then we could just be content with theorems talking about n-conjunctions. But this is mostly speculation. I will say more about the specifics of Weber's mathematics in Ch.3.

[58]That being said, one can easily imagine this spiraling out of control very quickly as soon as the maths gets a bit more advanced - at the very least a more compact notation would have to be introduced!

2.5 Adaptive logics

When it comes to everyday reasoning, it is sometimes argued that the weakness of a given paraconsistent logic is not too worrying, because we only need to use it in some very special cases, and context can provide evidence for when we are in such cases. There is, after all, no need to entertain the possibility that the front door could be both open and closed every time we try to leave the house. If we believe, with [Beall, 2009], that contradictions are sparse, at the limits of thought so to speak, then as long as we are well away from said limits we have no reason to worry: it is only when we get close that we may want to abandon our *default* (i.e. classical) ways of reasoning.[59] We may name this the *global default strategy*: use classical logic unless inconsistencies are expected, in which case switch to a paraconsistent logic.[60]

The global default strategy works particularly well for logics like **LP** where it is fairly clear what the relation with classical deductions is, especially if we look at it in a multiple conclusion setting: then we can see that a form of Disjunctive Syllogism is present in **LP** as $\sim A, A \vee B \vdash B,\ A \wedge \sim A$.[61] In a context with no contradictions, this automatically delivers the classical rule back, which in turn delivers classical logic back, as discussed in [Beall, 2013a].

In the context of inconsistent mathematics, however, the global default strategy is unhelpful because the very fact that we are doing inconsistent mathematics means that we know from the start that there are going to be inconsistencies. Given the usual working assumption that our axiom sets are consistent until proven otherwise, inconsistency will only appear if we intentionally postulate it, contradicting the global default right away. But this is the same as just picking a different logic for inconsistent mathematics! If we really want to exploit the idea of default, we need some kind of *local default strategy*: instructions on how to deal with contradictions within a theory without thereby preventing classical deductions from taking place when possible. This is where *adaptive logics* come in.

The core idea of the adaptive approach to inconsistent mathematics is that contradictions can be assumed to be not true until proven otherwise.[62] In technical terms, one fixes a *lower limit logic* (LLL), a set of *abnormalities*, and a *strategy* to deal with the appearance of an abnormality in a derivation. The *upper limit logic* (ULL) is the one used when no abnormalities appear. In the paradigmatic scenario,

[59] This rarity of inconsistency is controversial among dialetheists. See [Weber, 2021a, ch.3] for a very different take.

[60] Technically, the default doesn't have to be classical logic. We may just as well pick intuitionistic logic, for example.

[61] Here the comma on the right is meant to be read as a (classical) disjunction.

[62] For a comprehensive introduction to the wonderful world of adaptive logics, see [Straßer, 2014].

the ULL is classical logic and the LLL is some subclassical paraconsistent logic, with the set of abnormalities including at least all atomic contradictions. While the LLL might be too weak to prove much of interest, an adaptive logic built on it will provide plenty more *conditional* results, i.e. formulas that are true on pains of abnormality. On the so-called *minimal abnormality* strategy, a formula becomes a proper *theorem* of the logic if it can be (conditionally) derived on every possible minimal choice of true abnormalities. An *adaptive gain* is achieved if the theorem was not derivable in the LLL.

Adaptive logics are, in general, *nonmonotonic*: it is not the case that $\Gamma \vdash B$ implies $\Gamma' \vdash B$ for every $\Gamma' \supset \Gamma$. This is an intended feature, and comes from the desire to capture defeasible reasoning: in general we cannot know for sure that the discovery of new information will not undermine our argument, so adding premises cannot always preserve validity. While the dispute about whether a nonmonotonic logic is really a logic needs not concern us, it must be said that mathematical reasoning shares with (the standard idea of) logical reasoning the apparent impression of undefeasibility, as evidenced by the fact that formalization in classical logic is taken to be always possible in principle.[63] But at the end of the day, adaptive theorems are as unshakeable as any other theorem: the strategies are complete and deterministic, and every backtracking during a derivation inevitable. Classical meta-reasoning can be used to determine whether a theorem has been proven indefeasibly, so even if one does not trust adaptive logic to underlie mathematical reasoning in general, one can still treat the resulting theorems as the classical outcome of a particular procedure. One possible objection is that in general the need for meta-reasoning ensures that adaptive logics are not even *semi-decidable*, i.e. there is no algorithm which can list all the theorems following from the axioms.[64] But we hardly make use of such an algorithm in mathematical practice, and the approach could still be deemed rational even if not standardly "logical"; in fact, part of the motivation for adaptive logics was precisely to formally describe real instances of human reasoning where there seems to be no such algorithm.[65]

What properties do we want an adaptive logic to have? [Batens, 2000] suggests three desiderata for an adaptive version of **LP**, call it **ALP**:

[63] Such a picture has been questioned, or at least argued to be not comprehensive enough. A few examples: [Lakatos, 2015] shows that proofs and concepts can refute each other in a cycle of revisions; [Ferreirós, 2015] points out that what we currently call a proof might always be invalidated by the rejection of the community; [De Toffoli, 2021] explicitly argues in favor of a fallibilist epistemology of mathematics.

[64] Every logic with a (finitary) Hilbert-style presentation is semi-decidable. A logic is decidable if and only if both its set of theorems and its set of non-theorems are semi-decidable.

[65] All of this is discussed in [Batens et al., 2009].

- *strong reassurance* asks that every **LP**-model can be restricted to a model of **ALP**;
- (weak) *reassurance* asks that every nontrivial **LP**-theory is also a nontrivial **ALP**-theory;
- *classical recapture* asks that **ALP** agrees with classical logic on inferences from consistent sets of premises.

The most popular version of **ALP** is called *minimally inconsistent* ***LP***, or ***LPm***, first presented in [Priest, 1991].[66] Here, the validity of an **LP** inference is checked by assessing the conclusion only in those premise-satisfying models containing the least amount of contradictory facts. There are a few different ways to cash in this intuition, depending on what we take facts to be and how we order models with different domains. Still, [Crabbé, 2011] shows that **LPm** - regardless of how we deal with domains - fails strong reassurance; in fact, it even fails weak reassurance if the language is expressive enough, e.g. contains both function symbols and equality.[67]

It is worth thinking about what these properties mean for inconsistent mathematics, and whether they are really needed there. The insistence on reassurance, weak or strong, seems to me in principle unwarranted.[68] As pointed out earlier, **LP** has a model for *everything*, so not allowing all of them to count as a counterexample for every argument is hardly a reason to pull one's hair out. Batens is worried about the situation where Γ has some models which satisfy A and some which do not, but the only minimally inconsistent ones are among the former. Then $\Gamma \vdash_{ALP} A$, which might seems like the wrong call: after all, we know how to build a counterexample which no amount of adapting will adjust. But in the context of mathematics, ignoring pathological counterexamples can be a legitimate way to proceed: this is a logical version of what [Lakatos, 2015] calls *monster-barring*. Besides, the counterexamples are not really lost: the **LP**-models satisfying Γ but not A remain available for inspection if we ever want to see what happens in them, regardless of what **ALP** says.[69]

[66] It is worth mentioning that **LPm** is not an adaptive logic in the so-called "standard format": see [Batens, 2013] for comparison.

[67] There is some confusion on this in the literature, as Priest changed the definition of **LPm** a few times, and the final proof of Reassurance published in [Priest, 2006b] is fatally mistaken. When in doubt, check [Crabbé, 2011] and [Priest, 2017].

[68] Which is not to deny that they make adaptive life a lot easier.

[69] There is an analogy here with using sentences like $0 = 1$ as approximations of triviality in relevant logics. We can have good reasons for ending our inquiry into A at $A \to 0 = 1$, and if A nevertheless happens to have some non-trivial models so be it; but if we really wanted to check them for whatever reason, we still can.

A similar point goes for weak reassurance: if some theories are too trivial for **ALP**, so be it. Not only we are still free to study their **LP**-models independently of this fact; we could also try and develop new adaptive strategies tailored to these particular theories. There is no particular reason why there should be one perfect adaptive strategy which works for *every* theory. That being said, we may want to be sure that there exist at least *some* inconsistent theories which **ALP** does not trivialize: we may call this *weakest reassurance*.

What about classical recapture? First of all, using any contraclassical extensions of **LP** (e.g. **dLP**) as an ULL will immediately fail this, and presumably the proposers of such logics would have no problem with it. But even if we interpret classical recapture as a request for **ALP** to not be *weaker* than classical logic in a consistent context, one can still object that some consistent structures come with intrinsically weaker logics, and forcing classicality there may well trigger undesired collapses. In such a scenario, it seems natural to ask that *both* the LLL and the ULL be subclassical.[70]

2.6 Chunk & Permeate

The idea of default - global or local - suggests the possibility of using two different logics in the pursuit of inconsistent mathematics: a strong one in "safe" contexts, and a weaker one around contradictions. But can we go further? Is there a way to systematically reason with a piece of inconsistent mathematics by means of any plurality of logics whatsoever?

The *Chunk & Permeate* (C&P) procedure is one such way. It was introduced in [Brown and Priest, 2004] as a formal model of reasoning from inconsistent sets of sentences which does not force inconsistent conclusions, nor does it require an underlying paraconsistent logic. The original applications were concerned with "rationally reconstructing" how historically mathematicians *could* have reasoned when faced with prima facie inconsistent premises. The first paper on the topic attempted such a reconstruction of the basic method of infinitesimals in the early calculus; later iterations dealt with Bohr's theory of the atom and Dirac's delta

[70] To be fair, [Batens, 2002] notes that the focus on classical recapture is mainly motivated by the pragmatic desire to cater to already existing mathematics and science: *"the situation is that scientists think to be using **CL**, that most historians think scientists always (implicitly) did, and that logicians have no decent practical alternative (nor results to prove the scientists and historians wrong)"* (p.139). So there seems to be no objection to dropping classical recapture in the search for alternatives, or in new scientific contexts.

function.[71]

The most general version of C&P, as presented in [Priest, 2014b], works as follows. A *C&P structure* $\mathcal{M}$ consists of:

- A set of theories - or *chunks* - $\{C_i : i \in I\}$, each with its own language $\mathcal{L}_i$ and consequence relation $\vdash_i$.
- A designated $i_o \in I$, indicating the *output chunk*. This specifies where the end result of the procedure is to be read from.
- For every $i, j \in I$, a *permeability filter* $\rho_{ij} \subseteq \mathcal{L}_i$ and a *translation function* $t_{ij} : \mathcal{L}_i \to \mathcal{L}_j$. Intuitively, the relation ρ_{ij} specifies which consequences can be carried from C_i to C_j, while t_{ij} specifies how they are to be carried.

The set of theorems for every chunk is recursively defined as follows, where $\overline{C_i^n} = \{\alpha : C_i^n \vdash_i \alpha\}$:

- $C_i^0 = C_i$
- $C_i^{n+1} = C_i^n \cup \bigcup_{i \neq j \in I} \{t_{ji}(\phi) : \phi \in \overline{C_j^n} \cap \rho_{ji}\}$
- $C_i^\omega = \bigcup_{n \in \omega} \overline{C_n}$.

In words: first the chunks are closed under their own consequence relations, then they simultaneously pass each other all the information they can while respecting the permeability relations. Then we close under logical consequence again, and so on. The procedure is repeated ω times, and in the end we take the limit. Since we are only interested in what is left in the output chunk, we say that $\mathcal{M} \vdash \alpha$ iff $\alpha \in C_o^\omega$.

Let us look at a simple example. The calculation of derivatives in [Brown and Priest, 2004] involves only two chunks, C_s and C_t, both classical. Both contain the second-order theory of the reals, but the language also contains two extra symbols: a function(al) symbol D (for derivative) and a function symbol δ (for infinitesimal). The source chunk C_s contains $Df = \lambda x((f(x + \delta x) - fx)/\delta x)$ and $\forall x \delta x \neq 0$, while the target (designated) chunk C_t contains $\forall x \delta x = 0$. The idea is, of course, to capture the fact that infinitesimals were taken to be both zero and not zero at different times in a derivation: first the differential is computed by treating the infinitesimal distance as nonzero (which amounts to drawing the consequences of C_s), and then in the final step all terms with

[71] See [Brown and Priest, 2015] and [Benham et al., 2014], respectively. See also [Heyninck et al., 2018] for extensive criticism of C&P as a way of understanding how people actually reason.

infinitesimal coefficients are forgotten. This is achieved by letting the permeability relation only allow transfer from C_s to C_t, and only of equations of the form $Df = g$ (no translation necessary), with some obvious restrictions. This works as intended, in the sense that the set of designated theorems does in fact turn out to be consistent and contains precisely the consequences that were historically drawn. Integration can also be implemented in a similar fashion.

It is controversial whether this kind of application of C&P should be considered inconsistent mathematics. [Brown and Priest, 2015] suggests that a C&P structure should be considered consistent if the output chunk is non-trivial at the end of the procedure. Under the assumption that the chunks are classical, this simply means that consistency depends on the consistency of the output chunk. If that's the case, none of these "historical" applications of C&P are actually inconsistent. Furthermore, [Mortensen, 2017] argues that the approach *"must meet the objection that to believe a conclusion obtained on this basis, one should believe all the premisses equally; and so an argument of the more common form, appealing to all the premisses without fragmenting them, should be eventually forthcoming. The objection is thus that Chunk and Permeate is part of the context of discovery rather than the context of justification"* (pp.10-11).

Still, there are two points to be made here. First of all, there are far less classical ways to apply C&P. In general, the final set of theorems needs not be consistent: the chunks, and in particular the output chunk, can be ruled by paraconsistent logics. For example, [Priest, 2014b] suggests importing the classical consequences of Peano axioms to relevant naive set theory (with the translation restricting quantifiers to ω), where they could live peacefully without generating triviality. There seems to be no reason to not consider this inconsistent mathematics, since we would still get a bunch of contradictions from the naive comprehension axiom.

The second point is that, even if we somehow still buy a strict distinction between context of discovery and context of justification despite living in a post-[Lakatos, 2015] world, it is not obvious that C&P has to be part of the former. The fact that C&P does not consist of a standard logical argument is hardly sufficient:[72] mathematics is certainly not new to problem-solving algorithms, and in fact the early calculus itself has been argued by [Vickers, 2013, ch.6] to be an example of algorithm-based practice. It seems untenable to maintain that most of 18th century maths was "mere" discovery. Furthermore, belief can be and routinely is detached from logical commitment, and therefore it seems possible - contra Mortensen - to believe every premise from an inconsistent set and their C&P consequences

[72] Actually, every C&P structure *can* be reduced to a structure in the logic of the output chunk, as shown in [Priest, 2014b]. However, it could be objected that such a reduction is generally intractable in practice, and therefore not particularly meaningful.

without any need for logical reconstructions of the more common form.[73]

2.7 Logical pluralism

I could go on.[74] But I will stop here, having given enough examples for future reference, and hopefully having abundantly made the point that any of the previous sections could have easily made on its own: there are a *lot* of possible choices of logic, infinitely many, involving wildly diverse strategies, most of which may generate different theories when applied to the same mathematical concepts. Almost every conceivable inconsistency-tolerant logic - and, as per C&P, even classical logic itself - is a potential tool for inconsistent mathematics. Given the amount of available logics and essentially unlimited ways to combine them to create some apparently meaningful piece of mathematics, the idea of identifying *the* logic of inconsistent mathematics seems somewhat absurd.

Now, there might be ways to narrow the field. Maybe one of these logics will eventually come out as the most fruitful; maybe only one logic is in some sense the *correct* one; or maybe one logic could suffice to encompass all of the current work. I now want to argue that none of these options can escape pluralism, or at least the kind of pluralism that can be witnessed by (prima facie) logically incompatible mathematical practices.[75]

First of all, as of now, there is *nothing* in the whole landscape of paraconsistent logics even remotely approaching the kind of technical and conceptual stability that intuitionist and classical logic enjoy; it is but an infinitely large and unfocused collection of variously motivated (and largely unmotivated) formal systems, even when restricting to the field of logics considered for mathematics. At best, some logics get special attention because of their status as a starting point for more interesting expansions, as it is the case for **LP** and **mBC**. But since there's no consensus whatsoever on what the right expansion should be, or even what the right expansion should *do*, this is not too much help. Furthermore, most paraconsistent logics have both a "classical" and a constructive version: **FDE** is a gappy version

[73][Kitcher, 1984] convincingly argues that the push towards logical rigor in the 19th century was a response to special contingent needs rather than a general tendency, let alone a tendency intrinsic to or necessary for mathematics.

[74]Some notable omissions are *annotated logics*, which are amongst the most deployed in applications [Abe et al., 2015][Zamansky, 2019]; *preservationist logic*, in which inference is taken to preserve "inconsistency level" rather than truth [Jennings and Schotch, 2009]; and *Abelian logic*, which has the integers as its semantics and is insane and I love it [Meyer and Slaney, 1989].

[75]It is of no importance to the arguments in this dissertation whether this particular understanding of pluralism constitutes "real" logical pluralism; that being said, [Caret, 2021] argues just that. For a defense of paraconsistent pluralism that does not go through mathematics, see [Bueno, 2002].

of **LP**, **N4** is a gappy version of **RM3**, and so forth.

Things are not much better at the level of mathematics. Consider, for contrast, constructive mathematics. The notion of constructive proof is not an invention of some nonclassical logician: it is something that every mathematician has at least a rough idea of. A proof that all natural numbers satisfying some property P are nonprime is clearly constructive if it consists in an algorithm that, given any number satisfying P, produces a divisor; any proof that the (classical) real numbers can be well-ordered is clearly nonconstructive, as no such well-ordering can be exhibited.[76] Everything in-between these extremes may fall on one side or the other depending on what it comes closer to, and there may even be disagreement on some borderline cases, but overall there is a fair amount of agreement. Matching the informal characterization, there is widespread agreement on what this means at the logical level: start from a standard formalization of classical mathematics, and remove the law of excluded middle together with every rule or axiom which would imply it (most notably, proof by reductio and the axiom of choice). This takes one from classical logic to intuitionistic logic.[77]

Such a widespread agreement seems to have no correspondent in inconsistent mathematics. What does it mean, in logical terms, to accept inconsistencies? It does not mean rejecting the principle of non-contradiction, since many inconsistent theories are grounded in logics (e.g. **LP**) that do in fact accept it together with its negation; and it does not mean taking it as inconsistently valid, since many other logics (e.g. **mbC**) take it to be just invalid.[78] It also does not mean to reject Explosion: non-transitive approaches can preserve it, and either way there is no agreement whatsoever on *how* to reject it. In the end, the difficulty in choosing between different logics comes down to the fact that there is simply no shared informal notion of inconsistency-tolerant proof.

We could say that the correctness of a logic for inconsistent mathematics depends on its goals. But then our correctness criteria will depend on why we are pursuing inconsistent mathematics to begin with. If for the sake of foundations, we want a logic that can underlie the required foundation; if for the sake of

[76] The existence of such a well-order is generally taken to follow from the Axiom of Choice, which is *the* non-constructive axiom par excellence. See e.g. [Jech, 2003, Thm 5.1].

[77] To be more precise, I should note that constructiveness can be *graded*. For example, [Nelson, 1949] argues that the intuitionistic negation $\neg A := A \to \bot$ is not really constructive, and should be replaced by what has come to be known as a *strong* negation. And when it comes to foundational theories, CZF (constructive ZF) is more restrictive than IZF (intuitionistic ZF), although they are still both based on intuitionistic logic (see e.g. [Crosilla, 2009]). But my point still stands: there is no such thing as a hierarchy of inconsistency-tolerance which can linearly order the various proposals, nor is there a privileged mainstream proposal.

[78] For the dual argument to the effect that the principle of non-contradiction struggles to characterize consistent mathematics, see [Brady, 2004].

dialetheism, we want a logic that can describe the world as it is; if from duality considerations, we want a logic that truly mirrors intuitionistic logic; if from Plumwoodian considerations, we want a logic that stays as far away from dualism as possible; and so on. This is already a clear road to pluralism, as long as we take all these motivations seriously: requirements are often incompatible, there is no evidence whatsoever that a single logic can take care of all these jobs, and there is no particular reason why a single logic *should*.

A similar line of argument also takes care of the misguided idea that one logic could or should capture all of the inconsistent mathematics on the market, as long as we take all of said mathematics seriously.[79] Compare, to give just one example, the **subDLQ**-based dialetheic set theory of [Weber, 2021a] and the LFI-based hierarchy of paraconsistent set theories ZF_i in [da Costa, 1986]. As we will see in Section 3.1, the former countenances nothing resembling a classical universe, and in fact *every* set has inconsistent subsets; meanwhile, the latter straightforwardly extend classical set theory and treat classical sets as fully consistent. The former countenances every set coming out of naive comprehension; the latter leave the existence of inconsistent sets open, and in fact many cannot be included on pains of triviality. These are not compatible pictures, and it would be contrary to the spirit of both to treat them as such, e.g. by attempting to embed them in some overarching multiverse.[80] Furthermore, the kind of logics that work for one picture are inadequate for the other and vice versa: Da Costa's LFIs are too strong to countenance unrestricted naive comprehension, while **subDLQ** is too weak to countenance any sort of straightforward classical recapture.

What remains open to the monist is the exclusionary road: to argue that only one kind of inconsistent mathematics is *really* inconsistent mathematics, or at least that one inconsistent mathematics takes priority over all others, and that is where we should read our correct logic from. But taking current practices seriously means naturalizing away concerns about "the one true inconsistent mathematics" at least in the sense that, even if some inconsistent mathematics was deemed false or not the best, the logic underlying it can nevertheless constitute evidence for pluralism, insofar as said mathematics can be recognized as having *some* value. We can also follow [Caret, 2021] in joining the naturalist attitude with an argument to the best explanation: acceptance of pluralism is the best way to explain what mathematicians working in logically incompatible practices are doing, regardless

[79]It is not necessary, for this point to go through, to argue that it is impossible to *translate* all of inconsistent mathematics within a single framework. In fact, classical logic could probably do that job just fine. However, this simply does not lead to the conclusion that classical logic *underlies* all of inconsistent mathematics. All kinds of properties get lost in translation.

[80]Which is not to say we cannot accept them both.

of what metaphysical judgements we cast on those practices from the outside.[81]

Here is a possible objection. There is a difference between reasoning *with* a logic, and reasoning *about* theories and structures which the logic underlies. Some of the logics we have seen, like **LP**, are useless to reason with, and the only way we get results is by thinking about **LP**-models; others, like adaptive logics, require meta-reasoning in order to get any indefeasible conclusion. One might then try to argue that the logic underlying inconsistent mathematics is the one underlying the metalanguage we adopt to reason about such structures.

For most of the existing work in inconsistent mathematics this perspective just ends up reaffirming classical logic. The structures we are interested in may be non-classical, but the space of such structures is treated classically: there is - at the moment - nothing like a systematic paraconsistent model theory, in the sense of being itself formulated in a paraconsistent logic.[82] Technical difficulties with developing alternatives aside, the adoption of classical logic is supported by considerations that are part pragmatic (classical logic is usually stronger and simpler), part social (classical logic is better understood by the larger community), and part based on classical default arguments. If we end the objection here, then the conclusion seems to be that the underlying logic of most inconsistent mathematics is classical logic; I am going to skip over waxing philosophical about the meaning of logic, and simply dismiss this as a wildly unhelpful perspective, since it erases the very concrete impact of adopting different logics while leaving nothing in its place.

Now, the objection *could* be a stepping stone towards a call for exploring more meta-languages. If we accept that we can use nonclassical meta-languages, however, then emphasizing the meta-language as an objection to pluralism naturally leads to pluralism resurfacing at the meta-level. Changes in metalanguage can give significantly different perspectives on logics, and even change the way we understand their properties. For example, there is a sense in which **LP** is as expressive as we could possibly want it to be: in fact, from the perspective of **LP** itself, **LP** is as expressive as classical logic![83] We are as free to pick our metalanguages as we are free to pick our object languages, and giving extra importance to the meta-language simply pushes the pluralism up one level. For my two cents, rather than engaging in this infinite regress, it makes more sense to just take the object language seriously and treat different metalanguages as different perspectives or contexts, as suggested e.g. in [Shapiro, 2014, ch.7] and

[81] [Kouri Kissel, 2018] defends a similar conclusion on pragmatic grounds.

[82] Although development is underway: see [Badia et al., 2022] for some initial results in the paraconsistent model theory of substructural **LP**.

[83] [Omori and Weber, 2019] make this point with respect to truth.

[Passmann, 2021].[84]

2.8 The nihilist attitude

Having hopefully convinced the reader that there is no evidence for logical monism in the context of inconsistent mathematics broadly understood - i.e. no evidence pointing at the one true logic of inconsistent mathematics - while examples of prima facie pluralism and good reasons to take them seriously abound, let me try and push the point even further: in the same context, *nihilism* can be a better option than pluralism.

Logical nihilism has seen some attention in the last decade, with a few different - if related - meanings. The common methodological upshot I am interested in is the openness to *all* possible logics: call this the *nihilist attitude*.[85] For example, according to [Russell, 2018], we can concoct a counterexample for every purportedly valid argument; it follows that the one true logic - conceived as the set of inferences valid in *all* contexts - is empty.[86] Depending on the context, any logic might come in handy. Meanwhile, according to [Franks, 2015], the whole monism vs pluralism debate should take a back seat insofar as it is detrimental to logic as a discipline, which would be better served by focus on the *relationships* between different systems from different perspectives. Every logic is relevant to

[84] Of course, there could be any sort of reasons to wish for a particular metalanguage in a particular project. For example, [Weber, 2021a] requires that the distinction between object language and metalanguage be dropped entirely. But Weber's work is just one drop in the puddle of inconsistent mathematics.

[85] This may sound like the eclecticism of [Shapiro, 2014]. However, Shapiro puts fairly strong requirements on the admissibility of a logic: namely, it must underlie interesting mathematical structures, and this fact has to be acknowledged by the mathematical community at large. His tolerant attitude comes from the fact that we can hardly find out if a logic satisfies the requirement without actually trying it out; but without a radical revision of what is usually meant by "mathematically interesting structures", this is still a far cry from admitting all sorts of logics.

[86] See also [Mortensen, 1989] and [Estrada-González, 2011].

such a study.[87]

Now, I am fairly sympathetic to logical nihilism in both these senses, but I defer to its proposers for a defense of it qua philosophy of logic. The question I want to focus on here is what these arguments mean for mathematics. Are Russell's counterexamples pertinent to mathematics? Is sticking to a single logic problematic for mathematics? Well, if you ask *classical* mathematics, the answer to these questions is of course a resounding "no": classical logic is enough to formalize all of mathematics, and classical inference has no mathematical counterexamples. However, opening the door to inconsistent mathematics can change matters a bit. To be more specific, I want to show that the arguments for logical nihilism, taken together with (some of) the arguments for inconsistent mathematics, naturally encourage a nihilist attitude concerning logic in mathematics.[88]

The first observation is that the argument from pure maths goes quite well with Franksian nihilism. If *any* maths is fair game as long as it is there, then it seems to follow that any logic - and combination thereof - should be fair game too. Limiting ourselves to a particular logic, or spending all our energies trying to fit everything into one logic, risks distracting us from the real treasure at the heart of inconsistent mathematics; there is a whole world of mathematical relationships between structures with different underlying logics to study!

Next, consider the argument from liberation. Bringing into mathematics all kinds of inconsistent interpretations would help counteracting the dualistic hegemonic influence of classical mathematics. But on Russellian grounds, we can generate interpretations to invalidate any logical law; so in order to truly be open to any possible interpretation, we need to be open to any possible logic - which just means, to any logic whatsoever. The point can also be made from the following perspective: every change in logic can be seen as a reinterpretation of logic, and in particular an inconsistent reinterpretation if we move towards a paraconsistent

[87]Another variety of nihilism comes from [Cotnoir, 2018]: *"There's no logical consequence relation that correctly represents natural language inference; formal logics are inadequate to capture informal inference"* (p.301). This is partly because different situations require different logics, as argued by logical pluralists; and partly because of phenomena like vagueness, semantic closure, and unrestricted quantification, which have been proven again and again to resist complete formalization (in a way that preserves the goal of matching natural language inference, anyway). So there can be no correct logic, in the sense of correct formal representation of natural language reasoning. I am going to set this version aside because, while inconsistent mathematics is sometimes concerned with capturing just these linguistic phenomena, it is not clear to me that Cotnoir's arguments commit one to the nihilist attitude as I have defined it. For all that Cotnoir says, it may still be the case that there is a relatively small class of logics which are better than others at overall approximating natural language inference.

[88]Again, I take this to be largely independent of whether we accept them to lead to any sort of "proper" logical nihilism, since e.g. one could take logic as related to mathematics to be different from logic as what the debate is about.

logic, insofar as inconsistent models will become available. So logical nihilism is *itself* the kind of subversion that the argument from liberation pushes for.

It might be objected that liberation should nevertheless be focusing on logics that are as non-dualistic as possible. So for example, following Plumwood's understanding of dualisms, all logics allowing for premise suppression should be avoided. This seems to be, after all, what Plumwood herself is suggesting: to *replace* classical logic with a logic that lacks dualistic features. We could weaken this requirement to an acceptance of any logics that are at least strictly less dualistic than classical logic, but this would still exclude classical logic itself, not to mention all sorts of logics that are similarly dualistic. While this is certainly a pluralist perspective, calling it a nihilist one might be a stretch.

Now, a common response to this kind of strategy is to pull a reductio. Since classical negation is explosive, getting rid of dualistic logics means getting rid of classical negation; getting rid of classical negation means getting rid of all classical dichotomies; and that is *absurd*, because there is nothing oppressive about "odd" and "even" being exclusive.[89] But I think this is too quick. A relevant negation like the one defended by [Sylvan and Plumwood, 1985] *does* allow for classical dichotomies, when it is deployed in classical situations; and classical situations, far from being postulated out of existence, are part and parcel of the semantics for relevant logics. To reject classical negation only means to reject it as the default notion of negation, which would make *every* dichotomy classical; relevant negation is more general, so it allows for classical *and* nonclassical dichotomies. The point is that it should be up to the context, *not* the logic, to determine whether a dichotomy is classical or not.[90]

Nevertheless, I think there are other reasons to be skeptical of the strict anti-classical approach. It is far from obvious that broader strategies for liberation should turn their back to the potential benefits of classical logic (and mathematics). [Russell, 2023] lists several ways in which logic - classical or not - can be adopted for feminist purposes; such uses need not automatically lead to the kind of reverse dualism Plumwood is worried about, where the oppressed become the new oppressors. Furthermore, as pointed out by [Eckert, 202x], the very features which in classical logic help centering the master's perspective can be used to center oppressed perspectives when their voice needs to be heard. In fact, this centering is *crucial* if the epistemic injustice brought by their exclusion is to be

[89] The argument is repeated in [Restović, 2023], which concludes from it that Plumwood should be more charitably interpreted as a pluralist who is not, in fact, demanding a revision of classical logic (p.248).

[90] An extensive discussion of the relationship between relevant and classical negation can be found in [Sylvan et al., 1982, Sect 2.9].

fought effectively.[91] So giving up on classical logic altogether may be not only a tactical mistake, but even itself a contribution to oppression!

2.9 Formalism freeness

Having made our peace with the unavoidable plurality of approaches to the inconsistent, it is legitimate to ask whether or not we can find some criteria to group together different formalizations, so as to reduce the infinite amount of possibilities - often only differing from each other in mathematically insignificant ways - to a more manageable and hopefully insightful classification. Of course, this is also a valid question for classical mathematics: there are many ways to make rigorous the same informal, vague, incomplete idea, and there are many ways to make that rigor formal in the logician's sense, even if we agree on the logic.[92] But it is far more urgent for inconsistent mathematics because of the lack of agreement on a logical framework, even more so if we adopt a nihilist attitude. By showing that mathematical concepts and theorems arising from inconsistent practices are somewhat stable under changes in formalization, we prevent them from being dismissible as a mere logical quirk, and make it easier to reason intuitively with them. It would also be helpful for general requirements to make sense independently of the choice of logic: for example, if we had a definition of "algebraically non-trivial" that does not depend on the logic, we could use it to uniformly set aside some algebraically uninteresting structures across the universe.

It is helpful to think of such issues in terms of *formalism freeness*, as presented in [Kennedy, 2020]. This is *"the idea that certain canonical concepts and constructions are stable across a variety of conceptually distinct formalisations, so seemingly insensitive to perturbations of logic and syntax"* (p.2). The dual concept is that of *entanglement*, which *"signals a breakdown in the adequacy of our formalisations by exposing misalignments between entangled formalisations and the intuitive concepts they are meant to capture"* (p.7). Formalism freeness allows us to free mathematical content from the tangential whims of particular formalizations; it provides some degree of *stability*, which in turn is often taken as a sign of having found the *correct* notion in the neighborhood.[93]

It is important to note that formalism freeness is not meant to imply

[91] This is formalized - using classical logic! - by [Saint-Croix, 2020].

[92] It is also not the case that in classical mathematics the logic is always agreed on: for example, continuous logic is usually a nicer option than classical logic when it comes to formalizing classical metric structures [Yaacov et al., 2008].

[93] It is in principle unnecessary to take this sort of correctness to carry metaphysical weight; it may be (and often is) simply understood as a bundle of theoretical virtues. Of course, many philosophers and mathematicians are happy to make the extra step; but hardly all of them.

independence from *every* choice of formalization: *"Formalism freeness is not an all or nothing affair, but a matter of degree"* [Kennedy, 2013, p.356].[94] A particularly pertinent example: while the classical equivalence of different presentations of computability is a paradigmatic example of formalism freeness, it was shown in [Meadows and Weber, 2016] that the equivalence collapses fairly quickly in some paraconsistent logics.

Let us see some positive examples. One kind of formalism freeness can be called *logic-independence*, i.e. *"invariance under substitution of one of a class of logics, considered on a case by case basis"* [Kennedy, 2020, p.92]. Kennedy's most striking showcase of this involves the notion of *set constructibility*.[95] A set is said to be constructible if it belongs to the set-theoretic universe generated by the following inductive definition (ranging over *all* ordinals):

- at stage 0 we have all sets definable without parameters;
- at stage $\alpha > 0$ we have all sets definable with parameters from previous stages.

Every step of the construction is very dependent on the logic, since what is definable and what not depends on the language. However, when we look at (classical) logics between first-order and second-order, we find a high degree of convergence: in infinitely many logics (e.g. first-order logic) we end up with Gödel's constructible universe L, while in infinitely many others (e.g. second-order logic) we get the universe of hereditarily ordinal-definable sets HOD.[96] Infinitely many cases are thus collapsed together, cementing constructibility as a far more robust notion than definability.

A different variety of formalism freeness consists in giving a *purely mathematical* definition of some prima facie logical notion. One of the most famous examples is Birkhoff's theorem that a class of algebras can be axiomatized by a set of equations if and only if it is closed under homomorphisms, subalgebras, and direct products; the formalism, which is encapsulated by the notion of axiomatization, simply disappears from the right-hand side.[97] Note that a purely mathematical definition needs not be logic-independent: for example, while classical first-order equivalence can be given what is arguably a formalism free

[94] In fact, Kennedy is only concerned with a particular range of extended *classical* logics. Opening the door to nonclassical logics makes the all or nothing ideal even more implausible.

[95] This is unrelated to the notion of constructibility I was discussing earlier, which is at the center of constructive mathematics.

[96] This does not cover *all* possible logics between first-order and second-order, although most of the famous ones are accounted for. For example, adding any or all of the cardinality quantifiers does not make a difference.

[97] See [Sankappanavar and Burris, 2012, Thm 11.9].

characterization in terms of the existence of isomorphic ultraproducts,[98] such a characterization is not logic-independent because equivalence in other logics - even classical ones - does not (usually) support the same characterization. That being said, there are cases of overlap: for example, the constructible universe L - which we just saw to be highly logic-independent - can be purely mathematically defined in terms of so-called Gödel functions.[99]

Since inconsistent mathematics was born and raised in formalism, it should not be surprising that a desire for formalism freeness has made its appearance in the literature before. Mortensen's already discussed notion of *functionality*, for example, can be seen as an attempt to provide a logic-independent characterization of computational nontriviality; this is particularly noticeable in the favorable comparison with *transparency*, which is far more dependent on the logic. The attempt in [Humberstone, 2020] to characterize *contradictoriness* (amongst other things) in a way that makes sense across different logics can be seen in a similar light.[100] On a different note, [Restall, 2013] offers necessary conditions on the formulation of naive principles which are formalism free to the extent that they don't involve any logical connectives: his goal is explicitly that of moving the conversation away from the quirks of any given formal language, thus avoiding the entanglement of naive principles with, say, the nature of the conditional.

There are also occasions where the acceptance of inconsistencies breaks the formalism free status of a notion. For example, [Tedder, 2021] suggests that once we accept inconsistent models we need to appropriately revise our notion of *essential undecidability*:[101] this is because there are arithmetical theories where the undecidability of consistent extensions appears to be completely independent of the decidability of *in*consistent extensions. Ideally, the revised notion should make sense across as many different paraconsistent logics as possible.

The potential advantages of a formalism free approach to inconsistent mathematics are many: it can put order into the myriads of different systems, guide research away from tangential worries about the exact choice of formalism and towards more substantial questions, abstract new mathematical concepts from the particular formal way they have been introduced, and better connect with classical informal mathematics. Thus, keeping an active eye for formalism freeness could be quite fruitful. We will see in Chapters 3 and 5 how formalism free notions can naturally arise from various kinds of inconsistent practices.

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[98] This is the so-called Keisler-Shelah theorem: see [Chang and Keisler, 1990, Thm 6.1.15].

[99] See [Jech, 2003, ch.13].

[100] The proposal is that A and B are contradictories w.r.t. a logic $\mathbf{L}$ if and only if there is no $\mathbf{L}$-sound bivalent valuation assigning the same truth value to both A and B.

[101] A theory is *essentially undecidable* if it and all its consistent extensions are undecidable.

2.10 Conclusion

In this chapter we have seen that all kinds of logics - including classical logic - can be adopted for the sake of inconsistent mathematics, forcing a pluralist attitude insofar as we want to take inconsistent practices seriously. This may evolve into a nihilist attitude in connection with certain motivations for inconsistent mathematics: in particular, the argument from liberation has been shown to go hand-in-hand with Gillian Russell's brand of nihilism, according to which an openness to all kinds of interpretations requires an openness to all kinds of logics. As a stabilizing counterpart to this welcome yet dazzling variety of logical methodologies, I suggested an active search for formalism freeness.

In the next chapter, we will take a close look to how inconsistent mathematics built on these logics can look like.

Chapter 3

Examples of inconsistent mathematics

Now that we are familiar with motivations and methods, the time has finally arrived to see what inconsistent mathematics can actually look like. The goal is to give an idea of the main lines of work in the field, showcase how the logics we discussed have been put to use in the literature, and provide some examples of how particular proposals may be assessed with respect to their respective projects.

3.1 Saving naive comprehension

The vast majority of the literature on inconsistent mathematics focuses on set theory. This is partly because of its foundational status, but also because it provides a very clear technical goal, which is to nontrivially incorporate the *Naive* (or *Unrestricted*) *Comprehension* schema:

$$\exists y \forall x (x \in y \leftrightarrow \phi(x)).$$ [1]

The appeal of the schema is two-fold. First, it is part of the history of set theory: it was featured in foundational work from the second half of the 19th century, until the set-theoretic paradoxes led to its replacement by *Restricted Comprehension* (also called *Separation*):

$$\forall z \exists y \forall x (x \in y \leftrightarrow \phi(x) \land x \in z).$$

[1] ϕ can be taken to range over all first-order formulas, or only those formulas which do not contain any free occurrence of y. The two versions end up being equivalent in many systems - for example, the classical one.

This restriction avoids the paradoxes and it is good enough for mathematics, as long as it is supplemented by several existence axioms ensuring we have access to basic set-theoretic operations (pairs, unions, power sets), infinite sets, and choice functions. This, together with the Extensionality, Replacement, and Foundation axioms, constitutes the currently accepted ZFC set theory, with the cumulative hierarchy as its intended model.[2] Still, the question remains: could mathematics have kept Naive Comprehension around after all?[3]

A second, related reason to be interested in Naive Comprehension is its connection with the so-called *naive conception* of set. The idea here is that sets just are extensions of concepts: *every* concept determines a set, namely the set of entities falling under the concept. Rejecting Naive Comprehension means rejecting the naive conception: for example, ZFC does not countenance the set of all sets which do not belong to themselves. This doesn't leave set theory without a conceptual basis, since ZFC has been extensively argued to capture an *iterative conception* of set;[4] still, the rise to power of a different conception does not magically erase the previous one. The search for a naive set theory can then be seen as as an attempt to reevaluate the naive conception.

[2]Extensionality provides identity conditions for sets: if two sets have the same elements then they are identical. Replacement says (roughly) that every image of a set is a set. Foundation says that there are no infinite descending membership chains (in particular, no set belongs to itself): this ensures that a set is always strictly more complex than any of its elements. While Extensionality tends to be part of every self-respecting naive set theory, and Replacement is generally entailed by Naive Comprehension, Foundation restricts the universe of sets and is therefore prima facie incompatible with Naive Comprehension. The original axiomatization by [Zermelo, 1908] did not include Replacement and Foundation; these were suggested respectively by [Fraenkel, 1922] and [von Neumann, 1925]. The cumulative hierarchy, first introduced by [Zermelo, 1930], is inductively defined as follows:

- $V_0 = \emptyset$;
- $V_{\alpha+1} = \mathcal{P}(V_\alpha)$, where $\mathcal{P}(X)$ is the power set of X;
- $V_\lambda = \bigcup_{\alpha<\lambda} V_\alpha$ for λ limit.

The privileged status of the hierarchy as a model can be explained by Zermelo's "quasi-categoricity" result: if $\mathcal{M}$ is a model of second-order ZFC, then $\mathcal{M} \cong V_\kappa$ for some κ.

[3]This may suggest a new kind of argument from philosophy of maths (see Section 1.5), where inconsistent mathematics is proposed as evidence for the *contingency* of mathematics. Because the commonly accepted terms of the inevitabilist/contingentist debate require that the proposed alternative be at least as fruitful as the current theory (see e.g. [Van Bendegem, 2016]), this is what needs to be shown for *Possibility*. *Indispensability* is easier than usual, since there is no need to show the alternative theory has any advantage whatsoever; it merely needs to be shown to be sufficiently different. That being said, *Possibility* and *Indispensability* are at odds with each other, since requiring exactly the same level of fruitfulness seems likely to lead to the kind of intertranslatability which would defeat *Indispensability*.

[4]See e.g. [Parsons, 1977].

The central problem of naive set theory is that Naive Comprehension is not merely inconsistent. Simply rejecting Explosion is not enough to support the schema, because the notorious Curry paradox (and its many variants) leads *directly* to triviality. The argument goes as follows. Consider the set $y = \{x : x \in x \to \bot\}$, which must exist by Naive Comprehension. If $y \in y$, then $y \in y \to \bot$ (by definition of y), hence $\bot$ by modus ponens. Thus $y \in y \to \bot$. But then $y \in y$ (by definition of y), and therefore $\bot$ by modus ponens. This is clearly unacceptable, so any naive set theory will have to somehow prevent this argument from going through.[5] Things get even more complicated when identity enters the picture: very roughly, even if we prevent the membership relation from behaving in ways that are susceptible to Curry-like paradoxes, the Grisin and Hinnion-Libert paradoxes show that such behaviours risk being transmitted from the identity relation to the membership relation via Extensionality.[6] Now, sufficiently weak logics may stop the derivations of the paradoxes; but then they may be too weak to prove anything of note, thus making the set theory unable to say much at all, let alone interpret mathematical theories like set theories are generally expected to do. How to best reach a balance between the desire for deductive strength and the need to avoid triviality, that is the problem.

A related issue concerns what Naive Comprehension is actually supposed to *mean*. The axiom is rarely expressed in classical terms; after all, we already know that the classical framework leads to triviality. But if we change the vocabulary in which the axiom is expressed, then the problem arises of whether or not the new version is at all what we were trying to recover in the first place. This is particularly relevant insofar as Naive Comprehension is often justified on the grounds of being *intuitively true*; this line loses a lot of its persuasiveness if we have to interpret the language in an unintuitive way in order to get the theory to work.[7]

[Restall, 2013] uses a bilateral framework to make the issue particularly evident. The very idea of Naive Comprehension seems to be intrinsically bound with some particular commitments concerning assertion and denial, which can be expressed in a sequent calculus without relying on any particular connectives. This, however, makes it really difficult to escape triviality. To be more precise, Restall

[5] This is of course analogous to the validity paradox discussed in Section 2.4. Note that the argument does not depend on the presence of $\bot$ in the language: the point is that via the Curry paradox we can prove *every* sentence.

[6] See [Weber, 2021a, ch.4] for the technical details.

[7] [Incurvati, 2020, ch.4] proposes two explanations why the naive conception might look intuitive despite its falsity, based respectively on the truth of a naive conception of sets *of individuals*, and a confusion of sets with objectified properties.

argues that the inferential commitments implicit in Naive Comprehension,

$$\frac{\Gamma, \phi(a) \vdash \Delta}{\Gamma, a \in \{x : \phi(x)\} \vdash \Delta} \; (\in L) \qquad \frac{\Gamma \vdash \phi(a), \Delta}{\Gamma \vdash a \in \{x : \phi(x)\}, \Delta} \; (\in R)$$

extensionality,

$$\frac{\Gamma, x \in a \vdash x \in b, \Delta \qquad \Gamma, x \in b \vdash x \in a, \Delta}{\Gamma \vdash a = b, \Delta} \; (E)$$

and identity,

$$\frac{\Gamma, \phi(a) \vdash \Delta}{\Gamma, a = b, \phi(b) \vdash \Delta} \; (= l)$$

are incompatible. Together with some uncontroversial structural rules - which Restall takes to be constitutive commitments of discourse in general - these rules lead to triviality: we can derive any formula A via the *Hinnion class* $\{x : \{y : x \in x\} = \{y : A\}\}$.

Now, it is of course open to the naive set theorist to reject Restall's characterizations (or the structural rules, for that matter). The problem is simply that, at least prima facie, these commitments are part of what naive set theory *means*. Nontrivial forms of Naive Comprehension are forced to reject some of its apparent constitutive commitments (or, even more dramatically, some of the constitutive commitments of discourse); so justifying such rejections should be part and parcel of defending a naive set theory.[8]

Assessment criteria

In the next few subsections we will go through some of the contemporary approaches to naive set theory and see how they fare, both against each other and with respect to their stated goals. Although assessing these theories is not my intent here, it will be helpful for the sake of comparison to spell out some assessment criteria which have been taken more or less seriously in the literature. Most of them can be easily adapted to the analysis of any other piece of inconsistent mathematics, although any given criterion may be more or less important depending on the project.

The first question is obvious:

1. How much of Naive Comprehension is preserved?

[8] Restall also applies similar considerations to inconsistent theories of arithmetic, where he argues that the natural commitments end up excluding the finite models, and to the naive truth schema, which collapses to triviality (modulo a few extra assumptions) much like Naive Comprehension does.

The reason we might be willing to accept an answer different from "all of it" is because not every way of restricting Naive Comprehension may be said to drive us away from the naive conception. For example, we will see several examples of naive-like set theories involving a restriction of the *language* allowed by the defining conditions. One might try and argue that this kind of restriction, unlike the ZFC one, still preserves the main idea of Naive Comprehension.[9]

As we will see, Naive Comprehension does not by itself guarantee much structure. For consistent sets, the hope might be to recover all or most of what we can classically say about them. But it seems to be just as important that the new, inconsistent part of the universe comes with some structure of their own, at least if we want its study to be mathematically interesting. In sum, we have two more questions:

2. To what extent is classical recapture achieved?

3. How structured is the inconsistent part of the universe?[10]

Note the difference between criteria 1 and 3: the former asks about the *extension* of the expansion, while the latter asks about its *depth*. We will see that there are models of naive set theory which truly countenance every naive set, yet fail to endow inconsistent sets with a nontrivial structure; conversely, non-well-founded set theory is a clear example of how lots of structure could be added to a standard universe without coming anywhere close to satisfying Naive Comprehension.

Next, we may turn to more standard criteria of theory choice. I take *fruitfulness* to be partly encapsulated by criterion 3, but structure is not everything: for example, inconsistent sets may be independently useful in modelling or computations. Two other standards that will be particularly relevant in the following discussion are *naturalness* and *simplicity*. Naturalness matters because Naive Comprehension is often praised for its intuitiveness, so an ad hoc version of naive set theory loses a lot of its appeal. Simplicity will mostly be aimed at the underlying logic: diversions from classical logic can raise some significant decidability issues, or make it really difficulty to work informally. So:

4. How fruitful is the resulting theory?

[9] Quine's NF is a famous example of restricting comprehension via a strict regimentation of the language. However, he did not really take his solution to constitute a fair representation of the naive conception. [Incurvati, 2020, ch.6] calls this the *stratified conception*. A hierarchy of inconsistent stratified set theories extending Quine's system can be found in [da Costa, 1986].

[10] This should not be taken too literally: not every naive set theorist believes in a clear distinction between consistent and inconsistent sectors of the universe.

5. How natural is the resulting theory?

6. How simple is the resulting theory?

The distinction between theory and metatheory has often been the subject of debate among inconsistent mathematicians. This is partly because of the general argument that a theory might be better represented by a semantics relying on a matching logic, and partly because historically one of the main driving goals of dialetheism has been to achieve semantic closure, thus rejecting the distinction between language and metalanguage altogether. Because of this, we might want to ask:

7. How discordant are theory and metatheory?

Finally, in the context of this thesis, it is important to ask:

8. Is this inconsistent mathematics?

This is not always obviously the case: as already discussed in Ch.1, supporting the naive conception of set does not automatically commit one to inconsistent mathematics.

All that being said, let us get started with some actual proposals.

LP-based naive set theory

There is a very simple way to avoid the Curry paradox. As discussed in Section 2.1, **LP** is an extremely weak logic from the deductive point of view: in particular, *modus ponens* fails. Because of this, **LP**-based naive set theory - the axiomatic theory consisting of nothing more than Naive Comprehension and Extensionality, and having **LP** as its underlying logic - is *provably* safe from triviality.

There are two ways of formulating the theory, depending on how we decide to treat identity: we may follow [Priest, 2017] in taking it as a logical primitive with classical extension but arbitrary anti-extension,[11] or follow [Restall, 1992] in defining it as "belonging to the same sets", i.e.

$$x = y := \forall z(x \in z \leftrightarrow y \in z).$$

Call the former theory $\text{NST}^{=}$, and the latter NST. While NST is slightly stronger, both theories admit some finite models which may be used to prove nontriviality. As [Weir, 2004] notes, the former version has the following one-element model:

[11] The extension of a binary predicate is the set of pairs satisfying the predicate; the anti-extension is the set of pairs satisfying its negation. Since **LP** satisfies LEM, extension and anti-extension must together cover all pairs; but they may have non-empty intersection.

$\in$	x
x	b

$=$	x
x	t

This universe consists in a single lonely set which both belongs and does not belong to itself. The theory is non-trivial: it cannot prove, say, $\exists x\, x \neq x$. However, *every* sentence of the form $t_1 \in t_2$ or $t_1 \notin t_2$ holds in the model, meaning that the theory cannot rule out any of them. So the nontriviality is minimal.

NST rules out the above model by proving that there are at least two different sets (though they may also be identical). Still, [Restall, 1992] presents a two-element model where the *only* sets are the universe V and its one distinct element r.[12] Formally:

$\in$	V	r
V	b	f
r	b	t

$=$	V	r
V	b	f
r	f	b

Thus NST is non-trivial too, since the model refutes e.g. $\forall x \forall y(x \in y)$ and $\forall x \forall y(x \notin y)$.

Now, unintended models are hardly a phenomenon unique to inconsistent mathematics. First-order ZFC has some notorious unintended models: in fact, by Löwenheim-Skolem, it has *countable* models. But to use a simpler example, think of the nonstandard models of first-order PA. We can easily tell that they are not *the* natural numbers; in fact, every such model provably *extends* the natural numbers.[13] However, this does not disqualify PA as a theory of arithmetic because:

1. the core inferential commitments of arithmetic have arguably been captured;

2. we know what the standard model is and how it relates to the nonstandard ones.

The unintended models of **LP**-based naive set theory are not worrying because they are unintended. They are worrying, first, because they appear to display a failure to capture the core inferential commitments of the naive conception of set, insofar as they invalidate Restall's $(\in L)$;[14] and second, because we (currently) have no

[12] The hard part is Extensionality; any universe containing at least one set x such that everything both belongs and does not belong to x (i.e. such that the $\in$ table contains a column of only b) is a model of Naive Comprehension.

[13] For an introduction to nonstandard models of arithmetic, see [Kaye, 1991].

[14] To see this, note that in the 2-element model $r \in \{x : V \in x\}$ (because r belongs to every set), but it is not the case that $V \in r$. [Ripley, 2015] notes that $(\in L)$ and $(\in R)$ *could* replace the comprehension schema in the axiomatization without inducing triviality, but then classical recapture would be blocked: the only **LP**-model satisfying both the resulting theory and ZFC is the trivial one.

intended or *canonical* model to tell us what the "real" naive universe is supposed to look like.[15]

The first point is connected to the fact that these theories are way too weak to carry out any mathematics in them. [Restall, 1992] shows that, while all of the ZFC axioms can be proven in NST (except Foundation, as would be expected), the same can hardly be said of most of their consequences: most dramatically, functionality in Mortensen's sense fails.[16] Working in $\mathrm{NST}^{=}$, [Thomas, 2014] amply showcases the inability of the theory to express basic mathematical concepts like singletons, Cartesian pairs, and infinitely ascending linear orders. Furthermore, contra Priest's hopes, going nonmonotonic does little to solve the problem: in **LPm** and all of its variations discussed in [Crabbé, 2011], either we have the same exact issue or the Weir model is the *only* model.

It is not hard to build highly complex inconsistent **LP**-models. A straightforward way to do this is via the *Collapsing Lemma*, a technical tool that lets us "collapse" classical models onto **LP** models in a way that preserves truths but may add contradictions.[17] To be more precise, given a congruence relation $\sim$ on an **LP**-model $\mathcal{M}$, consider the *collapsed* structure $\mathcal{M}^{\sim}$ such that $\mathcal{M}^{\sim} \models \phi(a_1, ..., a_n)$ if and only if there exists $b_1, ..., b_n$ with $b_i \sim a_i$ such that $\mathcal{M} \models \phi(b_1, ..., b_n)$; in particular, this means that $\mathcal{M}^{\sim} \models a = b$ if and only if $a \sim b$. The Collapsing Lemma then says that $\mathcal{M}^{\sim}$ is still an **LP**-model of the same theory: in symbols, if $\mathcal{M} \models \phi$ then $\mathcal{M}^{\sim} \models \phi$. Collapse can only add satisfied formulas, never remove them; note that every proper collapse is inconsistent, since it must satisfy some inconsistent identities.

A remarkable feature of the Collapsing Lemma is that any classical substructure of the original model can be fully preserved by the collapse, simply by putting each element of the substructure in its own distinct equivalence class.

[15] One could try and argue that e.g. some two-element model is the intended one, the two elements being "being" and "not being" or whatever. I am not aware of any serious attempt to do so, although some of the more colorful interpretations of the Routley set in [Weber, 2021a] might be read in that direction. See also [Estrada-González, 2016] for a straight-face defense of a trivial one-element structure.

[16] Restall doesn't discuss Choice, but like most other axioms it can also be expressed as an instance of Naive Comprehension. To see functionality fails, consider the following model:

$\in$	a	b	c
a	t	f	b
b	t	f	b
c	t	f	b

$=$	a	b	c
a	b	b	b
b	b	b	b
c	b	b	b

Then $a = b$ and $a \in a$ hold, but $b \in b$ does not. Note that this is also a counterexample to Restall's $(= l)$, of which functionality is a special case.

[17] This is essentially an idea of [Dunn, 1979], which [Ferguson, 2017] later extended to a more general framework; see also [Ferguson, 2019b] for discussion in expanded languages.

[Priest, 2017] exploits this in order to build **LP**-models of naive set theory which extend the classical universe. Take any fragment V_α of the cumulative hierarchy (which is a model of ZFC), and collapse together all elements not in V_α: this is a model of NST$^=$ which by the Lemma also satisfies all *theorems* of ZFC. If κ is an inaccessible cardinal, then V_κ itself is a model of ZFC;[18] so the collapsed model consists of a fully classical universe together with a single inconsistent set trivially satisfying *all* the conditions dictated by Naive Comprehension, since everything both belongs to it and doesn't.

There is also a way to have a properly inconsistent cumulative hierarchy as a model, with no annoying sets sitting "outside" of the hierarchy. Starting from any model of ZFC, we can build an increasing chain of models $(\mathcal{M}_\alpha)_\alpha$ such that at step α we denote by α_A, for every formula A, the set of A-witnesses in V_α. In the limit, every instance of Naive Comprehension will hence have a witness amongst the α_A's. This generates an *extended* cumulative hierarchy which models both NST$^=$ and ZFC.[19]

These models certainly show that naive models can be highly structured, especially insofar as all classical structures can be recovered within (some of) them. But structure does not a canonical model make. As Priest puts it, *"the model-theoretic constructions provided deliver no reason to suppose that they are decent models of the universe of sets"* (p.107). Lacking an intuition of what a decent model should look like, one possible next step might be to provide a more systematic classification of the models of the theory, in the hope that a particularly special model (or class thereof) might stand out.[20]

There is an axiomatic counterpart to this kind of full classical recapture. All we need to do is *shriek* the primitive predicates, i.e. postulate their consistency on pains of absurdity. Shrieking was used by [Thomas, 2013] to restrict the class of admissible models of **LP**-based set theory to the ones extending a model of ZFC. The idea is to add:

- a classicality predicate, and axioms to the effect that classical sets satisfy the ZFC axioms;
- rules to the effect that classicality and membership between classical sets behave consistently (i.e. we can reason classically with classical sets).

[18] See [Jech, 2003, Lemma 12.13].

[19] This is still a model of ZFC because of the Monotonicity Lemma for **LP**: see [Priest, 2017, p.63].

[20] As a matter of fact, some set theorists are still looking for the correct model of *classical* set theory. The cumulative hierarchy is a conceptual template, but hardly a fixed model, insofar as there is little agreement on which additional axioms it has to satisfy. Such axioms can affect, roughly speaking, the *width* and *height* of the hierarchy. See e.g. [Fontanella, 2019].

Sure enough, all ZFC theorems can then be proven to hold for classical sets.[21] This is in some sense a very deep theory, as it explicitly includes the whole of ZFC, which in turn suffices to represent most mathematics. However, it has almost nothing to say about inconsistent sets, for the same reason **LP**-based set theory doees not.

Thomas' approach shows that the classical recapture problem is, at least in some sense, trivial:[22] nothing prevents us from simply postulating that the inconsistent universe includes the classical one as a definable substructure, if we so wish. This may not sound very satisfying from either a mathematical or a philosophical point of view: the approach does not in any way help grounding classical mathematics, since it takes the fundamentality of ZFC as a starting point; it is as uninformative as NST when it comes to inconsistent sets, nor does it provide any extra technical tools to work with them; and the problem of finding a canonical model remains open. The only upshot is this: if we have no trouble believing in the consistency of ZFC (or parts of it), we are technically free to include it in our inconsistent journeys.

Strengthening the language

In a sense, the problem with **LP**-based naive set theory is that there are just too many models. Since this is directly connected to the lack of expressiveness of the language of **LP**, one idea might be to simply strengthen the language. [Libert, 2005] tries to find a middle way between finite models and ZFC-based models by adding several connectives, among which is the **A3** conditional. The Collapsing Lemma is thereby lost, but this may be seen as a positive, in the sense that many "bad" models might end up being ruled out this way.[23]

The real downside of Libert's approach is that Naive Comprehension is unable (on pains of triviality) to accommodate set-defining formulas containing the new connectives in full generality. If we are trying to capture the naive conception of set, this may sound problematic because the conception does not appear to motivate any a priori restriction on the language of set-defining formulas. Still, Libert moves on to presenting a fixed-point method of constructing topological models where a whole bunch of formulas using the new connectives happen to

[21] There is a slight technical complication: the ZFC axioms have to be expressed in prenex form, and classical recapture is only proven for theorems in said form.

[22] This is also the conclusion of [Beall, 2013c].

[23] Not that we have much of a choice: [Ferguson, 2019b] shows that there is no way to both add a truth-functional detachable conditional to **LP** *and* preserve the Collapsing Lemma.

be admissible.[24] For example, one can prove the existence of any set defined by $\forall x(\phi(x) \to \psi(x, y))$ where $\to$ is the **A3** conditional. So, even if they do not *fully* satisfy Naive Comprehension, these models might still tell us a lot about how an interesting naive set theory might work.

A different kind of solution was proposed in [Omori, 2015]. On the one hand, we add many new connectives to the language, just like Libert does.[25] On the other hand, the set-theoretic axioms and the definition of identity are still formulated entirely in the language of **LP**.[26] The good news is that, because the material conditional is so weak, it being the main connective of the axioms means that we need not exclude the new connectives from occurring in set-defining conditions; in fact, the resulting set theory is provably non-trivial. The bad news is that the theory is non-trivial because of the *same* 2-element model that made NST non-trivial, and in fact Restall's $(\in L)$ and $(= l)$ fail exactly as they do in NST.[27]

While both Omori and Libert are concerned with having a sufficiently expressive language, the two approaches are in a sense diametrically opposed. In Libert's, the extended language is used to make sure that Naive Comprehension means what it is supposed to mean, although this forces some degree of restriction on what counts as a defining condition. In Omori's, the extended language is there

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[24]Here is a sketch of the construction. Let X be any complete metric space. We can interpret formulas in X by assigning two closed subsets to each formula, an extension and an anti-extension. By LEM, the set of interpreted formulas can then be identified with

$$F(X) = \{(A, B) \mid A, B \text{ closed in } X \text{ and } A \cup B = X\},$$

which in turn admits a natural complete metric. If we think of F as an operation on spaces, it admits a compact fixed point M. The homeomorphism $h : M \cong F(M)$ provides a model of set theory when given the following interpretation:

- $x \in y$ for every $y \in h_1(x)$, and $x \notin y$ for every $y \in h_2(x)$;
- $x = y$ if x, y denote the same element of M;
- $x \neq y$ if $\exists z(z \in x \setminus y$ or $z \in y \setminus x)$.

[25]This is single-handedly achieved by adding a consistency connective, going from **LP** to **LFI1**: see Section 2.2.

[26]This kind of strategy was first suggested by [Goodship, 1996].

[27]A slightly stronger set theory is also proposed, obtained by replacing the main conditional (but not the biconditional) in the Extensionality axiom with the **A3** one. The resulting theory is still non-trivial; however it still allows for the following model, which falsifies both $(= l)$ and $(\in L)$.

$\in$	a	b	c
a	t	f	b
b	t	b	b
c	t	f	b

$=$	a	b	c
a	b	b	b
b	b	b	b
c	b	b	b

To see $(= l)$ fails, note that $c = b, a \in c$ but $a \notin b$. To see $(\in L)$ fails, note that $b \in \{x : a \in x\}$ but $a \notin b$.

so it can be used to further extend the theory, e.g. by adding axioms or introducing new pertinent notions, while Naive Comprehension remains unrestricted at the price of severely watering down the intended meaning, exactly as it was in NST. Again, one could try and suggest that this is a positive; the problem merely lies in the disconnect between the stated motivation of naive set theory and the apparently off-topic implementation. The limitations have not been independently argued for, and there can be no pragmatic justification as long as the reasons why we want a naive set theory essentially involve the fact that Naive Comprehension is thereby captured.

Adaptive approaches

Let's take a step back. What reasons do we really have to believe that naive set theory, a theory with the alleged potential to describe anything whatsoever, should be an interesting one? Maybe there just isn't much that can be indefeasibly said to hold true of everything. Maybe the best way to make progress with naive set theory is to rely on some defeasible choice principles, like "reject contradictions (unless you absolutely cannot)".

This kind of approach has always in a sense been part of mathematical practice. Reasoning routes are pursued until they lead to problems, at which point something is changed. This was even more widespread in the days before formalization, when the scope of methods and definitions was almost never explicitly set out in advance. This was also the attitude towards paradox that many had in the early days of set theory: many were aware that trying to reason with certain collections lead to contradiction, but this did not lead to their rejection - only the acknowledgement that they needed to be reasoned with differently (or ignored).[28]

An early post-Zermelo approach of this kind can be found in Paul Finsler's set theory.[29] Roughly, the idea is to countenance all possible sets *except* those that lead to contradiction. In this framework, a definition refers to an object only if the existence of that object does not lead to a contradiction; otherwise, the definition fails to point at anything and is simply said to be inadmissible. In particular, the idea that every well-formed formula constitutes a mathematical property is rejected: admissibility is not a *syntactical* property, but a *mathematical* one. The downside is that we cannot know in advance what is admissible and what not, and if we countenance several sets at once there may be no algorithm to decide which ones are the real problem. Because of the lack of an independent way to distinguish

[28] See e.g. Cantor's letter to Dedekind in [Van Heijenoort, 1967, pp.113-117]. Cantor's distinction between consistent and inconsistent multiplicities can be seen as a precursor of the modern distinction between sets and classes, which is expressed independently of inconsistency.

[29] See e.g. [Finsler and Ziegler, 1996].

between good and bad definitions, Finsler's strategy was not well received by his peers, and his set theory was overshadowed by ZFC.

More recently, as we have seen in Section 2.5, a formal framework for defeasible reasoning has been provided in the form of adaptive logics. The big difference from earlier informal approaches consists in the presence of a precise, deterministic set of instructions on how to deal with contradictions. This somewhat softens the blow of defeasibility: bad instances of e.g. Naive Comprehension, while still lacking an a priori characterization, are not rejected altogether, and we can locally adjust our reasoning when they lead to trouble. One possible downside is that it can be difficult to justify any particular choice of strategy: much like in Finsler's set theory, the framework itself does little to explain why we should accept certain truths and not others, over and beyond the fact that some lead to contradiction and some do not.

Two different kinds of adaptive naive set theories have been proposed in [Verdée, 2013]. The first one, *Maximally Consistent Comprehension* (MCC) set theory, takes as its lower limit logic an expansion of classical logic with a diamond operator such that $A \rightarrow \Diamond A$ for every $\Diamond$-free A.[30] Naive comprehension is then expressed in a modalized form: $\Diamond\exists y\forall x(x \in y \leftrightarrow \phi(x))$. The intended interpretation here is that all instances of Naive Comprehension are *possibly* true, but the theorems of MCC are exactly the classical consequences of all the "unproblematic" instances of comprehension, where being problematic is rigorously spelled out in adaptive terms.[31] MCC is an interesting example of a perfectly *consistent* naive set theory; however, it may be objected that Naive Comprehension is not captured in a strong sense, since the lower limit logic is unable to unconditionally derive *any* non-modalized instance. This is why Verdée presents MCC as a merely *pragmatic* foundation.

The second kind of theory, *Maximally Rich Universal* (MRU) set theory, takes as lower limit logic a 4-valued extension of classical logic involving a special kind of biconditional which is used to express Naive Comprehension.[32] MRU has a far richer unconditional basis than MCC, meaning that plenty of new theorems can be derived already in the lower limit logic. It also contains a paraconsistent negation which supports the derivation of *inconsistent* theorems. However, MRU is acknowledged by Verdée to be more difficult to interpret than MCC, as the meaning of the logical connectives appears to change depending on the consistency of the formulas they appear in.

Both theories are haunted by the existence of *Curry sets*: these are sets that,

[30] $\Diamond\phi$ should be read as "it is possible that ϕ". The assumption is made that diamonds cannot be nested nor occur in the scope of quantifiers.

[31] One possibility is to say that an instance A is derivable only as long as $\Diamond A \wedge \neg A$ is not.

[32] This approach is also discussed in [Verdée, 2012].

while innocuous enough on their own, if left unchecked have the unpleasant power to spread inconsistencies all over the theory (via variants of the Curry paradox, as the name suggests). What this means in practice is that, unless some measures are introduced to deal with these sets, both theories end up having no adaptive gain, i.e. they can prove no theorems that could not have been proved already in the lower limit logic. This may be thought to defeat the purpose of using an adaptive logic, if what we ultimately care about is indefeasible consequences. The issue can be fixed by a little tweaking of either the set of abnormalities, the axioms, or the strategy; this could be seen as somewhat ad hoc, but the pragmatic stance undermines this kind of worries. A different reaction could be to just embrace the inevitability of defeasibility; such is life, after all.

[Batens, 2019] has recently attempted to provide a somewhat cleaner solution by imposing a complexity order on the set of abnormalities in a way that prevents bizarre Curry constructions from "infecting" sets at a lower level. On a technical level, this can be achieved through the use of a *sequential* adaptive logic, and there are many different ways to implement the core idea.[33] The resulting theories are flaunted as the best current examples of what a true Fregean set theory should look like, although none of them have been studied in much detail yet.

A general problem with adaptive set theories is their complexity. Since the adaptive logics in question satisfy Reassurance,[34] all of these theories can be (and have been) shown to be non-trivial by simply exhibiting a finite non-trivial model for the lower limit theory. But of course, as for any adaptive approach, the price to pay is the need for meta-reasoning and loss of semi-decidability.

The adaptive set theories discussed here are open to some ZFC-friendly tweaking, in the sense that the unconditional truth of the classical ZFC axioms can be simply imposed by divine intervention without much hassle. Non-triviality can be proven, although of course only relative to ZFC. Once again, classical recapture turns out to be an almost trivial affair on a purely technical level, although once again this way of achieving is unlikely to suffice for foundationalist or logicist aims.

Rejecting transitivity

In a sense, the philosophy underlying the adaptive approach is that standard logical reasoning, with its assumption of undefeasiblity, is *not enough* for naive set theory: the base logic must be supplemented by conditional reasoning. A different diagnosis might be that standard logical reasoning actually goes *too far*: when

[33] Very roughly, sequential adaptive logics combine different adaptive logics by applying one after the other. See [Straßer, 2014, ch.3].

[34] Recall this means that if the lower limit theory is non-trivial then the adaptive theory is too.

dealing with Naive Comprehension, some of our fundamental assumptions about deducibility have to go.[35] In this section I am going to look at the *Normalized Naive Set Theory* (NNST) from [Istre, 2017], which avoids triviality by rejecting the *transitivity* of deducibility.[36]

The starting point for NNST is a standard intuitionistic natural deduction system for naive set theory, where extensionality and Naive Comprehension are included as the introduction and elimination rules for, respectively, identity and membership. Of course this would usually lead to triviality, but Istre's idea is to treat as valid all and only the *normal* derivations in the system. What is excluded by this move is precisely the derivations of the usual paradoxes - or, to be more precise, the derivations of absurdity through the usual paradoxes. NNST is inconsistent - it proves, say, that the Russell set both belongs and doesn't belong to itself - but it is also provably non-trivial: we can prove a contradiction from the axioms, and by Explosion we can prove absurdity from any contradiction, but there is no way to stitch these proofs together into a normal proof of absurdity from the axioms.[37] Furthermore, NNST is strong enough to interpret second-order Heyting arithmetic, which in turn is normalizable (so all of its theorems remain valid) and generally acknowledged to suffice for most of constructive mathematics. It is still an open question whether a classical version of NNST could interpret second-order Peano arithmetic, although the answer is conjectured to be affirmative. Such a result would arguably be more than enough to consider the goal of classical recapture achieved.

It is conjectured that the set of NNST-proofs is not decidable - because normalizability is not - which does not bode well for the future of NNST automated proof search. Despite this, the proposed philosophical justification for the loss of transitivity is based on a *computational* reading of proofs. The central idea is that *"normalization in a natural deduction system is the process by which we construct the proof that transitivity is implying exists"* (p.156). If we cannot

[35]Framing the matter differently, we could say that the adaptive approach is also merely dropping a basic assumption, namely monotonicity. It depends on whether we consider the lower limit logic or the upper limit logic to be the starting point.

[36]Istre's is not the only non-transitive naive set theory around. [Ripley, 2015] presents a similar approach from a sequent calculus perspective, and is somewhat unique in tackling Restall's challenge head-on: the natural commitments of naive set theory are preserved, including a Naive Comprehension rule allowing for full intersubstitutivity, but triviality is avoided by dropping unrestricted Cut, which is argued to be too strong of a commitment in certain contexts - the reasons for this go well beyond mathematics, and are discussed in [Ripley, 2013]. Another nontransitive set theory, based on a paraconsistent and paracomplete "neoclassical logic", comes from [Weir, 2015]; however this theory is (apparently) consistent, and in fact the goal is *"to see just how far we can go, how strong a logic can we get, without rendering naive truth or set theory inconsistent"* (p.3).

[37]See Theorem 6.31.

actually construct the proof, then simply *postulating* that there is such a proof is uncalled for: in particular, such a postulated proof carries no persuasion power. The reason why the transitivity assumption is so pervasive is that most of mathematics (in particular, whatever can be interpreted in second-order PA) can fit into normalizable proof systems, so the assumption is usually "safe" and provides a useful reasoning shortcut; but where we do not have a normalizability result, e.g. in set theory, transitivity ends up being completely ungrounded. In this sense NNST may actually be a faithful model of informal mathematical reasoning, only correcting our unfortunate tendency to generalize shortcuts outside of their safe haven.

Is NNST inconsistent mathematics? It certainly appears to countenance some inconsistent sets, and in fact is able to prove some contradictions about them; furthermore, it has plenty of *consistent* mathematical content insofar as it can provably retrieve a large body of established mathematics. Finally, the consequences of each conjunct of a provable inconsistency can be incorporated into the theory (although never mixed), so there is a sense in which contradictions are fruitful even if strictly speaking nothing can be proved from them *qua contradictions*.

Relevant set theory

A classical default assumption is at least implicit in all of the approaches to naive set theory discussed in the previous sections. **LP**-models of naive set theory can contain the cumulative hierarchy; in more expressive languages we can define a consistency operator, and in some sense reason classically within its scope; in the envisioned classical NNST, we are free to reason classically as long as we are working in a normalizable subsystem, and most of classical mathematics can fit within such a system; and the various adaptive approaches are ways to make precise the idea that classical reasoning is acceptable "until we stumble into a contradiction".

According to [Weber, 2021a], classical default is a misguided ideal. Only the *true* classical theorems have to be preserved, and the true theorems are precisely those which can be derived within a sound - and therefore naive - set theory. The main technical goal is not classical recapture, but rather *paradox recapture*: enough mathematics has to be developed on dialetheist grounds to prove the arguments that justified the rise of dialetheism in the first place. If everything goes well, in the end we will have a coherent system of mathematics, logic, and philosophy that is able to best describe this inconsistent world of ours. If this new system is incompatible with classical mathematics, so be it.

Weber's logic of choice **subDLQ** (see Section 2.4) is reverse-engineered from what is needed to avoid triviality of Naive Comprehension, support basic mathematical reasoning, and reject the distinction between language and metalanguage. The two conditionals play very different roles in the set theory: the relevant conditional is essentially used to state the *axioms*, while the other one - which satisfies a deduction theorem - is used to express most *theorems*. One way to look at it is in terms of an opposition between two contrasting characterizations of sets: as intensional entities determined by properties, and as extensions determined by their elements. Both are correct, and both are used in reasoning: *"intensionality is good for sameness, extensionality is good for difference"* (p.140). As the slogan suggests, this theory makes it generally very difficult to prove that two sets are really the same, because it involves showing that a very strongly relevant implication obtains; while on the other hand any difference in membership suffices to prove that two sets are different.

How does this play out? Basic standard properties for subsethood, union and intersection can be derived. The universe V is defined as the collection of all things that have a property, while the empty set $\emptyset$ is defined as the collection of all things that have every property; both appear to work more or less as we would expect them to. Absurdity is defined as $\perp := \emptyset \in \emptyset$, and this can be used to present the universe as $V = \{x : \sim \perp\}$, something we simply *cannot* conceive out of. The Zermelo axioms can be derived as instances of Naive Comprehension; one can even obtain a "soft" well-ordering of the universe which may be interpreted as a version of global choice (pp.191–192).[38]

When it comes to paradox recapture, **subDLQ**-based set theory appears to be quite successful. Cantor's paradox can be recovered (p.186): the universe is both larger and not larger than its power set, in the sense that V is extensionally identical to $\mathcal{P}(V)$ yet there is no surjection from V onto $\mathcal{P}(V)$. Enough of a theory of ordinals can be recovered to prove the Burali-Forti paradox (p.190).[39] The universe can be shown to have the following *fixed point property*: for every ϕ, there is a set t such that $x \in t$ iff $\phi(t)$. This can be used to prove the Liar paradox (p.182). Furthermore, enough topology can be built on top of this set theory to prove the existence of inconsistent boundaries (p.286).

A most striking feature of **subDLQ**-based set theory is that the existence of the

[38]It is "soft" in the sense that there is a "soft" injection of the universe into the ordinals, where f is a soft injection if $x \neq y$ implies $f(x) \neq f(y)$. The axiom of global choice says the universe admits a choice function picking an element from each nonempty set; it is classically equivalent to the universe being well-ordered, although strictly speaking neither of these statements can be expressed in ZFC.

[39]The Burali-Forti paradox says that the set of all ordinals is and is not an ordinal. The classical solution is, as usual, to say that there is no such set.

Routley set $Z = \{x : x \notin Z\}$ clouds the whole universe in inescapable paradox.[40] Not only is every object both in Z and not in Z; it is also the case that every nonempty set includes a part of Z, which - among other things - implies that every nonempty set is Dedekind-infinite.[41] All of this seems to support Weber's thesis that contradictions are literally everywhere, from boundaries to predicates to objects themselves, which in turn suggests there can be no such thing as classical default.

Even if we fancy this overdose of contradictions, **subDLQ**-based set theory has some trouble dealing with some basic mathematical notions. Functions are quite problematic, insofar as names cannot be assigned to them in a uniform fashion: in fact, the above fixed point theorem turns the very presence of a function symbol into a potential source of triviality (p.185). Furthermore, the interchangeability of partitions and equivalence relations fails, endangering the very notion of quotient; this also leads to the failure of the Cantor-Bernstein theorem (if two sets inject in one another, then they are the same size), thus seemingly preventing any Cantorian notion of cardinality (pp.193-195).[42] None of this means that mathematics cannot function in this framework; rather, it means that a whole lot of new, framework-appropriate notions and tools will have to be devised, and that the end result is likely to look very different from anything we are used to.

Another difficulty concerns the reduction of other branches to set theory. I will only briefly discuss arithmetic here, as it is usually considered to be *the* test case for set-theoretic reductionism.[43] In Weber's framework, one can take the set of natural numbers to be the intersection of all inductive sets and show that it satisfies the Peano axioms (p.198).[44] One of the axioms is shrieked: it is crucial - for the sake of deductive power - that 0 be the smallest number *on pain of absurdity*. This makes the theory quite different from most previous attempts at inconsistent

[40] This does not depend too much on the specifics of **subDLQ**, and in fact the special status of this set was already noted in [Sylvan, 1977].

[41] A set is said to be Dedekind-infinite if it is in a bijection with one of its proper parts. Due to this result, Weber concludes that *"Dedekind's definition of the infinite is not appropriate here"* (p.176).

[42] It may still be open to Weber to explore notions of size different from the Cantorian one, either by requiring additional structure (e.g. geometric or topological structure may allow for some notion of dimension) or by just going contra-Cantor, e.g. along the lines of a theory of numerosities [Mancosu, 2016, ch.3]. On this last point, it is worth noting that the standard proposals for a theory of numerosities depend on ZFC-independent existence statements, so the more generous universe of a naive set theory might provide a more natural environment!

[43] Of course there is little reason to assume the classical reduction of the rest of mathematics to arithmetic would carry over to Weber's framework, so a relevant reduction of arithmetic would only be a first step anyway. Other parts of Weber's mathematics will be discussed in later sections, largely independently of his set theory.

[44] Up to contraction, anyway. A set is inductive if it contains the empty set and is closed under the operation $x \cup \{x\}$.

arithmetic, as it rules out any finite models. Addition, multiplication, and order work as usual; in fact, no contradictions are proved, although their presence is countenanced. However, in order to do any serious number theory - e.g. to prove the fundamental theorem of arithmetic, or even the existence of irrationals - Weber is forced to additionally postulate a least number principle, complete induction, and the impossibility of infinite descent, none of which appear to be provable in the naive set theory (p.210).[45] So, at least for now, the set-theoretic reductionist dream has to be put on hold.

Summary

It should be clear by now that there are plenty of ways to tackle the problem of naive set theory, and no unique way to assess them. Focusing on different aspects will call for different methods, which in turn will generate different theories. If our main priority is classical recapture, then shrieking and adaptive approaches will get it easily enough, but both rely heavily on non-logical principles; if we want to find complex naive universes, Libert's models look promising, but they cannot satisfy fully unrestricted comprehension; if we want to explain what exactly is going wrong with naive classical reasoning, NNST has us covered, but it involves a reformation of our notion of proof; if we want to be dialetheist all the way down and still preserve deductive power, Weber's approach appears to be pretty much the only option on the market, but classical mathematics is left behind.

Naive set theory is but a piece of inconsistent mathematics, and maybe not its best representative: its extreme generality seems to inevitably come with a lot of distracting fluff (e.g. Curry sets) taking most of the technical effort hostage. Even if for some reason we want to preserve the reductionist dream, this line of research may end up being more fruitful once we actually have enough data on the kind of inconsistent mathematics that we might want (or be able) to recover; and it may be easier to collect such data if we try and forget about Naive Comprehension altogether.

In the next sections I will look at some work that still goes under the name of inconsistent mathematics, but forgoes all foundational worries and can therefore focus on other values.

3.2 Adding inconsistent objects

Talk of inconsistent mathematics evokes questions like: What if x could both belong and not belong to y? What if a was both equal and not equal to b?

[45] These are all classically equivalent to the induction axiom.

One option might be to interpret our theory of interest into a naive set theory and try to derive some answers from there; but the most direct approach is to start from our (classical) theory, introduce the possibility of inconsistency, maybe locally axiomatize their occurrence, and see what happens. This would also be more in tune with common mathematical practice: foundational theories are not meant to dictate which particular structures are legitimate and which aren't, but rather provide an environment where such structures can be embedded, compared, and checked for consistency (or maybe nontriviality). At least in contemporary practice, it is the particulars that determine the foundation, not the other way around.

This kind of approach is best exemplified in Florencio G. Asenjo's *antinomic mathematics*, which studies the consequences of allowing certain basic mathematical predicates to both hold true and hold false of the same entities. Both the predicates and the entities in question are then called *antinomic*. There is no pre-theoretic inconsistent subject matter driving the investigation, no intuitive concept; rather, antinomic predicates are simply postulated in, and their study is justified on grounds of mathematical curiosity. The hope is that introducing antinomicity in familiar contexts may allow for interesting structures and fruitful distinctions to come to light.

Consider for example the **A3**-based antinomic number theory of [Asenjo, 1989]. The central idea is to introduce two unary predicates in order to distinguish *ordinary* numbers, which satisfy the usual axioms of arithmetic, from *antinomic* numbers, which satisfy different axioms imbuing them with contradictory-looking properties. It follows from Asenjo's axioms that antinomic numbers are *bilocated*: each antinomic number is both on the left and on the right of $\mathbb{N}$. More generally, every model of the axioms consists of a model of PA together with two copies of the same unbounded chain of antinomic numbers, one on the left and one on the right. This leads to some fun properties: for example, every antinomic number is both greater and smaller than every number, and the order is dense insofar as, for every n_1, n_2 with n_1 antinomic, we can always find a (natural) number between n_1 and n_2. Unsurprisingly, antinomic numbers break some basic tools of number theory: for example, the lack of a first antinomic number breaks the method of infinite descent.[46] In fact, not much has been said on how we can actually *work* with these new entities, or what we should do with them. Until then, antinomic arithmetic remains more an extension of the *model theory* of arithmetic, rather than of arithmetic itself.[47]

[46] We already saw this loss of equivalence between infinite descent and induction in [Weber, 2021a, ch.6].

[47] The connection between the two is hardly a necessity: even classically, there is little overlap between work in arithmetic (in the usual sense) and work on nonstandard models of arithmetic.

The study of antinomic set theory in [Asenjo, 1996] takes a different turn.[48] What really sets it aside from other work in the field is the refusal to fix an underlying logic governing negation. The classical positive propositional fragment is accepted, but there are no logical laws or axioms involving negation: thus, negative formulas - whether merely true or antinomic - are never derivable from positive formulas, and must rather *"be asserted as needed, not inferred, much as one chooses proper axioms for a given first-order theory"* (p.65). This follows from Asenjo's belief that *"it should be the mathematics that eventually determines the logic, rather than the other way around"* (p.55). Full formalization should eventually happen, but we cannot know the best way to formalize antinomies until we actually know how antinomic maths looks.[49]

Here is how the possibility space looks. LEM and its dual $\neg(A \wedge \neg A)$ are said to hold for the metalinguistic "not" but not for the object-language negation: the goal of these conditions is to make sure that a theory can countenance antinomic formulas, while at the same time making it determined and consistent whether a certain formula belongs to a theory or not. Furthermore, untruth and falsity are distinct and independent. This means that for every formula ϕ there are four exclusive and exhaustive options:

1. $\models \phi$ and $\not\dashv \phi$, i.e. ϕ is true and not false;
2. $\models \phi$, and $\dashv \phi$ i.e. ϕ is true and false;
3. $\not\models \phi$, and $\dashv \phi$ i.e. ϕ is false and not true;
4. $\not\models \phi$, and $\not\dashv \phi$ i.e. ϕ is not true and not false.

Since object-language negation is in no way constrained, the same four options are in principle open for $\sim \phi$, regardless of what is the case for ϕ. Concerning the two classical quantifiers $\forall$ and $\exists$, either can be meaningfully taken as positive, although the logics generated in the two cases are different; either way, classical duality is not a given.

Asenjo presents three different set theories taking different antinomic predicates as primitive, namely membership, inclusion, and union; here I am going to focus on the first option. In this theory, called AS1, ϵ denotes the primitive binary

[48] [Asenjo and Tamburino, 1975] already took a shot at paraconsistent set theory by grounding it in **A3**. This version countenances a very mild form of Naive Comprehension, with strong restrictions on the syntax of defining formulas; however, [Asenjo, 1989] notes that formulation was mistaken.

[49] This agnosticism about negation is arguably captured by the logic **CLoN**, on which see e.g. [Batens et al., 1999]. However, note that Asenjo only takes this attitude to be temporary: the idea is that the mathematics will eventually tell us how we should extend the logic.

membership predicate, and for all sets x, y there are three possible exclusive cases (each with its unique primitive notation):

1. $x\epsilon\!\notin\! y$, i.e. $\models x\epsilon y, \models x\notin y, \Dashv x\epsilon y$ (antinomic membership);
2. $x \in y$, i.e. $\models x\epsilon y, \not\models x\notin y, \not\Dashv x\epsilon y$ (classical membership);
3. $x \notin y$, i.e. $\models x\notin y, \not\models x\epsilon y, \not\Dashv x\notin y$ (classical not-membership).

These cases are taken to be exhaustive, thus eliminating the possibility that neither $x\epsilon y$ nor $x\notin y$ are true. Note that each case contains at least one unknown concerning falsity: for example, $x \in y$ is compatible with both falsity and not-falsity of $x \notin y$.[50] The antinomicity of the primitive predicate spreads to all the defined predicates, and therefore to the whole of set theory: thus we have several more or less antinomic variants of inclusion, power sets, etc.

The axioms and definitions of AS1 include most of the usual ones from ZFC, although antinomicity introduces many ambiguities that need resolving: for example, the Power Set axiom will require choosing one among many possible power set notions, and so on. There is also a completeness axiom making sure that what is true in the theory is exactly what can be derived in the theory: this needs to be an axiom because there is no formal system underlying the negative fragment of the theory. *"For positive formulas in AS1 the only change with respect to the classical situation is the addition of semantic antinomicity in some cases. For negative formulas in AS1 the application of [the Completeness Axiom] is ad hoc and goes from semantics to syntax. Again, the positive diagram of a given model of AS1 is predetermined by the axioms. The negative diagram, i.e., the collection of all negative formulas true or antinomic in such a model, remains incomplete and open to successive additions"* (p.75).

The advantage of an antinomic approach is cashed out in terms of new conceptual possibilities. Asenjo's main example is the failure of certain consequences or characterizations of the axiom of choice (AC). Let an *inductive* set be one that is counted by an initial segment of the natural numbers; let a *reflexive* set be one that is equinumerous to a proper subset of itself; and let a *mediate* set be a noninductive nonreflexive set.[51] Classically, under AC there is no such thing

[50]The asymmetry between truth and falsity here is only a matter of exposition: we are describing the three cases in terms of their truth conditions rather than their falsity conditions. There is however a false notational symmetry when it comes to antinomic membership: the symbol $\epsilon \notin$ appears to put ϵ and $\notin$ on the same level, yet there is in fact a difference between antinomic membership and antinomic *not*-membership (which would presumably require $\Dashv x\notin y$ instead of $\Dashv x\epsilon y$).

[51]None of this is common terminology today. The reader is advised not to carry these definitions outside of this paragraph.

as a mediate set, because inductivity turns out to be equivalent to nonreflexivity; antinomically, however, the possibility reopens since a set can be both mediate and nonmediate. Similarly, AS1 allows for the existence of *amorphous* sets, i.e. sets which are noninductive but not the union of two disjoint noninductive sets - another classical impossibility. The upshot is that there are many potentially fruitful notions which AC forcefully collapses together in a classical framework, and AS1 lets us study them in their distinctness without having to sacrifice such a basic operation like choice: *"Indeed, choice is as indispensable from a mathematical point of view as the equally primitive operation of comprehension"* (p.91). The argument is analogous to that commonly heard against the famously unintuitive equivalences of AC: *"it seems rather forced to extrapolate the well-ordering principle from the set of natural numbers to all unimaginable sets simply to be able to single out a definite individual from every nonempty set. And it seems just as forced to identify infinity with [reflexiveness] since, for example, it is shortsighted to assume that nonfinite nonreflexive sets are useless because we have not yet found any use for them"* (pp.90-91).

What to make of all this? The observation that classical mathematics unjustifiably collapses together substantially different concepts goes back at least to [Brouwer, 1913], who for example took the Continuum Hypothesis to be a badly formed hypothesis that could be split, via disambiguation in an intuitionistic framework, into easily solvable questions.[52] Antinomic mathematics, however, does not put itself in opposition to classical mathematics, but rather seeks to supplement it. In this sense, there is no risk of losing what is almost universally taken to be valuable mathematical information, e.g. the (classical) equivalence between AC and the well-ordering principle. Antinomic mathematics is not meant to point out any *mistakes* in classical mathematics: rather, it allows us to say even more things by drawing even more distinctions, which can then be collapsed back to classicality via postulation at any moment if necessary. Of course, one has to show that these novelties are mathematically fruitful: Asenjo would presumably agree with this, his papers merely intending to show the way.

While Asenjo focused on distinctions that antinomies introduce between *classical* notions, there can also be the hope that allowing for antinomies might allow for distinctions between inconsistent entities themselves. For example, ZFC has no tools for distinguishing between two sets whose existence is rejected by the theory itself. A naive set theory might do the job, but that brings a lot of trouble of its own, and can be a bit overkill: we may just want *some* inconsistent sets, rather

[52]The Continuum Hypothesis, or CH, states that there are no intermediate cardinalities between the size of the naturals and the size of the reals. It was shown by Gödel to not be provable in ZFC, and by Paul Cohen to not be disprovable in ZFC. See e.g. [Cohen, 2008].

than *all* of them, and we may want to study their interaction independently of the much stronger assumption of Naive Comprehension. As an example of this kind of approach, [Arruda and Batens, 1982] showed that, under very minimal logical and set-theoretic assumptions, the union of the Russell set just *is* the universal set. Going formalism free, one might gloss this as the theorem that every set-theoretic universe containing the Russell set must also contain the universal set.[53] Note that this kind of result would be trivialized by the assumption of Naive Comprehension just as much as by the assumption of consistency!

It is not hard to add inconsistent sets systematically, yet with no appeal to Naive Comprehension: simply cast the ZFC axioms (or some appropriate variant of them) in a paraconsistent logic, and add a postulate stating the existence of at least *one* generic inconsistent set. Depending on how the inconsistency is defined, and how the usual set-theoretic operations interact with it, this might suffice to make inconsistent sets appear all over the universe, for example if the union of a consistent set with an inconsistent set is always a (different) inconsistent set.

This strategy was applied by [Oddsson, 2021] using the logic **BS4**, and it does in fact results in an inconsistent universe extending the classical one. Since **BS4** has a consistency operator $\circ$, there is a pretty straightforward way to define inconsistent sets: they are simply the sets u for which it is not the case that $\forall x[\circ(x \in u)]$.[54] The theory in question has a model in ZFC, and there is a clear sense in which inconsistent sets can be represented as pairs of classical sets. However, this needs not undermine their inconsistency status. The same thing happens in Aczel's non-well-founded set theory: it is proven in [Aczel, 1988, ch.3] that the new sets can be represented by equivalence classes of (consistent) graphs, but we still call them non-well-founded.

3.3 Inconsistent nonstandard models

While set theory is taken by many inconsistent mathematicians as the paradigm of a branch that historically has been done a disservice by focusing on consistency, no such argument is usually carried out for arithmetic. No one is really questioning the validity of the Peano axioms, and it is generally accepted that the natural

[53] The result applies just as well to the hierarchy of paraconsistent set theories in [Carnielli and Coniglio, 2016, ch.7]. The reason they can still conjecture Russell sets to be admissible in such systems despite the universal set leading to triviality is that by "Russell sets" they mean sets x such that $x \in x \wedge x \notin x$.

[54] **BS4** was defined in Section 2.2. Oddsson's theory also has *incomplete* sets, which are defined through a *completeness* operator; however the existence of inconsistent and incomplete sets is postulated independently, and either can be removed.

numbers are, in fact, perfectly consistent.[55] Therefore, inconsistent arithmetic either involves the *addition* of inconsistent numbers - as in Asenjo's antinomic arithmetic - or the study of inconsistent models of a *consistent* theory in the vicinity of Peano Arithmetic. In this section I will look at the latter approach.

Unlike PA, relevant arithmetic $\mathbf{R}^\sharp$ supports legitimately inconsistent models, in the sense of models where some formula and its negation can both be true. It all started with [Meyer, 1976] presenting the following **RM3**-model for $\mathbf{R}^\sharp$:[56]

- the domain consists of the integers modulo 2, which we can represent as $\{0, 1\}$, with the usual operations;
- $0 = 0$ and $1 = 1$ are both true and false;
- $0 = 1$ and $1 = 0$ are just false.

Since the model is non-trivial, this seems to show that $\mathbf{R}^\sharp$ is non-trivial; furthermore, since the model is finite, this constitutes a finitary nontriviality proof for arithmetic.

Inconsistent models were originally a mere tool to prove properties of consistent systems, rather than an object of investigation themselves. In the following decade, the focus shifted from the axiomatic arithmetic to the inconsistent models themselves and their theories. For example, [Meyer and Mortensen, 1984] noted that the above model construction could be generalized in two directions: first, by taking as domain the integers modulo m, and second by using the logics **RM**n.[57] The resulting model is not necessarily unique, but for whatever choice, the intersection of the theories of all these models is decidable and inconsistent, and an interesting case study of how classically equivalent logical notions can come apart in new logical contexts. For example:

- It is incomplete, but nevertheless extensionally complete (i.e. complete w.r.t. $\rightarrow$-free formulas).[58]

[55]There are exceptions. Edward Nelson famously claimed in 2011 to have discovered a proof of the inconsistency of PA; even considering later updates, nowadays it is generally agreed that said proof contains mistakes that cannot be fixed (see the introduction to [Nelson, 2015]). Less dramatically, [Priest, 1994] argues that some finite inconsistent models of arithmetic appear to be at least as believable as the standard one. Finally, as discussed in Section 1.5, [Van Bendegem, 1994] argues that every mathematical theory with infinite models - including arithmetic - should have a correct finite inconsistent model.

[56]Recall that **RM3** extends **R**, the logic on which $\mathbf{R}^\sharp$ is based on.

[57]**RM**n is the generalization of **RM3** to n linearly ordered truth-values (with n odd), with the upper half being designated. $\wedge$ and $\vee$ are min and max respectively; $\neg A$ is the opposite point on the order; $A \rightarrow B := \neg A \vee B$ if $A \leq B$, and $\neg(A \vee \neg B)$ otherwise.

[58]Curiously, this is already the case for the intersection of all theories of **RM3**-models on the integers modulo m.

- It is ω-complete (i.e. if Fn holds for every n then so does $\forall x Fx$), yet fails to be E-complete (i.e. $\exists x Fx$ may hold despite Fn holding for no n).

These first investigations rarely left the model-theoretic stage. [Mortensen, 1988] suggests that this is *"a consequence of the fact that intuitive inconsistent thinking is undeveloped (though not entirely absent) among mathematicians and logicians. [...] in its absence it is necessary to demonstrate that control of the deductive consequences of contradictions is possible"* (p.46). A strong connection is conjectured to exist between the models and informal mathematics: according to Mortensen, this study *"can usefully be viewed as dealing with mathematical objects which have inconsistent properties especially when models which inconsistently extend various consistent classical standard theories of classes of mathematical objects are considered"* (p.50). However this is rarely cashed in, and it is certainly not obvious *how* to cash it in, as always when moving from the formal to the informal, and especially in a nonclassical context.

One potential application concerns the use of these new models to prove or refute certain classical conjectures. Consider the model NSN from [Mortensen, 1987], obtained by adjoining to the natural numbers the set of all numbers between m and $2m-1$ for some m nonstandard, with self-identities of nonstandard numbers being all false (and true). The sum is the usual one on the standard fragment, but if either n_1 or n_2 is nonstandard then $n_1+n_2 = m+(n_1+n_2 \mod m)$; similarly for the product. NSN contains a counterexample to Fermat's Last Theorem, namely the least inconsistent number m: in fact, $m^3+m^3 = m^3$.[59] This is potentially interesting because the truth value of the theorem in PA can be reduced to its truth value in certain inconsistent models. Unfortunately in this case the reduction does not appear to simplify the problem, and to my knowledge this kind of strategy was never explored further.[60]

An attempt to focus on the change of perspective that the new models can offer was made in [Meyer and Mortensen, 1987]. Most notably, the paper presents a model of arithmetic which encompasses something resembling the rational numbers ("alien intruders"). This certainly raises some intriguing questions: for example, how can the induction axiom possibly hold on a model containing the rationals, given their density? Recently, [Ferguson and Ramirez-Camara, 2021] have provided a more insightful construction of such a model as an ultraproduct

[59]Fermat's Last Theorem states that there are no $a, b, c > 0$ such that $a^n + b^n = c^n$ for some $n > 2$. It was finally proven in 1994.

[60]For a bit of irony, [Friedman and Meyer, 1992] presented a devastating application of *classical* models to an open problem in relevant mathematics: classical recapture of PA was shown to fail by exhibiting the complex numbers as a classical model of the positive fragment of $\mathbf{R}^\sharp$, and showing that a classical theorem fails there (this suffices because $\mathbf{R}^\sharp$ is conservative over its positive fragment, i.e. introducing negation adds no negationless theorems).

of finite **RM3**-models.[61] This has the effect of demystifying these pseudo-rational numbers by reducing them to sequences of natural numbers: for example, it is possible to carry induction on the "rationals" *because* it is possible to carry it on their components.

In the '90s, the focus once again shifted, this time from models of relevant arithmetic to **LP**-models of arithmetic.[62] A complete classification of the finite **LP**-models of arithmetic is given in [Paris and Pathmanathan, 2006]. Given an element i in such a model $\mathcal{M}$, we say that:

- the *nucleus* $N(i)$ is the set $\{x \in \mathcal{M} : i \leq x \leq i\}$;
- a *period* of $N(i)$ is any $p \in \mathcal{M}$ such that $i + p = i$;[63]
- a nucleus is *proper* if it is not a singleton.

Classically, there are no proper nuclei and the only period is 0. But in **LP** there are two new possibilities: *cyclic models* are just proper nuclei containing 0, while *heap models* consist of a linear sequence of improper nuclei (i.e. a classical initial segment) followed by a cycle.[64] Heaps terminating in 1-cycles were one of the original motivations in studying finite **LP**-models: the idea in [Van Bendegem, 1994] and [Priest, 1994] was that the first inconsistent number may be conceptualized as the largest number.[65]

When it comes to infinite **LP**-models of arithmetic, things get a lot more complicated. As [Paris and Sirokofskich, 2008] show, there is a model with a proper nucleus not closed under sum; there is a model with a nucleus having an infinitely descending sequence of periods; and there are models which cannot be obtained by enlarging the anti-extension of identity in the collapse of a classical model.[66] Still, some of these infinite models are actually *decidable*, much like their finite cousins. A most bizarre example is the model defined as follows:

[61] For details on nonclassical ultraproducts, see [Ferguson, 2012].

[62] Although not entirely: see e.g. [Slaney, 2022].

[63] Note that the period of a nucleus N is not necessarily unique, nor does it depend on the choice of $i \in N$.

[64] Cyclic models are just the finite **RM3**-models of $\mathbf{R}^\sharp$ discussed earlier. Both finite cyclic and heap models were shown to be axiomatizable in **A3** by [Tedder, 2015], although there is a slight oversight in the proposed axiomatization of the latter: namely, the axiom $x' \neq y' \rightarrow x \neq y$ needs to be dropped (since the heap model with a cycle starting at m' falsifies $m' \neq m' \rightarrow m \neq m$). This also shows finite heap models are not **RM3**-models of $\mathbf{R}^\sharp$; which is to be expected, since if they were they would finitarily show the consistency of $\mathbf{R}^\sharp$ by not validating $0 \neq 0$!

[65] It was also suggested by [Mortensen, 1997] that they could provide a model for Penrose's tribar, which I will discuss in Section 3.6.

[66] It is said to be nevertheless the case that every **LP**-model can be so obtained from the collapse of a *substructure* of a classical model.

- Take any countable nonstandard classical model $\mathcal{M}$. This can be seen as the union of initial segments C_i closed under successor and product, with $C_0 := \{0\}$.[67] For all $a, b \in \mathcal{M}$ let $a \sim b$ iff $a = b = 0$ or $a, b \in C_{j+1}/C_j$. The domain of our model is the set of $\sim$-equivalence classes.

- The successor function $'$ is reflexive everywhere except on 0, while $0' = a \in C_1/C_0$.

- $+$ is the max function.

- $\cdot$ is the (classical) sum, but with 0 as a multiplicative identity.

This model is as close as trivial as one can get: *every* true identity is contradictory except $0 = 0$.[68]

Going in a different direction, inconsistent models of arithmetic based on a *contraclassical* logic were presented in [Ferguson, 2019a]. This time the underlying logic is the connexive logic **C**, which can be obtained from **N4** simply by modifying the falsity condition for the conditional: in a standard axiomatization, this means replacing $\sim (A \to B) \leftrightarrow (A \wedge \sim B)$ with $\sim (A \to B) \leftrightarrow (A \to \sim B)$. While **N4** is an uncontroversial subclassical logic, **C** is not only contraclassical but also inconsistent: for example, both $(A \wedge \sim A) \to A$ and its negation are theorems. This means that every **C**-model of arithmetic - no matter how we formulate the axioms - is an inconsistent model.

Now, the finite heap **LP**-models of arithmetic are also models of arithmetic based on **C** or **N4** (call these **C**$^\sharp$ and **N4**$^\sharp$ respectively) - or, to be more precise, there are models of **C**$^\sharp$ and **N4**$^\sharp$ with the same domain and the same interpretation of terms and atomic formulas as the finite heap **LP**-models. Such **C**$^\sharp$-models and **N4**$^\sharp$-models will disagree *only* on which implications are made false from the interpretation. It might be tempting to conclude that there are no *mathematical* differences between these models regardless of the underlying logic; the difference remains confined to the logical level, where it may not have much of an effect on the mathematics. Of course, we can always substitute mathematical terms into, say, $\sim (A \to \sim A)$, which holds in **C** but not in **N4**. But I would be hard-pressed to say that the theorem $\sim (2 + 2 = 5 \to 2 + 2 \neq 5)$ tells us anything about *numbers* in particular, at least without an argument to the effect that **C** is a logic good *specifically* for arithmetic. This, in turn, raises the question of whether we are really

[67] For example, take any end segment of nonstandard numbers $a_1 < a_2 < \ldots$ and let $C_i := \{b \in \mathcal{M} : b < a_i^n, n \in \mathbb{N}\}$. All of these are proper segments because $a_i^{a_i} > a_i^n$ for every n.

[68] It is worth noting that not every inconsistent mathematician was happy with all this weirdness: in very much the opposite direction, [Vermeir, 1999] offers an inconsistency-adaptive arithmetic based on **CLuNs**, whose goals was rather to *restrict* the space of models by identifying the minimally abnormal ones.

talking about inconsistent mathematics, if the inconsistency is not mathematical in nature and may fail to lead to any specifically mathematical results.[69]

To conclude this section, I want to briefly discuss a recent trend of studying inconsistent nonstandard models of ZF.[70] We already saw that the Collapsing Lemma is one way to build such models; but there is another popular approach, which generalizes the classical construction of *Boolean-valued models* of ZFC. The original construction goes like this. Given any Boolean algebra B, the structure V^B is the limit of the following hierarchy:

- $V_0^B = \emptyset$;
- $V_{\alpha+1}^B = \{f : X \to B,\ X \subseteq V_\alpha^B\}$;
- $V_\lambda^B = \bigcup_{\alpha<\lambda} V_\alpha$ for λ limit.

We then get a model of ZFC by inductively interpreting identity and membership as follows:

$$[u \in v] = \bigvee_{x \in \mathrm{dom}(v)} ([x = u] \wedge v(x))$$

$$[u = v] = \bigwedge_{x \in \mathrm{dom}(u)} (u(x) \Rightarrow [x \in v]) \wedge \bigwedge_{x \in \mathrm{dom}(v)} (v(x) \Rightarrow [x \in u])$$

and letting the top element $1 \in B$ be the only designated truth-value, meaning that $V^B \models \phi$ if and only if $[\phi] = 1$.[71]

Now, Boolean-valued models are *classical* models. They satisfy the ZFC axioms, and all of their classical consequences. However, the construction can be generalized. For example, if we take Heyting algebras instead of Boolean algebras, the procedure generates models of intuitionistic ZF instead.[72] More importantly for us, [Löwe and Tarafder, 2015] show that so-called *reasonable deductive algebras* with the *NFF-bounded quantification property* generate models of the negation-free fragment of ZF (call this NFF-ZF) without Foundation,[73] where an algebra is

[69]The matter is not restricted to finite models either: Ferguson also produces a class of infinite structures that model both $\mathbf{N4}^\sharp$ and $\mathbf{C}^\sharp$ (in the same sense as before). Both logics come with a possible world semantics similar to that of intuitionistic logic; roughly, the idea behind Ferguson's models is to take any infinite collection of distinct finite n-cyclic models to serve as possible worlds w_n, and give them the ordering $w_n \leq w_m$ iff m divides n.

[70]ZF is ZFC without the Axiom of Choice.

[71]The recursive clauses for non-atomic formulas are the natural ones: each connective corresponds to an operation on the algebra, and quantifiers are interpreted as generalized disjunction/conjunction. See [Bell, 2011, ch.1] for more details and proofs.

[72]See [Bell, 2011, ch.8].

[73]All of the ZF axioms have negation-free formulations, in the sense that the only negations occur in particular instances of the Separation and Replacement schemas. Classically, NFF-ZF is equivalent to ZF.

reasonable deductive if the following conditions hold:

- $x \wedge y \leq z$ implies $x \leq y \Rightarrow z$;
- $y \leq z$ implies $x \Rightarrow y \leq x \Rightarrow z$ and $y \Rightarrow x \leq z \Rightarrow x$;
- $(x \wedge y) \Rightarrow z = (x \Rightarrow (y \Rightarrow z))$;

and the NFF-bounded quantification property is

$$[\forall x \in u\, \phi(x)] = \bigwedge_{x \in \mathrm{dom}(u)} (u(x) \Rightarrow [\phi(x)]).$$

for every negation-free ϕ. Using this technique, the authors are able to construct an inconsistent model of NFF-ZF: in particular, the sentence $\exists u \exists v \exists w (u = v \wedge w \in u \wedge w \notin v)$ is both true and false. However, the model does not satisfy Naive Comprehension: in fact, it does not even contain the Russell set.[74]

Building on these ideas, a whole class of algebras delivering inconsistent models of various paraconsistent ZF theories (i.e. of the ZF axioms together with their consequences under some paraconsistent logic) was obtained in [Jockwich-Martinez et al., 2021]; more pointedly, [Jockwich-Martinez, 2021] showed that there is a 3-valued algebra providing inconsistent **LP**-models of the ZFC axioms.[75] It is said that a big advantage of these models is that they satisfy Leibniz's Law, i.e. Restall's $(= l)$; but there is a price to pay, namely the loss of the vast majority of classical theorems.

These algebra-valued models are maybe the most striking example of how the iterative conception needs not be sacrificed for the sake of inconsistent sets. Not only is Naive Comprehension not at all a consideration; not only do even the most basic naive sets fail to make an appearance; but every model clearly mirrors the cumulative hierarchy. That being said, the motivation behind their study remains somewhat mysterious: no canonical model has emerged yet, and it is as yet unclear whether these inconsistent models can contribute to the development of ZFC in the way that Boolean-valued models did.

[74] The underlying logic, which they call $\mathbf{PS}_3$, is **LP** with the following conditional: $a \Rightarrow b$ is false if b is false and a is not, and true otherwise (never both true and false). I am not aware of any attempt to make sense of it, although a technical investigation can be found in [Tarafder and Chakraborty, 2015]. A generalization of this approach, using *twist-valued* models, can be found in [Carnielli and Coniglio, 2021].

[75] Both results require modifying the interpretation of identity, albeit in ways compatible with the classical case.

3.4 What follows from the false

The original motivation for $\mathbf{R}^\sharp$ was not only to provide a more accurate formalization of standard arithmetical practice, but also to *expand* the scope of mathematics by allowing for the exploration of what-ifs that classical mathematics rejects by default. *"If we are to think relevantly about mathematics, what is to be hoped for most of all are not new routes to old truths but an expansion of the pragmatic imagination. Let us be free to wonder what it would be like if* 0 *were equal to* 2*, and let us not be stopped short by our conviction that* 0 *isn't* $2''$ [Meyer, 2021a, p.158]. The material conditional trivializes such wondering by making everything follow from a falsehood. The relevant conditional is what makes it possible to even ask the question, which can be formalized as: for which A is it the case that $0 = 2 \to A$?

Now, merely "wondering" what would happen if $0 = 2$ does not suffice to generate inconsistent mathematics unless we simultaneously hold $0 \neq 2$ true within the wondering; but this is exactly how $\mathbf{R}^\sharp$ functions, since $0 \neq 2$ is a *theorem*. Every time we are assuming a falsehood we are picturing a set of possible arithmetics different from the true one, as made particularly clear from the Routley-Meyer semantics; and in *those* worlds $0 \neq 2$ may not hold. But this is not to say that we reject $0 \neq 2$. On the contrary, alternatives are conceived in relation to true arithmetic, which in turn influences their structure by imposing itself as the point of reference. If we picked a different starting point, fixed a different actual world (or rather, different normal worlds), then the space of possibilities would itself be different. To make sense of $\mathbf{R}^\sharp$ as an extension of scope, we need to look at the whole universe of possible arithmetics *as shaped* by the actual truth of $\mathbf{R}^\sharp$.

If this picture sounds intriguing, I should point out that the choice of semantics is really important to make sense of this. Compare the 3-valued semantics of **RM3**, where $0 \neq 2$ holds in every model of $\mathbf{R}^\sharp$ (sometimes together with its negation, of course), and the Routley-Meyer semantics of **R**, where $0 \neq 2$ may fail in some worlds. Of course, even in the Routley-Meyer semantics $0 \neq 2$ holds in every *model*, because that is just what theoremhood means; my point is that the 3-valued semantics hides the intended - or at least, the coolest - meaning of $\mathbf{R}^\sharp$ by turning true arithmetic from a *perspective* on other possible arithmetical worlds to a common *content* for all of them.

A fascinating feature of this picture is that it is not really clear what it even means for something to follow or not from $0 = 2$. It certainly does not help to think of what follows from $0 = 2 \wedge 0 \neq 2$; if anything, this kind of framing only hides the asymmetry between what is accepted as true and what is imagined to be true. As a very broad guideline, a minimal mutilation principle suggests itself: each assumed falsehood should lead to as few falsehoods as possible, so that the result can be as similar to true arithmetic as it can. But this line of thinking only gets us so far, since some falsehoods are allowed to wreak havoc: for example, $0 = 1$ implies every positive formula in $\mathbf{R}^\sharp$.[76]

One could say that only *relevant* consequences should count. For example, [Sylvan, 2019] shuns $m = m \leftrightarrow n = n$ (which holds in $\mathbf{R}^\sharp$) because it is not a *"correct entailment principle"* (p.65), by which he means that the truth of $m = m$ is not *sufficient* for the truth of $n = n$.[77] But this is one explication of relevance amongst many, and for any such explication there may be many adequate logics.[78] [Estrada-Gonzalez and Tapia-Navarro, 2021] tentatively propose, as a more definite criterion, that only consequences sharing some *weak q-content* should be accepted, where the weak q-content of a sentence is understood as the set of terms and relevant predicates occurring in it. This seems to justify the theorem above, since both sides involve identity. The downside of such a syntactic approach is the extreme sensitivity to language manipulation: should the set of valid implications really change depending on who the primitive terms of the theory are?[79]

The appeal to some notion of relevance to explain arithmetical implication is itself controversial, even in the context of relevant logics. For example, [Mortensen, 2019] charges Sylvan with a confusion between entailment and implication. *"A true implication does not need Anderson and Belnap's meaning-overlap restriction: the relevance of a logic is a matter of its logical theorems satisfying meaning-overlap. Perhaps [Sylvan] was thinking that [$m = m \leftrightarrow n = n$] suffers from irrelevance; but irrelevance is a property of the theorems and deducibility of logics, not of the nonlogical theorems of arithmetical theories"* (p.189).

Suppose we buy Mortensen's line, and take relevance considerations to be unrelated to the truth of nonlogical theorems in relevant arithmetic. What *does* then determine the truth of such theorems? Answering "the axioms and the logic" just pushes the question back to what determines the truth and adequacy of those. The only positive argument from Mortensen concerns the fact that implications expressing functionality are well and good; this, in turn, seems to stern from his characterization of functionality as a minimal requirement for meaningful

[76] See [Meyer, 2021b, p.336].

[77] Sylvan is arguing in favor of a **DKQ**-based arithmetic, DKA. This is not merely a matter of logic choice, but also of axiom choice: for example, he suggests replacing $x = y \to x' = y'$ with $x = y \wedge 1 = 1 \to x' = y'$.

[78] [Standefer, 2022] provides a nice survey.

[79] This is particularly worrying in cases of theory reduction. In particular, assuming that mathematics can be formally reduced to set theory, it seems that *any* two mathematical sentences should be relevant to each other in virtue of both involving the membership relation.

computation. It seems unlikely, however, that such pragmatic considerations would suffice for settling the truth of relevant implications in arbitrary arithmetical contexts. One might take a very liberal view of things here, and accept that it is okay to postulate truth as we see fit; but the worry remains that having to resort to this indicates a failure to fully understand relevant implication in mathematical contexts.[80] Still, Mortensen's approach may be thought of as a search strategy: one pragmatic consideration after the other, we may eventually gather enough data to formulate a plausible theory of how relevant implication should behave in mathematics.[81]

I have no such theory, though I do have more examples. Weber's work on analysis and topology, while hardly in the Meyer tradition, feels quite open to the same reading. Consider these two theorems from [Weber, 2021a, ch.8,9]:

Theorem 1. *Let $f : [0,1] \to \{0,1\}$ be a continuous surjection. Then:*

1. *There is $X \subseteq [0,1]$ such that $[0,1] \subseteq f[X]$.*
2. *If there is $p \in [0,1]$ such that $0, 1 \in f(p)$, then $p \in f(p)$.*

Theorem 2. *Let $D \subset \mathbb{R}^2$ be closed connected. Every continuous function $f : D \to D$ sending closed sets to clopen sets has a fixed point.*

In both theorems, the hypotheses are nonsense from a classical perspective: there are no such functions. In a sense, this makes the theorems classically true: there is no f making the antecedent true,[82] so the (material) implication is true. On the other hand, Weber is working in naive set theory, where basically everything exists: so the theorems do indeed express derived properties of mathematical entities.

And yet, I think that the best way to understand these theorems is *not* as consequences of a given axiomatic naive set theory. This is not to say that their proofs are invalid, or that **subDLQ**-based set theory was not the best tool for discovering them; my point is merely that framing things that way makes these theorems unbearably mysterious to anyone who is not familiar with the details of this particular system, and unacceptable to anyone who does not commit to everything in it. A better option might be to see these theorems as a scope expansion: a possible explication (by example) of what it means to follow from the

[80] I do not mean to suggest the existence of a single correct relevant conditional lurking in the Leibnizian shadows of a perfect language. I am merely saying that more should be said about what implication is supposed to mean or do.

[81] This is basically how Asenjo deals with negation in his antinomic set theory. It is also how [Maddy, 2011, ch.2] suggests axiom choice works in contemporary set theory.

[82] Unless D is a singleton.

false in a mathematical context, to be superposed to the classical results regardless of whether the latter can be recovered with the same tools. Only as a formality does this contradict classical mathematics; after all, classical mathematics is not asking the question, which is precisely why it considers classical logic an adequate enough formalization despite trivializing the answer.

Now, a possible objection might be that the result cannot be meaningfully extricated from the underlying framework. Changes in logic all too easily lead to changes in the mathematics they underlie; we may have as many explications as logics, so the logic should be kept in mind. At the level of complexity achieved by these theorems this is somewhat conjectural, since Weber's work is currently the only offer on the market, but let us entertain the point anyway. As I discussed in Section 2.9, it is part of the logician's work to make their results as stable as possible across different formalisms: to provide some degree of formalism freeness, precisely so that we can throw the result back into the world of informal mathematics where things are far more flexible and understandable. I do not believe Weber's results are particularly fragile, simply because I tend to believe in the stability of any mathematical result on which enough informal thought has been spent - even if sometimes it can take a while to find out how to cash said stability in. However, if they *were* extremely fragile, if there was no way to make sense of them outside of this one particular logical and set-theoretic framework, this *would* count against them and suggest looking for a more stable formulation.

One final comment. Recall that Weber works in **subDLQ**, where there is in fact a conditional satisfying the deduction theorem, and it is *not* a relevant one. So these theorems would not be expressed formally with a relevant conditional as main connective. Thus, if my reading holds water, it may be another point against putting too much weight on relevance in explaining these implications.

3.5 Doing algebra with contradictions

As pointed out in [Mortensen, 1987] and [Mortensen, 1988], inconsistency becomes more dangerous the more algebraic structure we add. The so-called *Dunn-Mortensen problem* is that *any* inconsistent equation in a field (e.g. the real numbers) will lead, under some very basic logical assumptions, to triviality. The argument, as presented in [Weber, 2021a, ch.7], is as follows. If $a \neq b$, then $a - b \neq 0$, and so $\frac{a-b}{a-b} = 1$. But if we also have $a = b$, then $a - b = 0$, and so by substitution $\frac{a-b}{a-b} = \frac{0}{a-b} = 0$. By transitivity of identity, we get $0 = 1$. A similar issue is generated by the interaction between a group structure and an order: if we have $a < a$, then $a + b < a + b$ and $(a + b) - a < (a + b) - a$. In a few elementary steps this delivers $b < b$. So if one element is less than itself, then every element is. Inconsistency cannot be quarantined.

A few possible solutions can be found in the literature. Asenjo's antinomic arithmetic, which does stumble into the problem by enforcing a cancellation law for the sum, avoids complete trivialization by distinguishing between antinomic and "standard" numbers, so that properties of the former do not necessarily affect the latter. While it is in fact the case that *every* antinomic number is smaller than itself (not to mention smaller than every other number), the order on the standard numbers remains the usual one.

While looking for feasible models of real analysis, [Mortensen, 1990] dodges the issue entirely by working with a nilpotent quotient *ring* of hyperreal numbers.[83] The construction is as follows. Having fixed any infinitesimal δ, we have the following congruence on the ring of noninfinite hyperreals: $a \sim b$ iff $\frac{a-b}{\delta}$ is at most infinitesimal. The collapsed quotient ring is then a functional inconsistent **RM3**-structure. The resulting theory shares some features with synthetic differential geometry: nilpotent elements can be used to simplify calculations, and all functions turn out to be continuous.[84] A most notable difference is that the cancellation law for the product fails in Mortensen's model, which already suffices to solve the Dunn-Mortensen problem: one can no longer generally conclude $\frac{a-b}{a-b} = 1$ from $a - b = a - b \neq 0$. Furthermore, since transparency fails[85] and division is not a primitive sign in the language, the step from $a - b = 0$ to $\frac{a-b}{a-b} = \frac{0}{a-b}$ turns out to not be generally justified; and finally, given the existence of nilpotent elements, it is not always true that $\frac{0}{a-b} = 0$.

Interesting as their theories may be, neither Asenjo nor Mortensen really address the Dunn-Mortensen problem directly: Asenjo simply accepted the trivialization of the order on antinomic numbers, while Mortensen just gave up on having a field structure. But the Dunn-Mortensen problem is first and foremost about abstract algebra, about the *general* notions of field and ordered group, and a solution to the problem is one that explains how to allow inconsistency within such structures in a uniform way.

Enter [Weber, 2021a, ch.7], offering a general solution in the form of *relative*

[83] The difference between a ring and a field is that in a field every element except 0 has a multiplicative inverse: e.g. the integers are a ring but not a field, while the rationals are both. The field of hyperreals extends the reals with infinite elements which are greater than every real number, and infinitesimals which are smaller than every positive real number. A ring is *nilpotent* if it contains a nilpotent element, i.e. some nonzero x such that $x^n = 0$ for some n. Da Costa's paraconsistent calculus, which uses a different logic and is embedded within a paraconsistent set theory, goes a bit further and also drops part of the ring structure: see [da Costa, 2000], [Carvalho, 2004], and [D'Ottaviano and de Carvalho, 2005].

[84] On synthetic differential geometry, see [Kock, 2006].

[85] A transparent model is also suggested, but it has the unpleasant consequence that every term becomes non-self-identical.

or *intrinsic* zeros and units. The starting point is the following observation: *"it is the assumption $x + (-x) = 0$ that generates the spread. Squinting, this equation is analogous to an 'ex falso' condition, that $p\&\neg p \Leftrightarrow \bot$"* (p.226). Since the 'ex falso' condition is rejected, combining x and its opposite should not lead to an absolute zero, but rather to a relative zero that depends specifically on x, which we may call 0_x. So the cancellation law for, say, an additive group becomes the *definition* $0_x := x - x$; similarly, for multiplication we should have $1_x := \frac{x}{x}$.

Of course, this is just the general idea. Let us see one way to implement this in the case of groups; the strategy is perfectly analogous for more complicated structures like rings, fields, and vector spaces. Weber defines a (commutative) group as a structure $(G, *)$ where $*$ is a binary operation satisfying the following axioms:

G1 G is closed under $*$.

G2 $*$ is commutative and associative.

G3 There is an *absolute unit* $e \in G$ such that $a * e = a$ for all $a \in G$.

G4 For every $a \in G$ there are an *inverse* $-a \in G$ and a *relative unit* $e_A \in G$ such that $a * -a = e_A$.

By substitution, it follows immediately that cancellation holds in the restricted form: $a * c = b * c \Rightarrow a * e_c = b * e_c$. However, we cannot conclude $a = b$ *unless* we know that $e_c = e$, which need not be the case. It is easy to show that each relative unit is unique (for the same reason the absolute unit is); but there is no way to show that they coincide, even less that they coincide with e. So any sort of Dunn-Mortensen-style derivation is stopped dead in its tracks.

It is worth noting that no inconsistency has entered the stage yet. Weber's definition does not *entail* inconsistencies: it is merely such as to *allow* for their nontrivial presence, as relative units make sure they are going to stay somewhat isolated. This means in particular that the definition remains perfectly valid if we take the underlying logic to be classical, although of course the stronger the logic the easier it will be for additional axioms to turn a Weberian group into a classical one.

Now, an interesting observation here is that G4 is entirely notational: there is no restriction whatsoever on $-a$, so the existence of elements satisfying G4 follows trivially from G1 if we simply pick a random $-a$ and let e_a be $a * -a$.[86] Since G1-G3 are the axioms of a commutative monoid, one could be tempted to conclude

[86] Formally, making $-$ a function symbol in the language would at least cement the extra structure; but Weber's framework shuns forcing *consistent* functionality.

that Weber's groups *just are* classical monoids, albeit with some extra notation thrown in. This would be, however, somewhat misleading. Even if the starting axiomatization puts no constraints on $-a$ and e_A, it still sets them up for being constrained, and any nontrivial constraint will generate a structure that was largely invisible from the monoid perspective.

For example, Weber suggests two possible additional axioms:[87]

G5 $--a = a$.

G6 $-a * -b = -(a * b)$.

These obviously build on G4, and so we could not countenance them as an extra assumption on monoids unless G4 was given; but the natural motivation for G4 comes precisely from seeing the monoid as a group! To be sure, both G5 and G6 are still trivially satisfiable in every commutative monoid in the sense that it is easy to find an appropriate assignment of inverses and identities;[88] but not *every* assignment will do, and it is a genuine question to ask which ones will. So here we have an example of formalism free inconsistent mathematics - the definition does not really depend on Weber's choice of logic - naturally suggesting new avenues for *classical* research!

3.6 Modeling inconsistent phenomena

Geometry has been a great source of inspiration for inconsistent mathematics. To start from the more obvious connection, consider the phenomenon of *impossible pictures*, which [Mortensen, 2010] defines as *"real pictures whose content is of logically impossible or contradictory objects"* (p.69). This kind of subject matter sounds perfectly suited for inconsistent mathematics, and Mortensen took up the challenge.

Now, there have been classical treatments of impossible pictures: see [Cowan, 1974], [Francis, 1987], and [Penrose, 1992]. Mathematically speaking, there is nothing wrong with these works, and they certainly seem to capture the *form* of these pictures. Mortensen's approach differs insofar as his focus is fully representing the *content* of an impossible picture, which includes the impossibility that the mind perceives when confronted with such a picture. This is not to say that impossible pictures have anything to do with inconsistencies out there in the

[87] Which, of course, classically follow from the group axioms.

[88] Most boringly, let $-a = a$ and $e_a = a * a$. Slightly less trivially, take any classical group G and adjoin to it an element 0 such that $0 * x = x * 0 = 0$ for every x: this is a monoid and not a classical group (0 has no inverse), yet G5 and G6 can be satisfied by letting $-a$ be the (classical) inverse of a for every $a \in G$, and $-0 = 0$.

world; rather, the point is that the *appearance* of inconsistency is real, and a truly complete description should be able to explain it.

[Mortensen, 2010] discusses four kinds of impossible pictures: the Necker cube(s), the triangle, the stairs, and the fork. The reasons behind the perceived impossibility are varied, and are to be discovered through an empirical study of the way we cognize such pictures. Different causes for the impossibility will motivate different mathematical representations, which in turn require different logical analyses. For example, impossible forks are argued to be an example of cognitive sorites: in some regions it looks like there is matter behind each point, in some it looks like there isn't, and the impossibility consists in the perception of a sorite-induced uniformity that is not actually there. On the other hand, impossible stairs induce a false impression of coplanarity when the observer is placed in such a way that the (consistent!) twist is invisible.

Mortensen's work on impossible pictures is particularly interesting as a piece of inconsistent mathematics because of the way in which the choice of tools is driven almost entirely from the mathematical content. Consider the analysis of Necker cubes in [Mortensen, 2006]. Not only does Mortensen - like Asenjo - refuse to fix any particular logic; he also singles out a region of sufficient agreement beyond which logical differences will not matter: *"the interesting facts about these Necker cubes and their classifications are all at the zero degree level of atomic relations compounded with ($\wedge, \vee, -$), together with entailments $\rightarrow$ between them. Quantifiers ($\exists, \forall$) can be eliminated in favour of disjunctions and conjunctions because these geometrical objects contain only a finite number of elements [...]. In particular, higher-degree nestings of entailments or implications, do not seem to reflect themselves in the pictures"*. We can see here that one way to achieve formalism freeness is by bounding the logical complexity required to capture the content at hand. Once the boundary is crossed, we are simply off-topic; the differences no longer matter for the mathematics.[89] A crucial point here is the distinction between a mathematical (informal) theory, whose boundaries may be put wherever we see fit, and a formal theory which treats all logical consequences as equally pertinent.

How is the mathematical theory obtained? Mortensen's theories, one for each Necker cube, consist of a list of general plausible axioms (the observer's *expectations*, i.e. local and global consistency) together with descriptive statements about the particular Necker cube under examination (the observer's *observations*). For each theory, the goal is not only to distinguish the cube from its peers, but also to provide explanation for the appearance of inconsistency (or lack thereof)

[89] This is a context-dependent judgement: there may well be mathematical contexts where all sorts of nested implications and quantifier complexities are pertinent.

in terms of the relationship between expectation and observation. For example, a Necker cube is said to be of the impossible sort (either locally or globally) if any of the consistency axioms are found to disagree with its descriptive statements. [Mortensen, 2009b] reformulates the characterization in terms of linear algebra: impossible Necker cubes are characterized by the existence of solutions for a certain associated system of equations. The approach is extended to chains of Necker cubes in [Mortensen, 2009c].

Inconsistent models may also be helpful in describing impossible figures. For example, [Mortensen, 1997] uses a heap model of the real line to represent a "backdrop universe" against which the various parts of Penrose's triangle are put.[90] The idea is that all distances after a certain point are indistinguishable to the observer, yet there is the expectation that they should be distinct: hence, inconsistency. Mathematically, such a model is obtained by collapsing all the real numbers (i.e. distances) on the left and on the right of some open interval. Such heap models necessarily suffer a loss of functionality, but this is taken to be natural and unproblematic in this context: *"It would hardly be surprising if one could no longer do the aríthmetic on different geometrical structures that one can do on the real line, since their algebraíc properties may be radically different"* [Mortensen, 2002, p.449].

[Mortensen, 2010, ch.13] provides a different explanation of the triangle, based on some experimental work of [Cowan and Pringle, 1978]. A *Merge* operation is described that, given two theories, delivers the deductive closure (under some paraconsistent logic) of their union. For every picture, we usually have two associated *consistent* theories: the theory T_1 describing what we expect, and the theory T_2 describing what we perceive. The full theory of the picture is then obtained by merging T_1 and T_2. In an impossible picture, the result will be an inconsistent theory. In the case of the triangle, T_1 describes what we see as embedded in $\mathbb{R}^2 \times S^1$ (following [Francis, 1987]), while T_2 contains the expectation that the space we live in is $\mathbb{R}^3$.

Now, impossible pictures do not represent actual physical impossibilities. The inconsistency is a trick of the mind; to my knowledge, no one claims that their existence entails dialetheism. However, there have been attempts to claim that the topology of the actual world is best understood as inconsistent, most notably in [Weber and Cotnoir, 2015] and [Weber, 2021a, ch.9]. If that is the case, it seems only natural that we could use some inconsistent mathematics to describe it.

Following the former paper, the central issue is the joint inconsistency between two prima facie intuitive principles. *Symmetry* demands that *"If there is no principled difference between two objects, then there is no principled difference*

[90] The decomposition goes back to [Penrose, 1992].

regarding their boundaries, either" (p.1269). On the contrary, *Connectedness* demands that the space of experience is not separated, i.e. it is not the case that *"there is an exclusive and exhaustive division of the space into two closed parts"* (p.1270). Classical topology rejects Symmetry: however we divide the space, one side will be closed and the other will not. This means that the boundary will arbitrarily be assigned to one and only one side of the partition.[91] Weber and Cotnoir's solution is, of course, to reject consistency instead: the same boundary may be part of both an object and its complement, so splitting the space does not need to generate an asymmetry despite the assumption of connectedness.

While there is a sense in which both this work and Mortensen's work focus on capturing some inconsistent experience, the difference in methodology is quite substantial. As we have seen, Mortensen left the logic undetermined beyond some basic assumptions, and the pertinent extent of his theories was little more than their axioms. On the other hand, Weber and Cotnoir immediately fix a logic, namely **DKQ**,[92] and proceed to look for a general axiomatization from which plausible-looking consequences can be non-trivially derived according to the chosen logic. Furthermore, while Mortensen's choice of theories had an empirical basis in cognitive science, Weber and Cotnoir's main guide is the need to preserve certain pre-theoretic metaphysical intuitions, similarly to much work in naive set theory.

3.7 Conclusion

That will do for now; more will surely come in the future, but of that I cannot yet speak. It should be clear by now that, if we do not make too big of a fuss about what is *really* inconsistent mathematics and what isn't, there is a surprising variety of approaches, methods, and results going around.

Naive set theory is certainly the most varied branch, a telling microcosm of the lack of homogeneity within the field. Most famous (and many unknown) paraconsistent logics have given it a shot, and different people focused on different goals: from classical or paradox recapture to the creation of powerful or bizarre inconsistent models. The search for a canonical model for the naive universe, and for the best logic to explore its properties, continues to this day.

Even when Naive Comprehension is left behind, we have seen that there are many different ways to generate inconsistent mathematics. We may manually add inconsistent objects to our consistent universe, either one at a time to see what each does, or more systematically to access a new universe. We may look at

[91] A region of space is closed if and only if it includes its boundary.

[92] Unsurprisingly, [Weber, 2021a, ch.9] switches to **subDLQ**.

the inconsistent nonstandard models of our favorite theories, made accessible by recasting them in some paraconsistent logic. We may wonder how things would be if our accepted mathematical truths were different, or if an inconsistent identity sneaked into our computations. There is no perfect logic for any of this, and in fact there is little reason to even look for one. Pragmatic considerations will of course constrain the choice of methods; but it seems like anything will do, at least in principle.

The more grounded mathematician needs not despair either, for the world itself can provide plenty for inconsistent mathematics to describe. From the mysterious properties of boundaries to the deceiving depths of impossible pictures, there is no shortage of real (allegedly) or apparent inconsistencies ready to welcome a formal treatment. Even for this kind of endeavour the variety of methods is notable, despite the constraints given by particular intuitions or empirical observations.

This chapter was, to some extent, a celebration of the diversity in inconsistent mathematics. In the next chapter, I will explore the question of what, if anything, all of these pieces have in common.

Chapter 4

Characterizing inconsistent mathematics

In Chapter 1 we have seen that inconsistent mathematics has been asked to fulfil many different and sometimes incompatible roles; in Chapters 2 and 3, we have encountered a plethora of logics, theories, structures, and methods to that effect. The variety is such that one might be forgiven in thinking that the only common theme running throughout the entirety of what I have surveyed is that it has been *called* inconsistent mathematics.

In this chapter, I want to find a characterization of inconsistent mathematics that is able to substantially answer the following question: what distinguishes inconsistent mathematics from classical mathematics? The hope is to find a uniform answer, in the sense that every piece of inconsistent mathematics fitting the proposed characterization should be non-classical precisely in virtue of fitting the characterization. In other words, I am looking for a lens through which all the work in inconsistent mathematics can be seen as genuinely distinct from classical work. Of course, the characterization should also be able to exclude work which is non-classical in ways that have nothing to do with inconsistency, e.g. intuitionistic mathematics.

There are a couple of reasons to be interested in this question. First of all, an answer would provide a clearer understanding of the meaning and potentialities of inconsistent mathematics *beyond* what classical mathematics already is and can do. This might in particular provide some degree of guidance for future research. Furthermore, having such a characterization is important when tackling the question of whether inconsistent mathematics is a genuine *alternative mathematics*, or even a *revolution*: this will be discussed in Ch.6.

One desideratum is to encompass as much as possible of the contemporary work which is usually referred to as inconsistent mathematics.[1] This is because I am not trying to invent a new field, but rather provide a new *perspective* on an existing field. Of course, like for every good perspective, my hope is for my proposal to both contribute to our understanding of the field *and* suggest new avenues for development; and this is more important to me than trying to account for *every* candidate. Inconsistent mathematics is after all very young, and it seems fair to say that it has not really found its way yet. Not every tentative direction may fit my conception of the field, and that is okay: any decision to exclude something from inconsistent mathematics as I imagine it is certainly not intended to cast judgement on whether it is worth pursuing or not. Even so, I will try to show that my characterization still ends up being broader and more welcoming than any of the alternatives in the existing literature.

A second desideratum is for the characterization to support an *argument* (in the sense of Ch.1) for inconsistent mathematics. A convincing vision of a field should, I think, contain and to some extent validate a proper justification for the field. I will show that my characterization naturally goes hand in hand with the argument from liberation, although many of the other arguments I discussed remain pertinent as well. Of course, having a general motivation for inconsistent mathematics does not prevent any particular piece from bringing in its own additional motivations, so the arguments that are left out are not thereby invalidated - they are simply arguments for a more limited conception than the one I am considering. This can also account for arguments incompatible with the overarching one: in that case, the same work will simply be valued for different reasons by different people.

4.1 Contradictions, theories, structures

As I discussed in Chapter 0, there is no agreed upon definition of inconsistent mathematics. Given how young and scattered the field is, many definitions simply reflect their author's particular view of what the field *should* be, descriptiveness be damned.

Let us start by looking at one of the most sophisticated definitions in the literature, in order to highlight some issues common to most attempts. [Mortensen, 2017] defines inconsistent mathematics as *"the study of the mathematical theories that result when classical mathematical axioms are asserted within the framework of a (non-classical) logic which can tolerate the presence of a contradiction without turning every sentence into a theorem"* (p.1). Prima facie, this excludes a lot of the work that is generally considered inconsistent mathematics. First of all, Asenjo's entire approach consists of introducing *new*,

[1] I take Ch.3 to be comprehensive enough to serve as a test for this.

nonclassical axioms governing the behaviour of inconsistent entities.[2] Second, Mortensen's own work often focuses on theories with no explicit axiomatization, e.g. theories of particular inconsistent models or duals of classical theories.[3] Finally, Chunk&Permeate does not need to rely on any inconsistency-tolerant *logic*, and in fact classical logic has been used in most applications.[4]

Generalizing Mortensen's proposal a bit, we may tentatively take inconsistent mathematics to be the study of inconsistent mathematical *theories*, independently of the underlying logic or chosen axioms, and of the inconsistent *structures* described by those theories. For simplicity, we may take this to reduce the study of inconsistent structures to that of inconsistent theories, at least for the purpose of characterization. The central question then becomes how to best make sense of the notion of inconsistent theory.

So, what is an inconsistent theory? Mathematical logic tells us that a *formal* theory is inconsistent if its logical closure contains both A and not-A. However, if the theory is informal - as mathematical theories tend to be - it may not be obvious whether such a derivation is really possible, whether a certain result should really be read as a logical contradiction, or even whether something should belong to a theory or not. And even if we did somehow manage to fix a formalization, in the context of nonclassical logics there can be strong disagreement on what counts as a logical contradiction, especially when contradictoriness becomes disentangled from negation.[5] This may not be too big a problem if the logic is fixed and given a clear canonical interpretation, but we have seen that the literature showcases very little agreement on such matters.

Furthermore, I take it to be always open to just *point* at contradictory pairs regardless of inner structure, which is the approach [Asenjo, 1989] seems to take with respect to antinomies. According to Asenjo, antinomies are more general than logical contradictions, in that they *"do not necessarily require explicit expression in terms of a formula and its negation. Negation may either be implicit or implied by a synthesis of opposite meanings"* (p.400). In fact, *"antinomies should be characterized not only independently of negation but also independently of truth values"* (p.409).[6] This does not prevent the possibility of formalizing any given antinomy in such a way that a logical contradiction is explicitly entailed; however,

[2]See Section 3.2.

[3]See Sections 3.3 and 1.2.

[4]See Section 2.6.

[5]On worries about the negation status of paraconsistent negations - and about whether they can express genuine contradictions - see e.g. [Berto, 2007, ch.7]. More generally, see [Humberstone, 2020] for a taste of how messy recognizing logical relations can get when we look at arbitrary logics.

[6]For a more general discussion of antinomicity, see [Asenjo, 1998].

such a formalization may not be required for the purposes of mathematics.

Thus, for the sake of generality - and to avoid confusion - I am going to introduce the notion of an *s-inconsistent theory* (for *substantially inconsistent*), and distinguish it from formal inconsistency. I take s-inconsistency to be - by definition - what inconsistent mathematics is concerned with, so insofar as we are focusing on theories we can say that inconsistent mathematics is the study of s-inconsistent theories.[7] The problem of characterizing inconsistent mathematics then becomes one of specifying the meaning of s-inconsistency; and insofar as the desired characterization is meant to distinguish inconsistent mathematics from classical mathematics, the specification should ensure that no classical theories be s-inconsistent.

Starting from theories, we may also recover kinds of inconsistent mathematics that are not strictly speaking concerned with the mere derivation of consequences from axioms. Say that an *s-inconsistent structure* is a structure that is at least partially described by an s-inconsistent theory.[8] Of course, every s-inconsistent theory comes with associated s-inconsistent structures, namely those described by the theory; conversely, every s-inconsistent structure or class thereof is associated to the theory describing them.[9]

What about procedures like Chunk&Permeate? If we designate a classical chunk whose contents at the end of the procedure are to be read as the "final" set of theorems, then it may seem like all we are left with is in fact a consistent theory, regardless of whether the starting set of sentences was inconsistent. However, I do not think the genesis of the set of theorems can be ignored in assessing the meaning of the theory; at the very least, a consistent reading is not *forced* on us, and we may just as well think of the starting set of sentences as an s-inconsistent theory whose consequences are extracted in a slightly more convoluted way than usual. For example, one possible interpretation is that the same entities have inconsistent properties (in different chunks) which get triggered at different times of a derivation; then the procedure could be said to describe the workings of some inconsistent mathematical entities. Because such interpretations seem to be in principle available, I take it that a properly comprehensive characterization of inconsistent mathematics should not exclude this kind of procedure a priori.

Having made sure that structures and procedures can be (roughly) accounted for even when thinking about theories, the question is: how should we think of

[7]The focus on theories will be lifted in Section 4.5.

[8]This does not (and should not) entail that every partial description of an s-inconsistent structure is s-inconsistent!

[9]Note that the latter half of the correspondence is easily broken by focusing on formal theories and structures in a fixed language: most infinite structures (in fact, *all* infinite first-order structures) are not categorical, i.e. cannot be described in their own language up to isomorphism.

s-inconsistency in order for the study of s-inconsistent theories to encompass the inconsistent mathematics literature and exclude classical mathematics?

4.2 Inconsistent formalizations

Inspired by Mortensen's definition, we may start by proposing that a theory is s-inconsistent if and only if its underlying logic is inconsistency-tolerant. Both directions seem highly questionable.

On one hand, the adoption of an inconsistency-tolerant logic does not in principle say anything about the inconsistency of a theory. Take for example the axioms of Peano Arithmetic with **LP** as the underlying logic: no contradiction can (presumably) be *proven*, and in fact we may see this as a fragment of classical PA. It seems unacceptable to say that classical (nontrivial) theories can have s-inconsistent fragments. To be sure, a fragment can have inconsistent models; however, these correspond to inconsistent *extensions* of the theory, and we certainly do not want to treat classical PA as inconsistent mathematics for having inconsistent extensions (e.g. Mortensen's PA*).

Conversely, theories may be formally inconsistent while still having a classical underlying logic: we are not forced to consider the (trivial) closure under logical consequence, because reasoning needs not take the form of (merely) following a logic.[10] We may well conclude, from the fact that a theory logically commits us to triviality, that the theory is false; however, we may just as well keep reasoning with it, e.g. analogically, inductively, or even by superimposing a Chunk&Permeate structure on it. The application of Chunk&Permeate to the early calculus is a classic(al) example: the theory - the initial set of sentences - is inconsistent, all of the logical reasoning involved is classical, yet the conclusion is not at all trivial. The falsity of a theory is no good reason to exclude it from mathematics - let alone inconsistent mathematics - as long as we can do something with it.[11]

The underlying logic of a theory thus appears to be independent of its s-inconsistency status. Still, there is a fairly obvious way to deal with the above objections: say that a theory is s-inconsistent if and only if its closure under logical consequence is inconsistent. This correctly excludes fragments of classical consistent theories, while at the same time including formal inconsistent theories regardless of their underlying logic.

[10] This point is made with a vengeance by [Michael, 2016], following [Harman, 1986]: inference needs not follow logical commitment, and in fact logic says nothing about what agents do or should infer.

[11] This is compatible with the fact some people might call it "bad" mathematics and look for a replacement.

Applied to informal theories, however, this criterion is simply too vague, because logical closure is a *formal* notion. A theory may not be intrinsically bound to any particular formal logic; furthermore, every piece of informal mathematics allows for many possible formalizations, and there is no reason to assume that different formalizations would (or should) agree on the logical closure of the theory, since the logical closure of the theory is not something that ever actually shows up in practice. This seems to make the suggested criterion inapplicable in most cases.[12]

We could, of course, have the criterion refer directly to a particular formalization. Say that a *formal* theory is s-inconsistent if and only if its closure under logical consequence is inconsistent.[13] This seems to be uncontroversial enough. To conclude the definition, however, we need to somehow relate the s-inconsistency of a given informal theory to the s-inconsistency of one or more formal theories. How can we do that?

We may say that an informal theory is s-inconsistent if and only if *some* adequate formalization is s-inconsistent. A first problem with this proposal is that it would exclude relevant arithmetic $\mathbf{R}^\sharp$, which does not appear to have any contradictions among its theorems. Now, $\mathbf{R}^\sharp$ - as a formal theory - was presented as a new formalization of *classical* informal arithmetic. From this perspective, it may seem natural to not think of it as s-inconsistent. However, classical informal arithmetic is far from complete, insofar as e.g. it does not care about relevant implications with false antecedents. We *could* consider an extension that cares: this would also be captured by $\mathbf{R}^\sharp$, and I argued in Section 3.4 that there are good reasons to consider such a theory s-inconsistent despite the lack of contradictory theorems.[14]

Even if one doesn't buy the s-inconsistency of $\mathbf{R}^\sharp$, there are other problems with the current proposal. It is far from clear how this notion of adequacy is to be cashed out for something as fuzzy as an informal theory. One problem that seems to show up no matter the specification is that merely formal contradictions could be essentially *inert*, in the sense of having little to no impact relative to

[12]Note that, as already discussed in Ch.1, the fact that it might be unclear how to provide an adequate consistent formalization of a practice is not good evidence for its logical closure being inconsistent, because - as a matter of history - there is very little reason to assume that a consistent formalization will never be found.

[13]This is of course just the usual notion of formal inconsistency. In the case of Chunk&Permeate we can check the logical consequences of our starting set of sentences, even if we do not actually plan to make use of all of them.

[14][Meyer, 2021a] explicitly argues we should care about such implications, which to me suggests a more normative stance than he is letting through. Maybe he means we should only care at the formal level; but for someone who talks about "expanding the pragmatic imagination" that seems a bit underwhelming.

the object of formalization. This is simply because we are free to postulate how inconsistencies work, so their presence does not by itself guarantee that they do much of anything. But if an inconsistency has no relevant effect, then its presence can be *underdetermined* by the informal theory, and there will be no difference in adequacy - ceteris paribus - between a formalization that countenances it and one that does not.

Recall for example Ferguson's connexive arithmetic $\mathbf{C}^\sharp$ from Section 3.3. That theory is inconsistent, because the underlying logic **C** is itself inconsistent. But does the fact that, say, both $(0 = 1 \wedge 0 \neq 1) \to 0 = 1$ and its negation are true really tell us anything about numbers, if they hold for *every* proposition and not just $0 = 1$? What sort of adequacy condition could force the exclusion (or acceptance) of such a contradiction, besides straight up logical prejudice? More to the point, some people might accept the picture that $\mathbf{C}^\sharp$ paints of (some) informal arithmetic, and just as well accept the picture that, say, the perfectly consistent $\mathbf{N4}^\sharp$ paints, given that the two theories merely differ on what falsifies a conditional when the antecedent is not a theorem - something which may easily be underdetermined by the informal arithmetic in question. Now, we could always look at some extension of the informal arithmetic which *does* determine whether a given contradiction should hold; however, that would be a *different* theory.

Here is another example. [Priest, 1994] argues that the standard model of arithmetic is, for all practical purposes, indistinguishable from the finite inconsistent **LP**-model collapsing all numbers after L, where L is some number so large as to lack any cognitive or practical meaning. Of course, in the finite model we have both $L = L + 1$ and $L \neq L + 1$; but by the very choice of L, this contradiction is completely irrelevant to our practices of counting, and so it does not matter whether or not we include it within our formalization. This is not to say that one or the other formalization could not have some advantages, but they would only affect the study of the formalization itself: the mathematics we wanted to formalize is, by assumption, the same, and both formalizations appear to capture it just fine.

One could try to get around this by incorporating some technical requirements into the very notion of adequacy. I'm not sure how that might help disambiguating the examples presented here; but either way, a problem with this strategy is that it makes adequacy much less pertinent to the question at hand, which is whether the *informal* theory is s-inconsistent. Suppose that, for some desirable property X, we have several consistent yet X-less formalizations of a theory but only inconsistent X-having formalizations. It is not at all clear to me why we should be entitled to conclude from this that the theory is s-inconsistent. In fact, the classicist famously resists this kind of move, and will just conclude that the theory admits no X-having formalization instead.[15]

The above examples show that merely asking for *some* adequate formalization to be s-inconsistent fails to distinguish s-inconsistent theories from classical (and subclassical) ones: the same theory can have both consistent and inconsistent adequate formalizations, due to the possibility of inert contradictions. We could step it up, and ask for *every* adequate formalization to be s-inconsistent. But this is a very high bar to clear, and puts an excessive burden on a precise definition of adequacy which we are unlikely to ever agree on. Furthermore, whatever we make of adequacy, it seems inevitable that satisfying such a strong condition will depend on inconsistency being in some sense *essential* to the theory. But then the inconsistency of formalizations will be the consequence of this essential inconsistency, rather than the cause, and our characterization efforts may be better spent on exploring what the cause might look like.

There is another reason why the focus on formalizations may be misguided, and that is the matter of *incidental* contradictions, i.e. unexpected dispensable ones. If inconsistencies are treated as a threat to the adequacy of a certain formalization, then it seems inappropriate to call a theory s-inconsistent just because the currently-in-use formalization contains some inconsistency that no one has yet found, or that workers in the theory are trying to get rid of - that is just standard classical mathematical practice. Whether the presence of a contradiction determines s-inconsistency must depend on factors beyond the mere shape of the formalization at any given time, factors that allow us to distinguish mistakes and work-in-progress within classical mathematics from inconsistency as it occurs in inconsistent mathematics.

To see what I mean, consider Fregean set theory as presented in [Frege, 1893]. The two volumes of the *Grundgesetze* contain no explicit contradictions;[16] furthermore, the inferences carried out by Frege appear to be by and large perfectly valid, and in fact perfectly *rational*. Of course his notorious Basic Law V happens to have instances which lead to triviality, but Frege never used them to derive a contradiction. This is not to say that the system was fine as it is: after all, it was also assumed that closure under a certain notion of logical consequence would not lead to triviality, and that turned out to be false. But it is only the combination of Frege's philosophical commitments that led him to give up on his work once Russell's paradox was discovered: they led him to take his theory to be

[15] This is one of the classical reactions to Gödel's first incompleteness theorem, leading to the idea that informal arithmetic cannot be given a feasible complete axiomatization. Of course, as [Priest, 1979] points out, one is also free to give up consistency. My point is just that there is a choice to be made, and merely pointing at the informal theory cannot settle it without imposing controversial adequacy criteria.

[16] Setting aside the afterword about Russell's paradox.

s-inconsistent, and this is what made it unacceptable to him.[17] Another way to see this is that Frege's proofs did not magically become worthless upon the discovery of an inconsistency somewhere in the corner. In fact, [Heck, 1993] shows that a (presumably) consistent formal subsystem of Frege's - namely, second order logic with Hume's Principle[18] - is still strong enough to derive arithmetic more or less like Frege did. Frege abandoned his project not simply because his formalization turned out to commit him to an inconsistency, but because this in turn led him to the conclusion that his conception of mathematics was committed to an inconsistency. If this had not been the case, he could have simply dropped the problematic axiom, since on a technical level he didn't actually need it.[19]

An example of the opposite reaction is the shift from naive set theory to Zermelo set theory. For decades the background set theory of mathematics resembled something like naive set theory, whose usual formalization via a naive comprehension axiom is inconsistent. Does that make the mathematics of the time s-inconsistent? It does not seem like it, since basically all of it could also be carried out in Zermelo's alternative - and presumably consistent - formalization; not surprising, as this was precisely Zermelo's goal.[20] Matters would have been different if certain parts of mathematics had been taken to be conceptually related to naive comprehension: then the inconsistency could have spilled over. But this was not considered to be the case - at least in the mainstream - and the inconsistent naive formalization was simply written away as an inadequate one. In other words, *set theory* - in the informal sense of set theory as it appears in informal practice - was judged to not be s-inconsistent after all.[21] Similarly, if tomorrow someone

[17] Insofar as inconsistent mathematics is understood as the study of s-inconsistent theories, admittedly this makes it sound like Frege was doing inconsistent mathematics all along, in contrast with my goal of separating inconsistent mathematics from classical mathematics. An easy workaround is to treat inconsistent mathematics as the *conscious* study of s-inconsistent theories. My final definition of s-inconsistency in Section 4.4 will take this into account.

[18] Hume's Principle provides identity conditions for "numbers of things": it states that the number of Fs is equal to the number of Gs iff there is a 1-1 correspondence between the Fs and the Gs. Frege derives it from Basic Law V, but - as explained in [Frege, 1884] - he does not believe it can provide on its own a proper foundation for the concept of number.

[19] I should of course point out that Frege *did* try modifying his system (see the above mentioned afterword) but his suggested revision was similarly inconsistent, and it seems to be generally accepted that there was no way to fix the issue without giving up some of his core commitments. Neologicist programs focusing on Hume's Principle, as proposed e.g. by [Wright, 1997], diverge quite substantially from the original Fregean plan.

[20] See [Zermelo, 1908]. Of course, the translation did not necessarily respect theoremhood: for example, the Burali-Forti paradox went from a proof of inconsistency to a proof that the collection of all ordinals is not a set, and similarly for the Russell paradox. This shift was not considered to be a loss of information, however.

[21] I am not denying that there exists an s-inconsistent naive set theory; all I am saying is that ZFC was presented as a different formalization of the same practices, rather than as a different practice.

discovered that ZFC (as a formal theory) is trivial, what would most likely happen is that ZFC would be amended - or, while waiting, taken to be amendable - to block off that particular route to inconsistency, and set theory would remain not s-inconsistent. However, *if* the inconsistency in question involved something we refused to change (for whatever reason), then we may talk about s-inconsistency.[22]

Since the adequacy of formalizations can be defeated by the discovery of incidental contradictions, it seems very unstable to pin s-inconsistency entirely on the adequacy of an inconsistent formalization. What we need is an additional reassurance that incidental contradictions would not defeat this adequacy (or worse, the theory itself) merely in virtue of being contradictions.[23] This reassurance cannot be grounded in the very fact that the formalization is inconsistent on pains of circularity. Much like the issue of inert contradictions, this suggests that the inconsistency of adequate formalizations should be at best a consequence or by-product of s-inconsistency, not the cause. The source of s-inconsistency needs to be found elsewhere.

4.3 Inconsistent foundations

Rather than grounding s-inconsistency in the adequate formalizations of a theory, we may try to ground it in the adequate *foundations* for a theory. We have already seen how a successful defense of an inconsistent foundation may provide a justification for inconsistent mathematics. But does it provide a characterization? Can we say that a theory is s-inconsistent if and only if the best foundational theory to ground it in is (independently) s-inconsistent?

Let us expand a bit on what grounding should mean here. It cannot follow, from the mere fact that a theory can be embedded into an s-inconsistent foundational theory, that said theory is s-inconsistent. This is because there is no obvious reason why an s-inconsistent theory - foundational or not - should not have perfectly classical fragments. For example, the ascent of naive set theory to best foundation of mathematics would have no impact whatsoever (prima facie, at

[22]One could argue that an inconsistency in ZFC implies an inconsistency in the iterative conception of set, and this should be enough to make set theory s-inconsistent. First of all, I would not be so quick in granting such a strong connection between ZFC and the iterative conception: the essentiality to the latter of both Replacement and Choice has been questioned (see [Incurvati, 2020, chs.2-3]). Second, even if the iterative conception was indeed judged to be inconsistent, maybe it would simply be rejected and a new consistent conception would arise in its wake, as it happened in the shift from naive set theory to ZFC.

[23]Of course the discovery of a particular contradiction could undermine the adequacy of a formalization also for the same reasons *any* discovery could, e.g. severe loss of faithfulness; but that is not an issue here.

least) on the consistency of basic arithmetic: we already saw this acknowledged even in otherwise completely revisionist work like Weber's. Inconsistency is not going to be ubiquitous in any foundational theory capturing millennia of consistent mathematics; so for the purpose of assessing s-inconsistency we should treat as the best foundation for a theory only what *that* theory specifically needs (which of course may vary with time, as the theory develops).

Note that the suggested criterion is not trivial, in the sense that we cannot (usually) take T to be a foundation for itself: mathematical theories often fundamentally rely on interactions with other mathematical theories, and this interaction should be captured by an adequate foundation. For example, contemporary arithmetic is hardly its own best foundation, simply because it relies on geometrical, algebraic, and analytic tools which cannot be captured from within the theory.[24]

A possible gap in the current proposal is that very different kinds of foundations have been proposed for contemporary mathematics, all of them with their pros and cons. There is no pressing need to definitively pick one over the other, because - as discussed in Section 1.4 - each style of foundation is better at playing different foundational roles. For the criterion to make sense as it is, all styles of foundations should agree on s-inconsistency. But what could possibly guarantee this?

One might be tempted to think that, since all the current foundational theories are - presumably - consistent, once we accept the value of an s-inconsistent foundational theory there needs be no mystery as to why we would ask this of every foundational theory. This reply is misguided, in that it presupposes a nonexistent symmetry between consistency and inconsistency: the ubiquity of consistency follows from the fact that consistency has been a *presupposition*, at least in the mainstream, of every formal theory. There is absolutely no reason why dropping the presupposition of consistency would turn *inconsistency* into a presupposition.[25] The formal intertranslatability of different foundations does not - by itself - help either, since there is no obvious reason why any such translation should preserve inconsistency, even if we ask that all foundations rely on the same logic.

We could bind the assessment of s-inconsistency to a particular foundational role, but it is not clear which one that should be. Not every option seems appropriate. For example, imagine that some s-inconsistent foundation turned

[24] Many of these tools can be recovered in second-order Peano arithmetic; but second-order arithmetic as a formal theory goes well beyond what arithmetic as a discipline is concerned with. Even if one wanted to argue that second-order arithmetic is what arithmetic really "is", the point will stand for some *fragment* of arithmetic.

[25] This does not prevent one from arguing that inconsistency *should* be a presupposition, of course: see [Sylvan, 1979]. But a more agnostic attitude, as described e.g. in [Sylvan and Meyer, 1976], is just as open.

out to be the best option for the sake of automated proof-checking. If we take automated proof checking to not be an integral part of a theory - and it usually isn't - then why should we conclude that the theory is s-inconsistent? The inconsistency of the proof-checking framework seems to have little to do with the theory itself; if we hadn't been interested in proof checking, then no inconsistency would have come up. More generally, it seems inappropriate to deduce s-inconsistency from any meta-theoretic advantage imparted by an s-inconsistent foundation.

Even if we managed to identify *the* apt foundational role for the job, grounding s-inconsistency in s-inconsistent foundations remains problematic because it requires independently verifying the s-inconsistency of a given foundational theory, and this does not seem to be any easier than for other kinds of theories. We cannot simply say that an s-inconsistent foundation is needed in order to interpret s-inconsistent theories that we want to study, as this would just bring us back to where we started: what could possibly guarantee that all of *those* theories are s-inconsistent? This is precisely the kind of question that we were trying to answer with an appeal to foundations, so moving back down to particular theories just defeats the whole idea. Similarly, I set aside appeals to subject matter - which I will discuss in the next section - since if we can find a way to link s-inconsistency to subject matter then we can simply do that for particular theories, no foundational detours needed.

We may hope that foundational theories generally require more constraints on their own formalizations, so that it becomes feasible to reduce s-inconsistency of foundations to the inconsistency of all adequate formalizations. Unfortunately, there is still the problem of *sufficiently equivalent* formalizations, where by "sufficiently equivalent" I mean equivalent for the sake of grounding what they are supposed to ground. Sufficiently equivalent formalizations are easy to find, and there is no guarantee that they will preserve logical properties like inconsistency.

This can be seen in contemporary foundations. From a technical everyday viewpoint there is barely any difference between ZFC and, say, MK (Morse-Kelley) set theory or NBG (von Neumann-Bernays-Gödel) set theory.[26] If a mathematician needs to rely on a background set theory, they will freely use whichever happens to be more convenient for the matter at hand, or even simply whichever they are personally more comfortable with. Arguably, there are some conceptual differences: for example, unlike ZFC, both NBG and MK allow

[26]See [Fraenkel et al., 1973] for technical and motivational details on all these theories.

quantification over proper classes.[27] But from the perspective of what these theories are needed for, there is no knockdown argument for one or the other: for example, while ZFC has the simplest language, MK and NBG are easier to work with, and MK is even deductively stronger. And yet, some logical features are not preserved across these systems: most notably, unlike the others, NBG is finitely axiomatizable.

This example shows that we cannot automatically argue from the presence of a logical feature in one formalization of our foundation to the indispensability of said feature. To see that consistency is just as susceptible to this as any other logical feature, consider **LP**-based set theory:[28] this also appears to be sufficiently equivalent to ZFC, since the working mathematician loses nothing (they can work in any model containing the iterative hierarchy, and restrict themself to it) and gains nothing (none of the new sets serve any purpose in classical mathematics).[29] Using Thomas' shrieking strategy, even axiomatic reasoning could be recovered, for whatever it's worth.

Since sufficient equivalence cannot be trusted to preserve consistency, in the end we just cannot escape having to *directly* argue that all sufficiently equivalent formalizations of our favorite inconsistent foundational theory also have to be inconsistent. But this is precisely the problem we started with, which we were hoping the existence of a best inconsistent foundation would fix. Thinking about foundations did not help one bit.

4.4 Inconsistent concepts

So much for formalizations and foundations. What about subject matter? Say that a theory is s-inconsistent if and only if it involves some *inconsistent concept*.[30] There is no shortage of prima facie inconsistent mathematical concepts to pick from: naive sets, infinitesimals, impossible pictures, and so on.

[27] A proper class is a class (i.e. definable collection) that is not a set. For example the collection of all sets, definable as $V := \{x : x = x\}$, is (classically) a proper class since assuming otherwise leads to contradiction. In ZFC variables range over sets, and the notion of class is not definable in the object language. In MK variables range over classes, and a class is a set if it belongs to some class. In NBG there are two kind of variables, one ranging over sets and one ranging over classes.

[28] See Section 3.1.

[29] This is, of course, a judgement that could vary with time: by definition, sufficient equivalence is dependant on current practice. Nevertheless, if two formalizations are sufficient equivalent at one point, then they should remain so forever for at least a *fragment* of the theory they are grounding.

[30] This may sound like it *entails* that every adequate formalization of an s-inconsistent theory be inconsistent, on grounds of faithfulness. However, as already discussed in Section 1.3, there are issues with taking faithfulness to be so central to adequacy.

For the sake of discussing this criterion, it will help to clarify what it means for a concept to be inconsistent. I am going to follow [Scharp, 2013, ch.2] in calling a concept inconsistent if and only if its *constitutive principles*, i.e. the rules specifying where the concept applies and where it does not, are either logically inconsistent or incompatible with other accepted claims.[31] This incompatibility can lead to either *application-inconsistency*, where a concept both applies and disapplies to x, or *range-inconsistency*, where it is both valid and invalid to ask whether a concept applies to x.[32]

It is important for application and disapplication rules to be given separately, otherwise we risk ending up with merely *unsatisfiable*, yet consistent concepts: for example, the concept of squircle, which applies to x if and only if x is a square and a circle, is not inconsistent (classically, anyway) because it is simply the case that nothing is a squircle. On the contrary, the concept of circlare, which applies to x if x is a square and disapplies to x if x is not a circle, is inconsistent. We do not want the involvement of unsatisfiable concepts to characterize inconsistent mathematics, on pains of taking ZFC to be s-inconsistent in virtue of countenancing the (empty) set $\{x : x \in x \ \wedge \ x \notin x\}$. Conversely, we should not take statements to the effect that an inconsistent concept (or rather, its extension) does not exist to count as involving said concept: we do not want to say ZFC is s-inconsistent because it proves that the Russell set does not exist.

As mentioned, logical inconsistency is not the only possibility. Usually, inconsistency arises by means of an incompatibility with currently accepted theoretical hypotheses, or even empirical claims. For example, Scharp's concept of rable is inconsistent - in this world - because it applies to x if x is a table and disapplies to x if x is red, so it both applies and disapplies to red tables; but the inconsistency disappears if there are no such things as red tables. A more interesting example he gives is the concept of Newtonian mass, which was found

[31] I am also going to follow Scharp in not fixing any particular ontological explication of the notion of concept, as it appears to be tangential to the current discussion.

[32] Scharp countenances the free stipulation of inconsistent concepts (i.e. of their constitutive principles), so existence is trivial; but he does not take this to entail dialetheism, because truth and meaning do not go together in the presence of inconsistency. *"Simply stipulating that a certain word has a certain meaning is enough to establish that it does indeed have that meaning, but it does not ensure that the constitutive principles in question are true. If the concept expressed by that word is consistent, then its constitutive principles are true; whether it is consistent often depends on what the world is like"* [Scharp, 2013, p.127]. By these lights, it seems that mathematical theorems involving inconsistent concepts will have to be *untrue*, in virtue of their depending on some untrue constitutive principles. While fully discussing this would take us too far afield into a comparison of views on mathematical truth, I want to point out that the inconsistent mathematician is not in principle forced to reject this aspect of Scharp's position: after all, the theorems would still be perfectly meaningful, possibly useful, and maybe even eligible for a true translation by replacing the inconsistent concepts in question with one or more consistent ones.

to be inconsistent in the 20th century and thereby split into the distinct concepts of proper mass and relativistic mass. This makes it clear that the inconsistency of a concept can be a legitimate *discovery*. Things are not so different in mathematics: the common practices of *exception-barring* and *monster-adjustement*, described in [Lakatos, 2015], involve modifying or reinterpreting mathematical concepts specifically in order to avoid newfound inconsistencies generated by unexpected new applications.[33] For example, the concept of polyhedron went through *many* iterations in an attempt to exclude counterexamples to the generally accepted Euler characteristic formula $V - E + F = 2$.[34]

Now, the main issue with grounding s-inconsistency in inconsistent formalizations was the failure to account for inert and accidental inconsistencies; switching to concepts is meant to somehow exclude those. To avoid the former issue, we may simply take involvement of a concept in a theory as entailing a non-inert contribution of the concept to the theory. Making this precise might lead to complications, but for the sake of argument I'll take this to be good enough for now.[35]

Accidental inconsistencies are significantly more worrying. I just mentioned that even in mathematics concepts can be *discovered* to be inconsistent. More generally, [Tanswell, 2018] convincingly argues that mathematics is not a *conceptual safe space*, i.e. we do not and cannot have complete control on the meaning and development of our concepts.[36] There are many reasons for this. First, Lakatos's case studies can be seen as showing that mathematical concepts are just as *open-textured* as empirical concepts, i.e. not every question concerning the application of the concepts can be settled a priori. Furthermore, the way in which research shapes the available answers and selects one at each point in time is largely unpredictable.[37] Finally, there can be much confusion between *manifest* concepts, which are the concepts as we talk and maybe even think about them, and the corresponding *operative* concepts which are used in practice:[38] for example, while manifest mathematical concepts are often set-theoretical because of the cultural influence of 20th-century reductionist movements, the operative concepts are all but, e.g. the question whether $3 \in 17$ is not treated as meaningful. All of this means in particular that the hope of fixing the consistency status of

[33] These strategies differ from the monster-barring described in Section 2.5, insofar as the latter does not touch the concept at all: alleged exceptions are simply not recognized as such.

[34] V, E, F are the numbers of vertices, edges, and faces respectively. The formula works for all the straightforward examples of polyhedra, e.g. cubes or pyramids; and yet it is far from trivial to find a definition of polyhedron - and proof of the formula - that would actually avoid counterexamples.

[35] The definition of *essential involvement* in Section 6.1 will take care of this anyway.

[36] The notion of conceptual safe space comes from [Cappelen, 2018].

[37] See e.g. the complex history of the notion of compactness in [Raman-Sundström, 2015].

[38] On this distinction, see [Haslanger, 2006].

a mathematical concept a priori is simply utopian: new unexpected applications could turn a concept inconsistent, definitions may naturally shift in ways that end up generating new contradictions or removing old ones, and no matter how much we try and make sure that a concept has a certain consistency status it may still drift into the opposite on the operative side.

Given this instability, the mere involvement of an inconsistent concept may not suffice for s-inconsistency, since it could be a merely temporary situation. It could be that, for that theory, the reaction will be to get rid of any inconsistent concepts which inadvertently pop up by either *revising* them or *replacing* them.[39] Following the first option, we may see the step from early calculus to nonstandard analysis as a revision of the concept of infinitesimal, or the step from naive set theory to ZFC as a revision of the concept of set.[40] According to our tentative criterion, this would mean that we used to mistakenly think (or at least, we were worried) that set theory was s-inconsistent, but it actually isn't: 'twas but a mistake, a blunder that only ever belonged to the context of discovery. This option makes it sound like a theory could only really be s-inconsistent if it could never shift into a consistent one, which I think just leads to the conclusion that there is no such thing as an s-inconsistent theory.[41]

If, on the other hand, we insist on stricter identity conditions for concepts and take the inconsistent versions of a consistent concept to constitute different concepts altogether, the fact remains that these inconsistent concepts could be treated as a mere stepping stone towards one or more consistent concepts doing the same job. For example, Scharp argues that truth is inconsistent, and thus should be replaced by two different concepts that he calls "ascending truth" and "descending truth". In mathematics, one example could be the rejection of Cauchy's infinitesimals in favor of Weierstrass's conception of limit.[42] This option would allow for (what are classically considered as) historical mistakes to count as genuine inconsistent mathematics, which does little to distinguish inconsistent mathematics from classical mathematics: in fact, it would generate s-inconsistency

[39]These two views of conceptual change correspond to the difference between Lakatosian and Fregean concepts as put in [Schlimm, 2012].

[40]This seems to be the position of [Incurvati, 2020], on the grounds that the naive conception and the iterative conception are different specifications of the *same* concept of set.

[41]Maybe this is a biased way of putting it: we may just as well take s-inconsistency as the default, and conclude that a theory could only really be consistent if it could never shift into an s-inconsistent one. But this would be just as unhelpful.

[42]One might worry that these examples arise from too broad a notion of "involving a concept", which countenances replacing a concept within a theory as long as it does the same thing (or close enough). But I doubt that a more pointed alternative like "being *about* a concept" would make much sense in a mathematical context, where concepts are usually introduced (and revised/replaced) precisely because of what we can *do* with them.

all over the history of mathematics.[43]

So, how do we prevent incidental uses of inconsistent concepts from determining s-inconsistent theories? One way could be to require that the concepts in these theories be actively *understood as inconsistent*. This in turn can be justified in terms of fruitfulness without too many worries about whether the concept is "really inconsistent" or not: this is because, as [Scharp and Shapiro, 2017] point out, replacement of an inconsistent concept is only required if the inconsistency inhibits the pursuit of the project. For example, despite the difficulties in interpreting infinitesimals consistently, they were accepted for over two centuries, until they started being a genuine obstacle in mathematical research. Similarly, the naive conception of set was discarded not simply because it was inconsistent, but because it was meant to provide a *foundation* and there was a general conviction that a foundation should be rigorous and therefore consistent. As long as things work out, any doubts on whether the grounding concepts were appropriate or not are somewhat obtuse: by their fruits we can tell that they were appropriate *enough*, even if of course this might change with the development of the theory.

The usefulness of an inconsistent concept does not prove that a consistent reformulation could not do a similarly good job, but here is where the situation differs from the earlier focus on formalizations: because there is a subject matter which we can *commit* to treating as inconsistent, the mere existence of said reformulations does not need to affect the status of the theory. The inconsistency is, for whatever reason, treated as non-negotiable: we know it is there, we welcome it, and we take it to be an important part of our interpreted theory.[44] Consistent reformulations are merely that: reformulations. Note that this is all very different from the attitude of, say, most early calculus practitioners: at the time there were frequent attempts to treat infinitesimals as consistent entities or mere notation, while believing in an inconsistency was uncommon and in no way helpful to the practice or its interpretation.[45]

So, my final proposal is this: *a theory is s-inconsistent if and only if it involves an explicit non-inert commitment to the inconsistency of some concept*.

[43] I do not mean to imply that what are now read as classical mistakes would never be worth going back to and developing into inconsistent mathematics; the point is just that if we want to distinguish inconsistent mathematics from classical mathematics we should generally not read classical mistakes *as they were* as inconsistent mathematics, because what they were was part and parcel of classical mathematics.

[44] A commitment to an inconsistent formalization may *follow*, but it cannot be taken as the source of s-inconsistency, insofar as it is perfectly possible for a classicist to commit (if only by mistake) to an inconsistent formalization of what they take to be consistent. I guess one possibility would be to link s-inconsistency to commitment to an inconsistent formalization (or a range thereof) that is acknowledged as such; I take the current proposal to be simply more general.

[45] See [Vickers, 2013, ch.6].

Let me reiterate that the kind of commitment I have in mind is a commitment to *interpreting* the theory in a certain way; this does not require (though it might involve) a commitment to any particular formalization, nor a commitment to the truth of any particular contradiction. It follows that, as long as we are committed to reading a given concept as inconsistent, any consistent reformulations of a theory involving said concept remain inconsistent mathematics. The intuition here is that consistently saying that something is inconsistent does not, after all, make it any less inconsistent. This way, the charge of collapse into classical mathematics is defused.

It is worth noting that this line of argument is hardly new to classical mathematics itself. For example, every Riemannian manifold can be isometrically embedded in an Euclidean space with a sufficient number of dimensions.[46] Does this make arbitrary Riemann geometries actually Euclidean? Not at all; nor, as far as I know, was it ever thought to. The embedding is a useful technical tool, but it does not cancel the intrinsic - and motivational - viewpoint. Translation is not reduction, and needs not be treated as such.

All that being said, putting things this way does bring up two new potential issues. First, one could ask: in what way does this kind of s-inconsistency lead to mathematics that is distinct from classical mathematics, if essentially the same mathematics can in principle be produced by following a consistent reinterpretation? [Priest, 2014a] argues that some inconsistent theories simply cannot be turned consistent without a loss of expressiveness. Maybe so; but we have seen many examples that *can* be reinterpreted relatively painlessly. Sometimes all it takes is a mere perspective shift with no change in the preferred formalism: I presented ways to understand relevant arithmetic and classical Chunk&Permeate applications as inconsistent, so I would call them s-inconsistent theories, but there are certainly perspectives from which they may look perfectly consistent. The worry is not that a theory may be both s-inconsistent and not; theories go beyond mere formalism, and we can simply say that different interpretations of the same formalism correspond to different theories. The worry is that s-inconsistency says nothing about what makes the inconsistent interpretation privileged beyond the fact that someone is going for it; in other words, s-inconsistency comes out as potentially *unsubstantial*.[47]

One possible counterargument could be that consistent reformulations are going to inevitably be less *natural*, that an s-inconsistent theory could only arise naturally from an inconsistent reading. However, naturalness claims are both

[46] By the Nash embedding theorem: see [Nash, 1956].

[47] It might be objected that the controversial status of my examples simply goes to show they should not be counted as s-inconsistent. But this objection seems to me to betray a consistency bias: why should the existence of a multiplicity of perspectives favor consistency rather than inconsistency?

difficult to argue for and never last very long in the face of future developments, so this line of reasoning seems unlikely to convince anyone who is perfectly happy with the consistent reading. A second counterargument might be that, because an s-inconsistent theory also comes with an insight on how we should *reason* with the concept in question, a consistent reformulation will not actually be able to recapture the very same mathematics, as it will naturally induce a different reasoning. But this also seems unlikely: after all, the very fact that we can discuss certain things in consistent terms seems to mean that we can consistently recapture them in some sense, and it seems to me that we can consistently express what it means to reason with inconsistencies, or even to reason inconsistently.[48]

The second issue is that, while I am happy to claim that the mere presence of a consistent alternative reading should not invalidate the s-inconsistency of a theory, the fact remains that any newcomer to the theory will be free to reinterpret it as they please. In fact, this is one of the goals of formalization: presenting a theory to new generations of mathematicians in a way that is separated from its original interpretation allows for a batch of new perspectives and interpretations to develop, untouched from old biases. Any potential loss of expressiveness may be perfectly acceptable to whoever is doing the reinterpreting, simply because the boundaries and uses of theories are ever shifting, intentionally or not. But then the s-inconsistency of a theory comes out as very *fragile*: it is a matter of interpretation, yet there is no way to shield a theory from consistent reinterpretations, insofar as everyone will bring their own.

These two issues - the fragility brought by the omnipresent possibility of what we may call *consistentization*, and the insubstantiality brought by the dependence of s-inconsistency on subjective interpretations - appear to make it impossible to force the necessity of the "inconsistent mathematics" label on any given theory. In the end, grounding inconsistent mathematics in such a notion of s-inconsistency risks turning the difference with classical mathematics into almost a matter of taste. Could there really be nothing more to it?

4.5 Inconsistent reinterpretations

I claim there is in fact more to it, and the previous discussion is secretly hinting at how. The first step is to note that the closing observations of the last section cut both ways: much like the inconsistent is all too easy to turn consistent, possibly devaluing the alleged inconsistency, so is the consistent not immune to constant *in*consistentization.

[48]For example, I believe Ch.2 to be consistent, preface paradox notwithstanding.

As a first example, consider Cantor's ordinal numbers. [Kitcher, 1984, ch.7] argues that much of the controversy around their introduction came from the fact that some mathematicians took them to be an inconsistent extension of the concept of number: if "numbers" are identified with complex numbers, i.e. all and only the possible solutions of equations with rational coefficients, then it is contradictory to define ω as the first number after all the natural numbers, since no such thing could be a number. Furthermore, it seemed to be part of the concept of infinite series - a series that does not come to an end - that nothing can follow it. The historical solution of the riddle is that there were already different conceptions of number, and of not coming to an end, in the air: numbers were also taken to be entities on which certain arithmetical operations can be carried out - most notably by Hamilton, who referred to his quaternions as numbers[49] - and limit points were a paradigm of entities following an infinite set.[50] This is presumably what Cantor had in mind, and in this sense Cantor's development of an ordinal arithmetic sufficed to show that the ordinals really are numbers and there is no inconsistency.[51] That being said, we *could* take ordinal numbers to be inconsistent by preserving the original reading, for example by saying that ω both comes and does not come after all the natural numbers: it does because it is defined that way, and it does not because nothing does. This is not how it happened, of course, but the question remains valid even now: what if ordinal numbers were *also* complex numbers?[52]

Some natural inconsistent readings can be found in contemporary mathematics as well. For example, as discussed in Section 1.6, [Wagner, 2017, ch.4] argues that the role of x in power series of the form $\Sigma_{n=0}^{\infty} a_n x^n$ is ambiguous in an unresolved sense, insofar as it is often treated as having different roles and meanings within the same practice: sometimes constant, sometimes variable, sometimes placeholder. This does not mean that the ambiguity *could not* be resolved, and in fact the underlying classical assumption is that it always can. However, we *could* also decide to take the ambiguity seriously, and turn it into a practice of actively working with an inconsistent x.

[49]That idea of number is even more common today, given the introduction of p-adic numbers ([Gouvêa, 1997]), surreal numbers ([Gonshor, 1986]) etc.

[50]A limit point x of a set A is a point such that every open neighborhood of x intersects A; the limit of a convergent sequence is a special case of this. A set is closed if and only if it contains all of its limit points.

[51]Even if such a conception had not been around it might have been possible for Cantor to introduce it on the spot, especially since it can be understood as an extension of the traditional conception.

[52]Versions of this question have already been explored consistently. For example, there is a sense in which surreal numbers are, in fact, ordinal real numbers, to the extent that they let the reals extend into the transfinite.

We can also find examples by engaging in some backward readings of the literature on inconsistent mathematics. Take Weber's inconsistent groups: as seen in Section 3.5, they can easily be read as consistent monoids with some extra notation thrown in. But now I can imagine opening a standard algebra book, looking up the definition of a monoid, and thinking: well, that is basically a group with the added possibility of inconsistency! And, as already discussed, this is far from a useless perspective (in theory, anyway): the idea of adding these symbols to the language makes it natural to look at certain classes of structures intermediate between groups and monoids, which maybe would not have come around to anyone's mind otherwise. Similarly, residue classes of integers (i.e. integers modulo n) are classical structures, but they may also be seen as inconsistent models of arithmetic by simply reading congruence modulo n as numerical identity.[53] Note that worries about how to capture these constructions as first-order structures are, if anything, worries that *arise* from the new perspective, rather than prevent it: after all, the intuition can be described without any formalism whatsoever.

Inspired by these examples, I finally suggest that inconsistent mathematics is best characterized as the *activity* of inconsistentizing mathematics, i.e. of *reinterpreting* existing mathematics as s-inconsistent. This naturally generates s-inconsistent theories (and structures); but, by focusing on the process rather than the output, the fleeting status of s-inconsistency is no longer a danger to the identity of inconsistent mathematics. After all, intentional inconsistentization is hardly standard practice in classical mathematics, and in fact it runs directly *against* the usual practice of consistentization involved in rigorization. Note also that, since s-inconsistency is required, the outcome needs to be (to some degree) *intended*; stumbling into contradictions by mistake does not count.

I should clarify that I am using "reinterpreting" very broadly here: it could be a reinterpretation in the formal semantics sense (e.g. switching from consistent to inconsistent models), but it could also involve, say, changing the axioms so that they entail a contradiction, reading a concept as inconsistent rather than consistent, etc. For example, contemporary naive set theories may be seen as an inconsistentization of ZFC, or maybe a *re*inconsistentization of early naive set theories. That being said, intentionality is important: I would hardly take Frege's original set theory to be an inconsistentization.

It may sound weird that, under this definition, inconsistent mathematics never comes first. But this is actually fairly natural if we think of inconsistent mathematics as an activity: mathematics develops from previous mathematics, and it is not hard to see that the entirety of the inconsistent mathematics literature is built on or at least inspired from mainstream mathematics. This is hardly a value

[53] See Section 3.3.

judgement: it is quite natural for new mathematics to grow from the old one. That being said, it is important to note that the target of the inconsistentization needs not be classical mathematics: rather, it could be itself the product of inconsistent mathematics, which then ends up being inconsistentized further or in a different way.[54]

The focus on activity brings attention to the figure of the *inconsistent mathematician*, which may now be seen as the mathematician with a tendency to inconsistentize. The inconsistent mathematician chooses to treat concepts as inconsistent; chooses to give inconsistent interpretations; chooses to contradict accepted statements; and maybe, eventually, even chooses to provide an inconsistent formalization.[55] None of this will be set in stone, and in fact every output will presumably be open to consistentization. The inconsistency - and the possibility of inconsistency - is, so to speak, in the eye of the practitioner; it does not intrinsically belong to any system, or to any given subject matter. This has the pleasant consequence of delivering us from endless debates about what is "really" inconsistent. Which is not to say that the inconsistent mathematician is delivered from pointing at a prima facie contradiction; the point is just that whether said contradiction can be explained away or not is independent of their activity being inconsistent mathematics. This tracks with the understanding of inconsistency from [Asenjo, 2002]: *"it is the subject's motivation that provides the semantic reasons why a statement is being taken as true, or false, or both, or neither"* (p.139).

Another welcome feature of this proposal is the way it reconsiders the relationship between formal logic and inconsistent mathematics. The informal inconsistency is no longer something that requires formalization in order to acquire dignity as mathematics; rather, the informal is the point, although logical investigations can serve to *suggest* reinterpretations like monoids as inconsistent groups and residue classes as models of arithmetic. In particular, the reinterpretation can come *before* the choice of logic: we are now fully respecting the plea from [Asenjo, 1996] that *"it should be the mathematics that eventually determines the logic, rather than the other way around"* (p.55).[56] This makes

[54]It could be objected that the history of mathematics could have started, in some alternate universe, with something that would nowadays be commonly called inconsistent mathematics in this universe. Then, according to my characterization, the inhabitants of that universe would not get to call it that. My answer is that I do not particularly care for my characterization to be stable across the multiverse, and it is up to them to decide what to call their own stuff.

[55]For example, Frege was not an inconsistent mathematician; but successors like [Batens, 2019], refining his theory while preserving the inconsistency, certainly are.

[56]This is compatible with a monist perspective, both because we may not know what the true logic is yet, and because even with a fixed logic there are always many possible formalization choices. I will say more about this in Ch.6.

for a healthier relationship: formalization can be desirable for many reasons, e.g. generalization or clarification, but *gatekeeping* is not one of them, and I agree with Asenjo that it shouldn't be in the context of inconsistent mathematics either.

Not all arguments from Chapter 1 mesh well with my proposal. First and foremost, the arguments from practice are largely off topic, to the extent that in no way does my characterization purport to be descriptive of what classical mathematicians informally do: in fact, I am not aware of any claim to the effect that mathematics is or ever was in the habit of purposefully making things inconsistent. At best, it could be said that the inconsistent historian is justified in reinterpreting the consistent reading given by the classical historian, when said reading is incorrect.

The argument from pure maths fits quite well, since it presents inconsistent mathematics as essentially an exploration, and exploration is an activity. Special cases of the argument correspond to particular styles of reinterpretation. Mathematical logic justifies the construction of formal inconsistent models - which is a way to inconsistently reinterpret a theory - as a piece of nonclassical model theory: similarly, duality is a systematic tool for reinterpretation, and dualizing an incomplete theory is a particular, overtly syntactical way to carry out an inconsistent reinterpretation. The arguments from foundations or philosophy of mathematics can also be thought of as justifying some sort of exploration, albeit a much more pointed one: in this context inconsistentization may function as a *search strategy*. For example, it can be adopted to look for an inconsistent foundation, and to find ways to interpret theories within such a foundation.

When it comes to subject matter, there are different perspectives we might take. On one hand, we could think of inconsistent reinterpretations as a possible way to access or look for inconsistent concepts, whose existence therefore justifies the reinterpretations. In this sense, the argument from subject matter is recovered. On the other hand, we could push for a reversal of the argument, so that inconsistent subject matter does not force inconsistent interpretations, but rather it is the persistent search for inconsistent reinterpretations that brings inconsistent concepts into play. On this view it is inconsistent mathematics that justifies the concern with an inconsistent subject matter, not the other way around.

Finally, inconsistent mathematics as an activity fits really well with the argument from liberation, which takes the core of the defense to be the social upshot.[57] Dismantling oppressive frameworks and implicit assumptions requires a constant effort; one cannot merely change the categories and leave it be, as the risk of new oppressions rising from the ashes of the old is ever present. In fact, some of

[57]This is not much of a coincidence: thinking of incomaths as an activity is precisely what led me to the argument from liberation.

the discussion in this chapter may be taken to show that if changing the categories was all there was to inconsistent mathematics, then we would not *need* a notion of inconsistent mathematics. On the contrary, the inconsistent mathematician is an essential member of the mathematical community, in that through their activities they can provide the much needed rebellion against a status quo that will always end up erasing or marginalizing *someone*.

There is a clear analogy between the liberatory activity of the inconsistent mathematician as I just described it and *gender fucking*, i.e. the activity of disrupting gender categories in order to counteract old and new gender-based oppressions.[58] With this analogy in mind, I will call this conception of inconsistent mathematics *queer incomaths*, and I will call *mathfucking* the associated activity.[59]

4.6 Inconsistent mathematics as a critical maths kind

I now want to argue that there is more to this analogy than meets the eye. For a start, consider the following definition from [Dembroff, 2019]: *"For a given kind X, X is a critical gender kind relative to a given society iff X's members collectively destabilize one or more core elements of the dominant gender ideology in that society"* (p.12). In contemporary Western societies, the dominant ideology is of course the strict sex-based gender binary, Dembroff's model of which I described in Section 1.8. Adapting this terminology, we can say that X is a *critical maths kind* relative to a given society if and only if X's practitioners collectively destabilize one or more core elements of the dominant mathematics ideology. Currently, the dominant ideology is classical mathematics.

Of course, *any* sort of nonclassical mathematics could be seen as a critical maths kind. We could even lump them all together under the kind of "classical maths defiers"; however, much like for critical gender kinds, this sort of move is "*not particularly illuminating"* (p.15), since different nonclassical mathematics can differ wildly in both how and how much they destabilize classical mathematics. The usefulness of the notion of critical kind lies in singling out and comparing these different ways to be critical. For example, intuitionistic mathematics and non-well-founded set theory may well be both seen as critical maths kinds, and

[58] See e.g. [Bornstein, 1994] and [Whittle, 2005].

[59] The idea behind queer theory's call for constant questioning is not simply that all conceptual frameworks are in principle open to criticism; rather, the very possibility of a stable categorization is questioned, insofar as identity - and therefore membership - is always in a state of flux. For an introduction to queer theory, see [Salamon, 2021]. The idea that queer theory could be fruitfully applied to mathematics is not new, and in fact has already made its way to the mathematics education literature: this is the so-called *queering mathematics* program, on which see e.g. [Dubbs, 2016], and [Yeh and Rubel, 2020].

yet have almost nothing in common in terms of what tenets of classical maths they are questioning: intuitionistic mathematics is subverting the received logic and metaphysics of mathematics, while non-well-founded set theory is much more modestly questioning the received conception of set.[60]

Now, "may" is doing some work here. To my knowledge, classical mathematicians never felt particularly threatened by non-well-founded set theory, possibly because of the easiness of translating it into a classical (i.e. well-founded) setting. It is perfectly possible to see non-well-founded set theory as a part of classical mathematics, as long as the axiom that there are non-well-founded sets is not understood literally, but rather as the claim that there are some well-founded structures which may be taken as representing non-existing non-well-founded sets.[61] But it's not hard to imagine someone wielding non-well-founded set theory as a way to genuinely contradict classical mathematics: all it takes is to take the respective axioms at their word.[62]

Intuitionistic mathematics is a very different beast. L.E.J. Brouwer - the main figure behind intuitionism - explicitly presented it as a challenge to classical mathematics, and it was perceived by classical mathematicians as such. This sparked the kind of reactions that we would expect in response to a critical kind. [Hilbert, 1927] considered intuitionism a *threat* to mathematics itself: *"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 the logical* ε*-axiom?"*.[63] Intuitionism is not just painted as being wrong here: it is *dangerous*, and therefore it has to be suppressed. The "war" famously escalated into Hilbert dismissing Brouwer from the Editorial Board

[60] On the metaphysics of intuitionism, see e.g. [Posy, 2020, ch.4]. On the "graph" conception of set associated with non-well-founded set theory, see e.g. [Incurvati, 2020, ch.7].

[61] Jon Barwise refers to this as a *"serious **linguistic** obstacle [...] arising out of the dominance of the cumulative conception of set"* (emphasis mine) in the Foreword to [Aczel, 1988] (p.xii).

[62] In fact, many naive set theorists agree that classical mathematics is *wrong* in rejecting non-well-founded sets, since they should be countenanced by naive comprehension.

[63] Hilbert's ε-axiom is $A(x) \to A(\varepsilon(A))$. The idea is that the logical operator ε picks a witness which satisfies A, as long as A is satisfied by anything at all. Quantifiers are then defined from ε as follows: $\exists x A(x) \leftrightarrow A(\varepsilon(A))$ and $\forall x A(x) \leftrightarrow A(\varepsilon(\neg A))$. The ε-axiom implies LEM, which is a good reason for intuitionists to reject it.

of the *Mathematische Annalen*.[64]

We can now understand queer incomaths to be a particular critical maths kind: mathematics whose practitioners collectively destabilize classical mathematics through their tendency to inconsistentize. Since the activity of inconsistentization is what characterizes inconsistent mathematics, all inconsistent mathematics can be seen as belonging to queer incomaths: the difference is one in degree, not in kind.

To elaborate on this difference, it can be helpful to consider *subkinds* of queer incomaths. Let *radical incomaths* be inconsistent mathematics destabilizing classical mathematics through inconsistent reconstruction: this is instantiated by contraclassical foundationalits projects like [Weber, 2021a]. On the other hand, let *conservative incomaths* be inconsistent mathematics destabilizing classical mathematics through inconsistent expansion: this kind takes classical recapture to be a prerequisite, and is by far the most common - most of Chapter 3 is about stuff like this. We can postulate analogous critical gender kinds: the radical kind proposes a new gender classification which is incompatible with the classical binary, while the conservative kind simply adds to said binary.[65] Such radical and conservative kinds still destabilize the dominant ideology, but they do it in different ways.

[Dembroff, 2019] also points out that the same individual can - and in fact, usually cannot avoid - belong to different gender kinds, not only at different times but possibly simultaneously. Resistance acts of various sorts can overlap; furthermore, membership to a dominant kind may not always be avoided, and in fact might be partly constitutive of the meaning of those resistance acts. We can see the same phenomenon in mathematics: for example, any piece of inconsistent mathematics rejecting LEM belongs to both queer incomaths and

[64] See [van Stigt, 1990, ch.2]. While intuitionism is obviously a critical kind with respect to the maths of today, it may sound a bit ahistorical to univocally frame it as the critical kind of its time as well: after all, both Hilbert's formalism and Brouwer's intuitionism were *new* responses to a foundational crisis, and it wasn't necessarily obvious the way the debate would have gone. However, Hilbert's formalism was essentially a way to preserve the status quo, insofar as none of the accepted theorems and methods had to be put into question. On the other hand, intuitionism was a *revisionist* position, whose adoption would have had vast consequences for the practice of mathematics far beyond the narrow confines of foundational studies. So when we look beyond the associated philosophies to what they meant for the practice, Hilbert's position appears to have been far more in tune with the dominant ideology of his time. Furthermore, by the time Hilbert was writing the above lines, his views - or at least, the conservative consequences of his views - had already thoroughly won the battle in terms of popularity; mathematicians were perfectly happy to continue reasoning classically. In this sense, Hilbert was writing from a position of threatened power.

[65] The former is often an explicit feminist project: see e.g. [Plumwood, 1989]. One example of the latter is the legal inclusion, in some European countries, of a third "diverse" option for one's gender marker.

some constructive kind.[66] The inability to escape membership to a dominant kind can be seen in the common use of translations to undermine the nonclassicality of inconsistent mathematics: the same inconsistent mathematics may then belong to both queer incomaths *and* classical mathematics.[67] I do not take this to undermine the characterization problem we started with: even when queer incomaths happens to be classical, it remains distinct from classical mathematics in virtue of being both classical *and* queer.

We can push this even further. Critical maths kinds - and in particular, queer incomaths - can serve as the means by which members of critical gender kinds enact their resistance. One way would be to mathfuck classical applications to gendered contexts (e.g. the marriage problem from Section 1.8): this is an example of what Dembroff calls *principled* destabilizing, i.e. *"destabilizing that stems from or otherwise expresses individuals' social or political commitments regarding gender norms, practices, and structures"* (p.13). Another way would be to express one's own relationship with gender in a way that contradicts classical mathematics: this is Dembroff's *existential* destabilizing, i.e. *"destabilizing that stems from or otherwise expresses individuals' felt or desired gender roles, embodiment, and/or categorization"* (p.13).[68]

The connection is particularly noticeable for the critical gender kind *genderqueer*, which Dembroff defines as *"such that its members have a felt or desired gender categorization that conflicts with the binary axis, and on this basis collectively destabilize this axis"* (p.16). Recall from Ch.1 that the "binary axis" here is the classical logical relation between man and woman. To explicitly formalize one's own gender identity via an inconsistent mathematical structure - which is an act of mathfucking, and so denotes membership to a critical maths kind - then comes off as genderqueer, and so denotes membership to a critical gender kind.[69] Of course this doesn't mean that every practitioner of queer incomaths - even in gendered contexts - should be counted as genderqueer; but it is one way of getting there.

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[66] [Oddsson, 2021] would be an example.

[67] This does not mean that queer incomaths is not really inconsistent. Trans women are women, and all that jazz.

[68] Ch.5 will provide both an instance of the former, and examples of how the latter might happen.

[69] The reason genderqueerness is the consequence rather than the cause here is because on Dembroff's account purely internal features of an individual - e.g. their gender identity - are not sufficient to classify them as genderqueer: what matters is the *act* of resistance. On the fraught relationship between gender identity, gender terms, and gender metaphysics see e.g. [Dembroff, 2018] and [Barnes, 2020].

4.7 Conclusion

Let us take stock. I have argued that formal logic by itself is unable to properly distinguish informal inconsistent mathematics from informal classical mathematics: the adoption of paraconsistent logics is neither required by inconsistent mathematics nor does it necessarily lead to contradictions, while the notion of adequate formalization cannot rule out accidental or inert contradictions, even if we look at foundational theories. Because concepts can always be consistentized or replaced, a characterization based on them encounters a similar issue unless we require an explicit commitment to inconsistency; but that leaves the inconsistency status of theories worryingly fragile and open to charges of insubstantiality.

To overcome these issues, I have suggested characterizing inconsistent mathematics as the activity of reinterpreting existing mathematics as inconsistent. Together with the argument from liberation this leads to queer incomaths, a critical maths kind which reads inconsistent reinterpretation as mathfucking - a liberatory activity analogous to gender fucking. In the next chapter we will see how these ideas can guide the practice when adopted consciously.

Chapter 5

A test case: inconsistentizing the Cantor space

Now that we have a functioning characterization of inconsistent mathematics, I would like to showcase how said characterization can guide the practice when applied consciously. More specifically, I am going to take a classical structure - the *Cantor space* - and discuss various ways to inconsistently reinterpret it, with no appeal to particular axiomatizations or logics. I will then poke at possible liberatory upshots by feeding a non-mathematical concept - namely, gender - to the Cantor space and then carrying the reading to the inconsistent reinterpretations. The reader unfamiliar with general topology will find all the necessary definitions and theorems in the Appendix; still, the focus here is on the general strategies rather than any particular result, so skimming over the more technical details should be a fine way to read this chapter and still get the gist.[1]

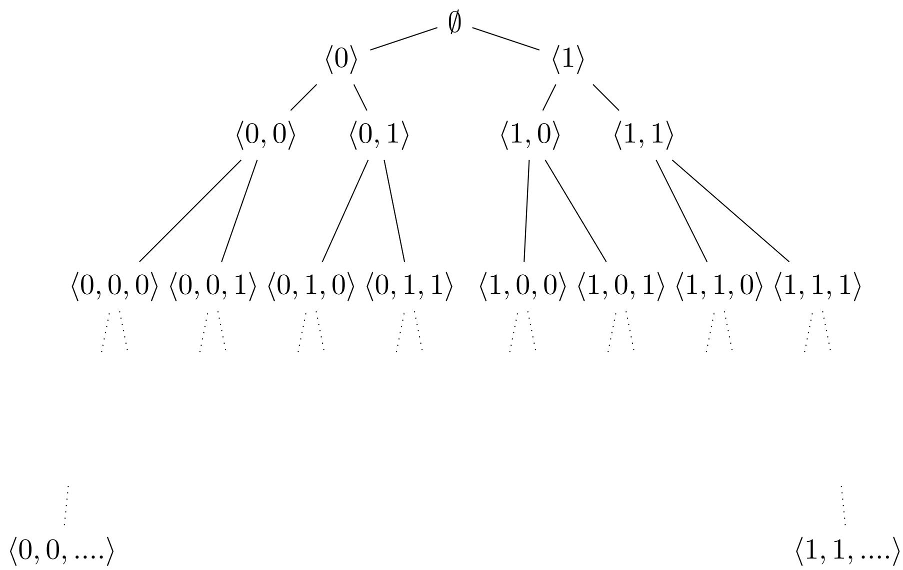
My goal here is not to uncover great (or any, really) mathematical or gender-theoretic depths; rather, this chapter should be seen as a small compendium of *conceptual experiments*, in the spirit of e.g. [Martínez, 2018] and [Van Bendegem, 2005]. Maybe some of the sketches to follow will one day find their way to the working mathematician, or to any sort of modeler, for use in a larger picture; or even to the classroom, where they could serve as a playful showcase of what one can do if the chains of classical mathematics are recognized as such, and inconsistent mathematics is consciously employed as a way to break them.

[1]That being said, I will avoid using the standard definition/theorem/proof structure so as to not tempt a careless reader to skip straight to the theorems, which would very much defeat the purpose of the chapter.

5.1 The classical space

The *Cantor space*, commonly denoted by 2^ω, has as its underlying domain the set of all binary sequences - i.e. sequences of 0s and 1s - of length ω. One way to think of its topology is as the topological product of countably many copies of $\{0, 1\}$, each treated as a discrete space. Call this the *product perspective*.

A (classically) equivalent perspective - which we may call the *tree perspective* - is as follows.[2] Picture the set $2^{<\omega}$ of *finite* binary sequences as a binary tree ordered by end extensions, the root being the empty sequence. Then 2^ω can be thought of as the set of "limit leaves" of this tree - or as the set of branches, if you prefer. Here is a pretty picture:



As a base we pick the sets selecting all the limits/branches of a full subtree: namely, sets of the form $N_p = \{s \in 2^\omega \mid p \prec s\}$ with $p \in 2^{<\omega}$, where $p \prec s$ means that p is an initial segment of s. Note that, since we are comparing initial segments, for $p \neq q$ we have either $N_p \cap N_q = \emptyset$ or $N_p \subset N_q$ or $N_q \subset N_p$.

A third (classically) equivalent perspective - call it the *questionnaire perspective* - involves seeing the Cantor space as a subset of the real line, endowed with the subspace topology. Starting from the unit interval $[0, 1]$, split it into three parts (by cutting at $1/3$ and $2/3$) and remove the middle third; then split each of the two remaining parts into three and remove the middle thirds; and so on. For each point x in the resulting set we can ask a sequence of binary questions on where it

[2] By equivalent I mean that the spaces associated with the two perspectives are homeomorphic via the obvious bijection, and so are essentially the same space.

is located, so that the sequence of answers can be coded as a binary sequence. For example, the first question is: is x in $[0, 1/3]$ or in $[2/3, 1]$? If x is in the left set, we mark a 0; if it is in the right set, we mark a 1. The next question will build on the previous answer, asking about subintervals of $[0, 1/3]$ or $[2/3, 1]$; and so on. Each x corresponds to exactly one sequence of answers, and therefore exactly one infinite binary sequence.[3] Note that this construction may be matched with *any* sequence of binary questions, e.g. by letting "yes" or "true" correspond to "left", and "no" or "false" to "right".

It will be useful to keep in mind the following classical facts about the Cantor space:

- It has the cardinality of the continuum.
- It is T2, because any two distinct sequences disagree on some finite initial segment.
- It is compact, because it is the product of finite (hence compact) spaces.
- It is second-countable and zero-dimensional: the sets N_p form a countable clopen base, since $|2^{<\omega}| = \omega$ and the complement of N_p can be written as $\bigcup\{N_q : \text{length}(q) = \text{length}(p),\ q \neq p\}$.
- It is metrizable: we have a metric

$$d(x, y) = \begin{cases} 0 & x = y \\ 2^{-d'(x,y)} & x \neq y. \end{cases}$$

  where $d'(x, y)$ is the smallest coordinate at which x and y disagree.
- It is perfect, since no N_p is a singleton.

These properties suffice to characterize the Cantor space up to homeomorphism, which justifies the *abstract perspective*: the Cantor space is the unique nonempty zero-dimensional compact metrizable perfect space.[4]

Enter inconsistency.

[3] More specifically, every sequence of subintervals selected from the questionnaire can be identified with its intersection, which (by the nested intervals theorem) is going to be a singleton.

[4] See [Kechris, 2012, Thm 7.4]. This entails that size, T2, and second-countability are implied by the other properties. For the curious reader: metrizable spaces are T2, compact metrizable spaces are second-countable, and nonempty perfect compact metrizable spaces have the size of the continuum (see respectively the Appendix, [Engelking, 1977, Thm 4.3.27 + Cor 4.3.6], and [Kechris, 2012, Prop 4.2 + Cor 6.3]).

5.2 Adding inconsistent identities

A very general way to introduce inconsistencies in any classical structure is to make it so that some identities are both true and false. On **LP** grounds, we know how this kind of collapse can make logical sense, and many properties of the original space can be preserved.

When it comes to the Cantor space, one uniform way to induce a collapse is to select some "special" coordinates such that two sequences may still be equal despite differing in those coordinates (and therefore differing). Borrowing from [Priest, 1994], this could be a way to treat all coordinates beyond the greatest natural number, since it would be beyond any practical use (or cognitive ability) to distinguish sequences differing that far ahead. More generally, any reason for identifying sequences may be given an inconsistent reading, maybe even in direct protest to an unwanted identification or distinction.

Formally, let $A \subseteq \mathbb{N}$, and redefine identity on 2^ω as follows: for every $a, b \in 2^\omega$ let $a =_A b$ if $a_i = b_i$ for every $i \notin A$. We denote the Cantor space with identity $=_A$ by $2^\omega_{=_A}$: by this I mean that the classical interpretation of identity on 2^ω is *extended* by adding the true identities just defined, so that e.g. if $0 \in A$ then $\langle 0, 0, 0, ...\rangle$ is both identical to and distinct from $\langle 1, 0, 0, ...\rangle$.[5] Of course, $=_A$ is a perfectly legitimate equivalence relation,[6] and we could keep everything classical by simply not identifying it with identity; but this hardly means the two procedures are equivalent. The natural topology on $2^\omega_{=_A}$ is as follows: a set $B \subseteq 2^\omega_{=_A}$ is open if and only if it is open in 2^ω. If we think of subsets of $2^\omega_{=_A}$ as closed under $=_A$, this simply mimics the quotient topology on $2^\omega / =_A$. However, we may also consider subsets that are not so closed. If such a subset B is open in 2^ω, yet contains some x such that $x =_A y$, $y \notin B$, and $B \cup \{y\}$ is not open, then it makes sense to say that B is both open and not open in $2^\omega_{=_A}$. Other topological notions should be similarly liberalized in a paraconsistent-friendly way, so that collapses can indeed be said to preserve topological properties. For example, it would be inappropriate to require distinct-but-coinciding points to have *consistently* disjoint neighborhoods, and we should let such points have distance 0 (as long as they also have positive distance).

Quotients are hardly guaranteed to preserve topological properties in general. For finite A, quotienting by $=_A$ does deliver a copy of the Cantor space, since

[5] I am not here thinking of sequences in $2^\omega_{=_A}$ as set-theoretically built from their components, and so I am not thinking of identity of sequences as set-theoretically derived from identity of components. The given definition of $=_A$ is merely a (classical) way to describe the new space as related to the classical one. More generally, this chapter will not be concerned with reductionist reframings of what is going on. The reader is of course welcome to entertain themself with attempts at, say, full formalization within a paraconsistent set theory - Chapters 2 and 3 should have provided plenty of tools for that - but I want to resist the idea that it be *necessary* in order to discuss mathematical ideas.

[6] It is also a congruence with respect to any component-wise operation on sequences.

the effect is the same as removing from every sequence the coordinates in A; the difference with $2^\omega_{=_A}$ lies mainly in the inconsistency. For infinite A, however, there is no particular reason for the quotient to resemble the Cantor space, so the difference with the collapse can become even more marked. For example, take $A = \{n : n \geq 1\}$. Then the quotient is homeomorphic to the discrete space $\{0, 1\}$, where 0 represents $N_{\langle 0\rangle}$ and 1 represents $N_{\langle 1\rangle}$. On the other hand, since collapses preserve topological properties, from the abstract perspective we are well justified in still calling the collapsed space a (finite) Cantor space.[7]

LP-style collapses are not the only way to uniformly introduce inconsistent identities. Another way could be to take identity as unable to distinguish between sufficiently similar sequences. Suppose the sequences are coding, say, a picture in such a way that every difference in coordinates corresponds to a minuscule difference in appearance. Then pictures coded by different sequences may well be perceived as both the same and different, as long as they differ on few enough coordinates. Formally, given a natural number n, for every $a, b \in 2^\omega$ let $a =_n b$ if there exists $A \subset \mathbb{N}$ such that $|A| \leq n$ and $a_i = b_i$ for every $i \notin A$. The larger n is, the more inconsistent the space with identity $=_n$ - call it $2^\omega_{=_n}$ - is. Note that $=_n$ (for any fixed positive n) is not transitive: in fact, its transitive closure collapses all sequences into one, since any two sequences are linked by a series of changes in a single component. Hence, $2^\omega_{=_n}$ is a structure governed by non-transitive identity. Given the above perception-based interpretation, this is a natural upshot of the apparent non-transitivity of perceptual indiscriminability.[8]

Note that $2^\omega_{=_\emptyset} = 2^\omega_{=_0} = 2^\omega$, so the consistent Cantor space comes out as a particular, maybe idealized case of all these constructions.

5.3 Turning the building blocks inconsistent

Messing with identity on the structure is not the only way to introduce inconsistency. For example, the idea of inconsistent coordinates may be implemented by focusing on the *construction* of the space.

For example, what does it mean for the place of the n-th coordinate to be inconsistent? It could mean that, for every sequence, its n-th coordinate is both 0 and not 0, i.e. 1. If we read this in terms of the product perspective, this means that the n-th projection function is also not a function, and in fact always delivers two values; or, similarly, that one of the discrete spaces $\{0, 1\}$ occurring in the

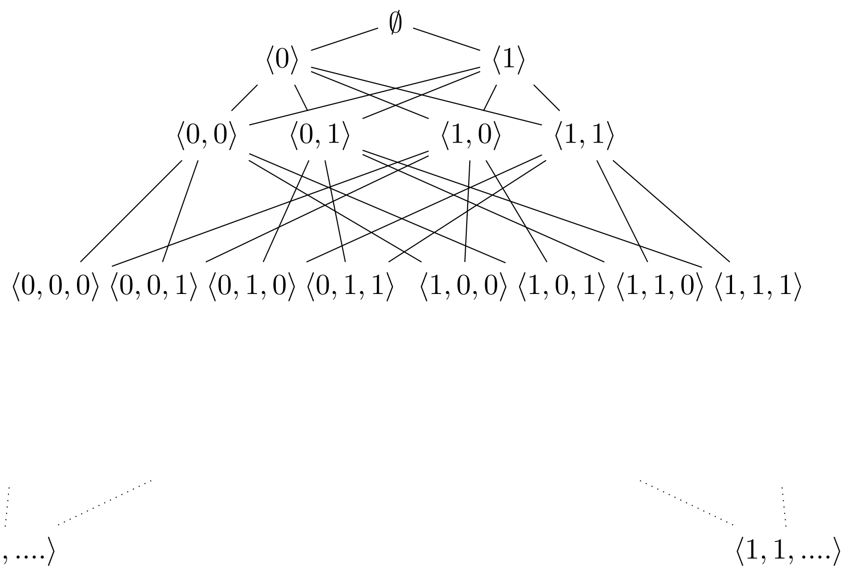
[7]Of course the uniqueness result cannot be applied directly, as it only ranges over *classical* spaces. Leaving consistency behind opens the door to a bunch of new spaces - besides, the notion of homeomorphism would need to be revised!

[8]See e.g. [Wright, 1975].

product is actually a singleton with two elements. This does not, in general, need to entail that a sequence with an inconsistent coordinate is both self-identical and not: it depends on how identity spreads from components to full sequences. So, to differentiate this approach from the previous ones, let us assume identity *on infinite sequences* behaves classically.

Now, what does this mean for the *topology* of the space? If a sequence has an inconsistent coordinate, then it will extend two incompatible finite sequences. This behaviour can be modelled on the product perspective by replacing the discrete space $\{0, 1\}$ corresponding to the coordinate with a trivial space such that neither $\{0\}$ nor $\{1\}$ are open; or equivalently, on the tree perspective, by modifying the nth and subsequent stages of $2^{<\omega}$ so that finite sequences differing only in their n-th coordinate have exactly the same end extensions (i.e. they adopt each other's successors). Note that both of these are perfectly consistent procedures. Denote the resulting space (with the topology being defined as in the classical case) by $2^{\omega}_{\{n\}}$.

Here is a sketch of $2^{\omega}_{\{0\}}$:



On this reading, assuming the nth coordinate to be inconsistent affects the topology by making it impossible for open sets to separate sequences which only differ in their nth coordinate. A first obvious consequence is that separation properties fail: $2^{\omega}_{\{n\}}$ is not even T0, and in fact we have a counterexample for *every* sequence.[9] For the same reason we only have pseudo-metrizability,

[9] The relationship between inconsistency and failure of T0 is also noted in [Weber, 2021a, ch.9].

since sequences differing only in their n-th coordinate must have distance 0.[10] However compactness, second-countability, zero-dimensionality, and perfectness are unaffected, and follow as in the classical case. We may call $2^\omega_{\{n\}}$ a pseudo-Cantor space, as the identification of sequences having distance 0 (or, equivalently, the identification of sequence pairs witnessing a failure of T0, i.e. the *Kolmogorov quotient*) will then produce a homeomorphic copy of the Cantor space.

Now, there is of course nothing inherently inconsistent in a topology being unable to distinguish two points. However, here the topology cannot distinguish said points *because* of an inconsistency. In fact, if we let this reverberate on the *identity* of sequences, then $2^\omega_{\{n\}}$ would be essentially the same as $2^\omega_{=_{\{n\}}}$, and we could say that T0 both holds and does not hold - and that there is a pseudo-metric which is both a metric and not - since the only counterexamples are both identical (hence not a counterexample) and distinct. However, sticking with classical identity is a conceptual possibility, and then what we have is an inconsistent assumption leading naturally to a consistently defined property of the structure (i.e. the failure of T0).

All of this can be straightforwardly generalized to the case where we have an arbitrary nonempty set A of inconsistent coordinates: denote the resulting space by 2^ω_A. The only difference is that, in the case A is an end segment, the Kolmogorov quotient will be a *finite* space of binary sequences, so there is no longer a consistent "way back".

5.4 Adding inconsistent sequences

We have seen that adding edges to $2^{<\omega}$ has a natural inconsistent interpretation. But what if, rather than adding cycles to the existing branches, we added new *nodes* specifically for the purpose of generating inconsistent sequences? For example, instead of turning $\langle 0,0\rangle$, $\langle 0,1\rangle$ into successors of $\langle 1\rangle$ and $\langle 1,0\rangle$, $\langle 1,1\rangle$ into successors of $\langle 0\rangle$, as we did for $2^{<\omega}_{\{0\}}$, we can add two nodes $\langle i,0\rangle$ and $\langle i,1\rangle$ which follow from both $\langle 0\rangle$ and $\langle 1\rangle$: the resulting branches will correspond to new sequences which extend both $\langle 0\rangle$ and $\langle 1\rangle$, while the classical sequences remain undisturbed.

Under the questionnaire perspective, there is a particularly nice rationale behind inconsistent coordinates so understood. As already mentioned, elements

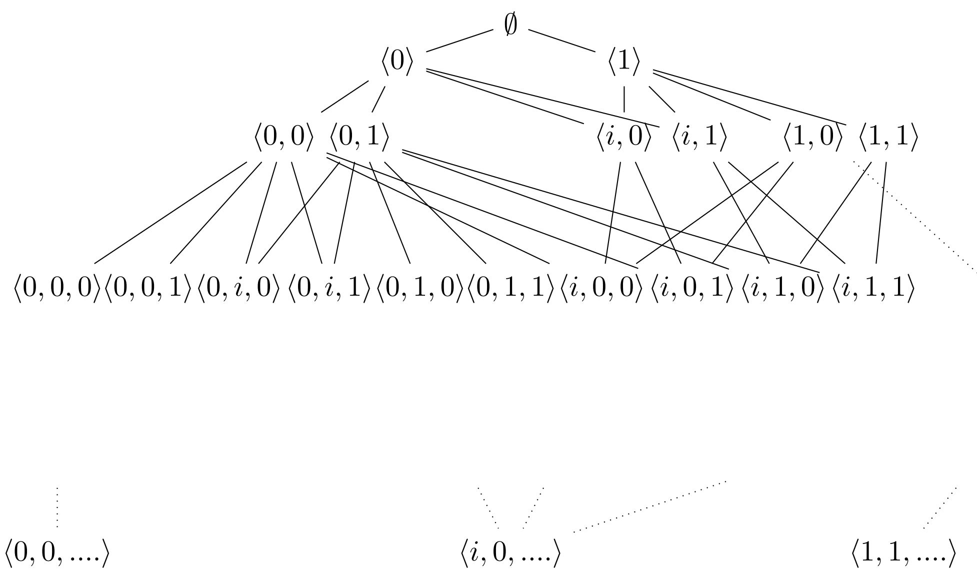
[10] The following works:

$$d(x,y) = \begin{cases} 0 & x = y \text{ or } x,y \text{ only differ in the } n\text{-th coordinate} \\ 2^{-d'(x,y)} & \text{otherwise} \end{cases}$$

where $d'(x,y)$ is the smallest non-nth coordinate at which x and y disagree.

of the Cantor space may be characterized by an infinite series of binary questions; classically, this means they may be defined by an infinite series of mathematical questions. But suppose we let one of the questions be: does the Russell set belong to itself? Then a dialetheist may well want to answer both yes and no. We could also replace a commitment to any absolute inconsistency with an acceptance of relativized questions, and ask: according to naive set theory (or, say, Zach Weber) does the Russell set belong to itself?[11]

We could pick any specific part of the tree to apply this idea, but for uniformity's sake let us postulate that the addition is made at *every* node (except the root) whose associated sequence involves fewer than n inconsistent coordinates, i.e. i's. Every such sequence can be extended to any longer sequence replacing some of its classical coordinates with i's; once the maximum number of i's is reached, all nodes from then on will split classically, i.e. they will have exactly two immediate successors each. In the $n = 1$ case, this means that the new nodes will correspond to all finite sequences containing exactly one i, except the ones *ending* in i.[12] Here is a sketch:



Denote the resulting partial order by $2_n^{<\omega}$, and denote the "expanded" Cantor space naturally arising from it by 2_n^{ω}. So, what topology does such an order induce on the space? For simplicity, I will focus on the case $n = 1$. First of all, note

[11]If the reader has a problem with both inconsistency and relativism in maths, I am frankly surprised they got this far into *this* thesis, and will admire their patience while nevertheless giving up on catering to them.

[12]It wouldn't change anything to add those nodes as well. They are redundant as far as the induced topology goes.

that $2_1^{<\omega}$ is *not* a subgraph of $3^{<\omega}$, insofar as there are cycles all over the graph. Inconsistent nodes are born out of consistent nodes, so we cannot just treat i as a "third value". This does not prevent the usual topology from working: the family of set N_p associated to the graph (i.e. so that p is any finite sequence with at most one inconsistent coordinate and a classical end coordinate) is still a base.[13]

Say that x is an *inconsistent variant* of y (and that y is a *consistent variant* of x) if they only differ in one coordinate which is i in x. The expanded Cantor space 2_1^{ω} has the following properties.

- It is not T1: neigborhoods of a consistent sequence are also neighborhoods of all of its inconsistent variants.

- It is T0: pairs of sequences that are both consistent or both inconsistent can be separated as in the classical case, while inconsistent sequences can be separated from any consistent sequence by simply taking a neighborhood generated by an inconsistent fragment.

- Since it is not T1, it cannot be metrizable; since it is T0, it cannot be pseudometrizable (otherwise it would be metrizable).

- It is still perfect, second-countable (we only added countably many sets to the base), and compact. For the last point, note that given an open covering of 2_1^{ω} we can cut it down to sets of the form N_p with p consistent (because consistent sequences can only be reached by neighborhoods of consistent fragments, which in turn will cover any inconsistent variants for free); and then by compactness of 2^{ω} cut it down to finitely many such sets.

- It is no longer zero-dimensional; in fact, it is connected! To see this, suppose there is a nonempty proper clopen C. We can assume it contains at least one consistent sequence (if not, consider its complement). Now, for every consistent sequence in C, since C is open it must also contain all of its inconsistent variants. Since C is clopen, this is also the case for the complement of C. Hence to preserve disjointness C must be closed under the relationship of "having the same inconsistent variant". But this means C contains all consistent sequences, and therefore all sequences, contradiction.

Hence, 2_1^{ω} is a T0 second-countable compact non-pseudometrizable perfect connectification of the Cantor space.

Now, the lack of pseudo-metrizability may seem a bit disappointing. But in fact, we can recover some notion of distance and offer a new interpretation of the

[13] While it is still the case that finite intersections are redundant, it is no longer the case that either $N_p \subseteq N_q$ or $N_p \supseteq N_q$ or $N_p \cap N_q = \emptyset$. For example, $N_{\langle 0 \rangle} \cap N_{\langle 1 \rangle} = N_{\langle i,0 \rangle} \cup N_{\langle i,1 \rangle}$.

relationship between sequences in the process. Let d be the usual metric on 2^ω, and define a map $d' : 2_1^\omega \times 2_1^\omega \to [0,1]$ as follows:

$$d'(x,y) = \begin{cases} d(x,y) & x, y \text{ consistent} \\ \min\{d(x,z), d(x,z')\} & x \text{ consistent}, y \text{ inconsistent variant of } z, z' \\ 0 & x \text{ inconsistent}, x = y \\ 1 & x \text{ inconsistent}, x \neq y \end{cases}$$

This map only lacks symmetry in order to be a pseudometric on 2_1^ω, i.e. it is a *hemimetric*. Note that $d'(x,y) = 0$ if and only if $x = y$ or y is an inconsistent variant of x. This hemimetric is compatible with our topology in the specific sense that finite intersections of open d'-balls with *consistent* centers constitute a base for it.[14]

Hemimetrics have some nice applications. Following [Coppola and Gerla, 2014], let $y \leq x$ if $d'(y,x) = 0$, and let the *diameter* of a sequence x be $\delta(x) := \sup\{d'(x_1,x_2) : x_1, x_2 \leq x\}$.[15] Note that consistent sequences have diameter 0, while inconsistent sequences have diameter 1.[16] Using this, one can check that $(2_1^\omega, d')$ is a *hemimetric space of regions*, meaning that for every x, y:

(1) $|d'(x,y) - d'(y,x)| \leq \delta(x) + \delta(y)$

(2) for every $\varepsilon > 0$ there is $x' \leq x$ such that $\delta(x') \leq \varepsilon$.[17]

This ensures that we can define a natural fuzzy part-whole relation $incl(x,y) := 10^{-d'(x,y)}$. In particular, two consistent sequences x, y only differing in one component come out as true parts of their common inconsistent variant z, i.e. $incl(x,z) = incl(y,z) = 1$, while inconsistent sequences are as far as possible from being contained in any other.

5.5 An application: gender through the Cantor space

Inconsistent mathematics can lead us to new and potentially liberatory interpretations of old concepts from outside of mathematics. There are at least two

[14] This is because open d'-balls with consistent centers coincide with the sets of the form N_p with p consistent, precisely as in the classical case.

[15] It immediately follows, from the fact that d' is a hemimetric, that $\leq$ is a preorder.

[16] If x is consistent, then $d'(y,x) = 0$ if and only if $y = x$. If x is inconsistent, take $x_1 = x$, and x_2 consistent variant of x.

[17] To prove (1), note that if x, y are both consistent then the left side is 0, while if either is inconsistent the right side is at least 1 (and the left side is always at most 1). To prove (2), if x is consistent take $x' = x$, otherwise let x' be a consistent variant of x.

ways to do this: the first way is to start from a somewhat classical mathematical interpretation of the concept, and then apply mathfucking; the second way is to just throw the concept in an unfamiliar mathematical context, and see what kind of contradictions arise from it.[18] Here I am going to try both strategies in reverse order.

Mathematical visualizations of the space of gender possibilities are not very hard to imagine. A strict gender binary corresponds to a classical two-element set. Recall from Section 1.8 the [Dembroff, 2019] model of (dominant) gender with its four axes: binary, biological, teleological, and hierarchical. We can clearly see the binary axis encoded in the set $\{0, 1\}$: it is given by 0 and 1 representing two exclusive and exhaustive possibilities. The biological and teleological axes are accounted for by the fact that the model includes nothing more: anything gendered has to fall under 0 or 1, so 0 will encompass everything a man is (or ought to be) - including having certain sexual characteristics - and similarly for 1. What about the hierarchical axis? Both the trivial and the discrete topology make no distinction whatsoever between 0 and 1. However, the so-called *Sierpiński space* gives us an intermediate possibility: let $\{0\}$ be open, but not $\{1\}$. Then 1 is only defined as a negativity, i.e. as what is not in the proper neighborhood of 0.

So, we have our classical model of the dominant conception of gender. What now? Sometimes we hear that gender is, rather than a strict binary, a continuum: this perspective maps gender onto the real interval $[0, 1]$, with the numbers between 0 and 1 corresponding to "intermediate" genders between man and woman. The binary axis is apparently lost; however, the logical relationship between 0 and 1 reappears between any element and its complement. Again, the biological and teleological axis can be understood as implicit: for example, the continuum may represent biological differences in sex characteristics, with teleological associations being derived from them.[19] The hierarchical axis is not captured by taking the subspace topology inherited from the real line, but it can be easily implemented via the order topology: let the basic open sets be all those of the form $\{x : x < r\}$ with $r \in (0, 1)$. Again, 1 can only be defined as a negativity, as

[18]This second way seems to be what [Wagner, 2017] has in mind: *"Perhaps, then, we should encourage mathematicians to explore conceptions that are feminist or queer. Perhaps we should encourage social and exact scientists to carry their latent and explicit ideological commitments through mathematics' obscure transformations. Perhaps this would lead us to explore new semiotic possibilities for confronting the impossible impasses in our ways of speaking gender and/or science. Perhaps encouraging signifiers to cut across discursive systems where they do not, supposedly, "belong," does have some therapeutic potential for our contemporary social malaise."* (pp.126-127). It is to these words that I owe my inspiration for much of this dissertation.

[19]An alternative option, reducing the severity of the biological axis, might be to let the continuum be mapped to *all* possible combinations of gender features, biological or otherwise. However, this makes it really hard to understand where the linear order is coming from.

what is not in any proper neighborhood of 0. Meanwhile, 0 is the *only* element that gets a fully positive definition, as the intersection of all neighborhoods: everyone else can only be recognized insofar as they fail to be in some neighborhood of it. We could even go further, and let $\{0\}$ be the only proper nonempty open set: then we have homogenization in the sense of [Plumwood, 1993b], as all genders different from "pure man" become (topologically) indistinguishable.[20]

All of this is very classical. So let's try something wilder. What could it mean to say that gender is a *Cantor space*? Well, remember that under the questionnaire perspective the Cantor space can be identified with possible answers to an infinite series of binary questions. We could think of it as a gender assessment questionnaire, where the questions are ordered by importance: the first question will determine a side, and the rest will tell where one sits between the center and the extreme. The questionnaire can be relative to a society, in the sense of providing the criteria by which society assigns gender to individuals; or it could be relative to individuals, providing the criteria by which they arrive at their self-identification.

Nowadays, a common first question concerns genitals, as in most Western societies that is the determinant (at birth) of what goes into legal documents.[21] The following questions could involve anything from hair length to favorite toys - after all, most things can be (and often are forcefully) gendered. The questionnaire being infinite is of course an idealization; but note that the topology only ever takes into account a finite amount of information at a time anyway, in the form of (finite) initial segments. Hyperseparation in the sense of [Plumwood, 1993b] is well enforced here: individuals giving different answers to the first question are as separated as they could possibly be, since this entails having *no* basic neighborhoods in common. Furthermore, at every step, the two admissible answers exhaust the possibility space. So the binary axis is more than accounted for.

What about the other axes? As usual, the hierarchical axis does not seem to make an appearance under the standard topology.[22] If we understand each sequence - i.e. each set of answers - as determining a gender, then the teleological axis is accounted for; but not so the biological axis, since while it is the case that a difference in assigned sex necessarily corresponds to a difference in gender, there are plenty possible genders for each assigned sex.[23] Meanwhile, if we understand *side* as gender, then the Cantor space actively contradicts the teleological axis: individuals belonging to the same gender are free to give different answers further

[20] In fact, both topologies induce the Sierpińksi topology on the subspace $\{0, 1\}$.

[21] Or at least, that is the *intended* determinant. This is what led to practices of surgical "correction" in perfectly healthy intersex babies. See e.g. [Bettcher, 2015].

[22] One way to reintroduce it would be to take the topology induced by the product of countably many Sierpiński spaces.

[23] One way to reestablish the full biological axis would be to have *all* questions be about biology.

along the line. We could then think of the two-element set as being the restriction of the Cantor space to the all-0s sequence and the all-1s sequence, with all other sequences being dismissed as failures to behave like one's gender demands. But then the biological axis is back with a vengeance, since it provides precisely the side-determining question. One way to counteract this is, of course, to just change the questionnaire: to not include the question about genitals, or at least not have it be the first question. Now, first of all, the biological axis remains unchallenged if the replacement question is about, say, chromosomes or weight at birth. But even if the replacement question has nothing to do with biology, we are still essentially pinning gender on a *single* feature of a person; in other words, the biological axis would risk being replaced by a different axis which may end up fulfilling basically the same oppressive role. This may be avoided by having the first question be about one's own felt gender identity; but then we have a problem because the set of answers to *that* question is hardly binary.

So let's go back to an identification of genders as sequences, and try mathfucking instead. Carrying the identification to inconsistent Cantor spaces appears to have interesting, and possibly liberatory, effects right off the bat. In 2^{ω}_{A}, questions with index in A become inconsequential to the final evaluation: for example, if $0 \in A$ then someone's genitals no longer have a role in determining what their gender is. Note that this is different from removing the question from A altogether: rather, 2^{ω}_{A} captures the fact that a feature may be *part of* our gender without necessarily having a say in *determining* it. In other words, a feature may always be gendered, while at the same time it may be up to each of us to decide *how* it is gendered.[24] Hyperseparation is also defeated: as long as $0 \in A$, *everyone* shares at least one basic neighborhood with someone on the other side; in fact, it doesn't even make much sense to talk of the "other side" any longer, which is quite the blow to the binary axis - and to the hierarchical axis, had we implemented it. Both the biological and the teleological axes are now subverted by the fact that the same answer can be taken by different individuals to have different implications for their gender.

On a different note, 2^{ω}_{n} allows for n yes-and-no answers, accounting for the obvious fact that most attributes are not actually binary and individuals do not necessarily fall neatly on one side or the other.[25] These answers still count toward the result, insofar as they point towards *new* genders; furthermore, the gender

[24] For example, as [Bettcher, 2013] notes, it is quite common in trans communities to gender one's genitals in counter-cultural ways.

[25] I take it that for sufficiently large n this should be enough to cover everyone's needs; after all, the idea that the questionnaire is infinite is itself an idealization. Of course, one moral of queer incomaths is that such assumptions must always be open to rebuttal. Still, this doesn't mean I have to do all the subverting myself.

matching the sequence $\langle i \rangle^\frown s$ will have as its parts both the identity matching $\langle 0 \rangle^\frown s$ and the identity matching $\langle 1 \rangle^\frown s$, so the relationship is more complex than a mere third-gendering, as would occur for example in straightforwardly modelling an extra "both / don't know / don't care" answer in 3^ω. The inadequacy of 3^ω - and of any n^ω, for that matter - in offering a truly liberatory perspective is helpfully acknowledged by classical mathematics itself, since 2^ω is homeomorphic to n^ω for every positive n.[26]

Now, the reader may be wondering: was the move to 2^ω_n not already encompassed within the simpler, consistent continuum perspective? Does the continuum not already offer *more* possibilities, given that the Cantor space is a proper subspace of $[0,1]$? Well, first of all, $|2^\omega_n| = |\mathbb{R}|$, so (Cantorian) size is hardly the issue here. Furthermore, the Cantor space dispenses with linearity, which is an assumption that can be hard to make sense of: it certainly does not seem the case that anyone's gender can be neatly pinpointed on a line, or that there is only one way to not univocally be on one side (namely being $1/2$). Now, could we not just multi-dimensionalize the questionnaire idea to infinitely many continua, hence to the so-called Hilbert cube $[0,1]^\omega$, without bothering with inconsistencies?[27] Well, maybe; but in the end the same problem reappears, that it seems very artificial to answer a real value from 0 to 1 to any given question. Identity is not so easily quantifiable.[28] Furthermore, the part-whole aspect of gender identities captured by 2^ω_n would still be lost, along with its deeper rejection of the binary axis.

A few final notes. First, nothing prevents us from adopting a mixed form of 2^ω_n and 2^ω_A: it is plausible that some questions should stop mattering, while others should allow for yes-and-no answers. Second, I do not of course claim that the interpretation I gave of these structures was the only possible one - it took a remarkable amount of self-restraint to not include three times the amount of footnotes. I also do not claim that these models cover all bases: this was a mere example of how inconsistentizing a classical model can show new possibilities, not an actual proposal for a complete mathematical classification of gender.[29] Third, I do not think it was *necessary* to go through the idea of inconsistency to get to these perspectives; but I also do not see any particular value in throwing away the ladder. Furthermore, remember that classically there is but one Cantor space - and, for that matter, one strict gender binary. So insofar as these models contradict the standard view, they themselves constitute an inconsistency.

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[26] Sadly I will not be able to discuss here what it could mean to say that gender is a *Baire space*, i.e. ω^ω.

[27] This is basically the many-strands model defended in [Daly, 2017].

[28] See [Collins, 2021] for an attempt to thwart this objection by using fuzzy, rather than classical, logic.

[29] Completeness would be an illusion anyway. I didn't call this queer incomaths for nothing.

5.6 Conclusion

In this section we have seen that there are many ways to inconsistently reinterpret the same classical structure without having to be particularly formal about it. We can collapse different elements, in both transitive and non-transitive ways; we can turn some of the pieces in the construction inconsistent and see how the inconsistency spreads; we can add extra pieces and let them generate new inconsistent entities; and surely much more. Inconsistency can motivate certain classical properties, e.g. connectivity or lack of T0, in contexts where they usually do not belong; and depending on how the inconsistency is read, we may see the properties themselves as holding inconsistently of the Cantor space, under the perspective that the inconsistent variants of 2^ω are just as much *the* Cantor space as 2^ω itself.

Finally, we have seen how carrying a non-mathematical concept - in this case, gender - through inconsistentization, by first providing a mathematical model and then mathfucking the model, may lead to new insights, and even to the discovery of new conceptual possibilities.

Chapter 6

Conceptions of inconsistent mathematics

In Chapter 4 I zoned in on a characterization of inconsistent mathematics as a particular kind of *activity*. In this chapter, I am going to expand on this by shifting the focus from theories to *practices*, and explore the many ways in which such an activity can be framed in terms of practices. My hope is that the variety of perspectives may prove fruitful in better understanding inconsistent mathematics' place in the world.

To start with, I will use the Framework-Agent pairs of [Ferreirós, 2015] to characterize the many directions inconsistentization can take, sketch a general classification, and showcase how the practices of inconsistent mathematics are indeed different from those of mainstream mathematics. I will then discuss the roles of inconsistent mathematicians in guiding and delimiting the field. I will rely on this classification to tackle two related, though distinct, questions: whether inconsistent mathematics can be thought of as genuinely *alternative*, and whether it can be thought of as a *revolution*. In particular, I will show that there is a sense in which queer incomaths can be seen as both.

6.1 Inconsistent practices

One upshot of the discussion in Chapter 4 was to bring to the forefront the figure of the inconsistent mathematician. If the core of inconsistent mathematics is (inconsistent) reinterpretation, it matters whose interpretation we are considering at any given point. Something belonging to inconsistent mathematics is at least in part a function of the practitioners involved. Fortunately, the philosophy of mathematical practice has long been looking for ways to incorporate the role of practitioners into the analysis, and can provide us with some helpful tools. In Chapter 1 I introduced Philip Kitcher's notion of practice, which takes into account the shared metamathematical views of its practitioners; here I will adopt the more refined approach of José Ferreirós, which distinguishes more clearly between formal and interpretive aspects of a practice.

According to [Ferreirós, 2015, chs.2-3], mathematical practice should be analysed in terms of *Framework-Agent pairs*. Frameworks incorporate the *symbolic* and *theoretic* elements of mathematical practices: shared languages and formalisms, bodies of theorems and proofs, problem statements, etc. On the other hand, agents - which may be individuals, but also communities, research schools, etc. - *interpret* the frameworks through both their particular cognitive skills, e.g. counting practices or linguistic competence, and metamathematical views, concerning e.g. proof standards or the scope of mathematics. It is the agent's participation in other (strictly speaking) non-mathematical practices that allows them to give meaning to the framework, and this cannot be encoded in the framework itself. *"The formal systems only come to life, so to speak, when they are interpreted in connection with a network of practices—only then can they be said to incorporate or codify knowledge"* (p.42).

Generally speaking, a mathematical practice is constituted by the *interaction* of different Framework-Agent pairs. *"The Framework–Agent pair is not to be identified with a mathematical practice, but is at the core of practice, and of the production and reproduction of knowledge. [...] mathematics in practice will typically depend on the performance of several Framework–Agent couples, intertwined in several possible ways"* (p.44). Still, for the purpose of looking at a field as small and varied as inconsistent mathematics - and identifying possible directions - it will be useful to also consider singular pairs as limit cases of practices.[1]

Following [Kitcher, 1984, chs.7-8], we can take a framework to contain (among other things) the following elements:

- a set S of accepted statements, including not only theorems (i.e. statements which have been proven) but also e.g. axioms or conjectures;[2]
- a set R of accepted reasonings, including not only proofs but also e.g. checking procedures and unrigorous, analogical, probabilistic, or inductive

[1] To be clear, this *is* an oversimplification. Even assuming the existence of inconsistent pairs disconnected from any of their colleagues, in any society where classical mathematics reigns there are going to be interactions between any given inconsistent pair and various classical pairs.

[2] S is usually not closed under logical consequence, not even if we restrict our attention to theorems. It is clearly false that Frege accepted every sentence before he noticed the paradoxes in his system.

reasoning;[3]

- a set Q of accepted questions, i.e. those open problems taken to have intrinsic or instrumental worth.

It is important to note that the distinctions between members of these sets, e.g. the value of a given question, are not themselves part of the framework, but they are determined by the agent. The same framework could generate quite different practices in the hands of different agents. For example, whether inductive reasoning should count as proof may vary between agents, and this in turn could lead to different agents calling different statements theorems, and so on.[4]

These distinctions are used by Kitcher to demystify many alleged historical examples of inconsistent mathematics. For example, the distinction between proofs and unrigorous reasonings makes it possible to treat the method of infinitesimals as both not proof-worthy - because of a lack of geometrical or kinematic interpretation, a necessary condition according to the Newton school - and accepted - because it confirmed older results and provided sensible new ones. Similarly, Euler's manipulations of infinite series did not generate contradictory theorems, only conjectures that could then be independently verified by proof-worthy methods. No inconsistency was accepted, no inconsistent concept was believed in, and no reasoning potentially leading to contradictions was taken to be a valid proof method.

So, in which sense do inconsistent practices the likes of which I have been discussing differ from these? I tentatively claim that a Framework-Agent pair is *inconsistent* if at least one of the following holds:[5]

(S) S contains mutually inconsistent statements;

(R) R contains some nontrivial *proofs* which make use of mutually inconsistent statements, but without either of them being the main hypothesis of a reductio or proof by cases;[6]

[3] I ignore here the distinction between proofs and "proofs" with fillable gaps (*enthymematic gaps*, in the terminology of [Fallis, 2003]), which technically are also unrigorous.

[4] I agree with Ferreirós that one *"should resist temptations to over-generalize and introduce an all-inclusive perspective of the kind of [...] Kitcher's "practices.""* (p.44). In this spirit, I mention in passing that I don't think it is a *given* that a mathematical framework requires S, R, and Q to be non-empty, or that the way I have subdivided them always makes sense. Still, this won't be an issue for the examples I am going to discuss.

[5] I take this to be only a *sufficient* condition: it will do to cover all the examples from Chapter 3, but I do not want to foreclose other possibilities relying on features of pairs which I haven't taken into account.

[6] The caveat is an attempt to exclude perfectly classical uses of inconsistent statements. Note that the condition does not require that the statements used in the proof be in S. I will show some examples in a bit.

(Q) Q contains some questions of *intrinsic* worth about inconsistent concepts.[7]

I will call any witness of condition (X) a *type-X inconsistency*. Note that the satisfaction of these conditions is not encoded within the framework, but depends on the associated agent: for the reasons discussed in Chapter 4, it is the agent that determines whether two statements are mutually inconsistent or whether a concept is; furthermore, it is the agent that determines whether a piece of accepted reasoning is a proof, and whether a certain question has intrinsic worth. This is not to say that within a given practice there can be no shared deterministic criterion for, say, inconsistency; the point is simply that the idea that such a criterion determines inconsistency cannot be intrinsic to the framework, and could vary across different practices.

We can now say that a practice is *inconsistent* if it essentially involves an inconsistent Framework-Agent pair; a pair is *essentially involved* in a practice if it cannot be replaced by a consistent one without affecting the practice.[8] Essentiality is useful to prevent the occasional quirky belief of an agent from characterizing the practice at large. For example, if we follow [Vickers, 2013, ch.6] in understanding Johann Bernoulli to be a true believer in inconsistent infinitesimals, his interpretation of the early calculus framework was inconsistent and thus so was *his* individual practice; however it does not seem like his beliefs were particularly essential to his way of working, insofar as the vast majority of the mathematical community was happy to do the same kind of work without relying on such a belief. So it seems fair to not let the inconsistency spread to the practice of early calculus more broadly conceived.

To test these definitions, let us check that the presence of a type-x inconsistency (for any x) suffices to turn a practice nonclassical. This is quite straightforward for (S): classical mathematics never explicitly accepted contradictions, insofar as it always fought to get rid of them when they popped up.[9] Concerning (Q), while classical mathematics may have occasionally relied on inconsistent concepts (e.g. naive sets and infinitesimals), those were never the main object of inquiry themselves, which is why they could be replaced or revised without loss once a better option was found. What matters in (R) is the emphasis on proofs: it is not uncommon in the history and common practice of mathematics to detour through inconsistency (or possible inconsistency) and come out consistent, but this

[7]The focus on intrinsic worth is meant to exclude an interest in "solving" the inconsistency, or replacing the concepts in question with consistent ones.

[8]I take this to generalize my final definition of s-inconsistent theory from Section 4.4.

[9]Note that there is a difference between a *practice* containing mutually inconsistent statements, and a *pair* containing mutually inconsistent statements. The practice of classical set theory, for example, contains several incompatible theories; but they either belong to different pairs, or belong to a pair where they can be relativized for meta-theoretical study so the inconsistency vanishes.

is precisely what makes some reasonings unrigorous; proofs have always been required to proceed consistently, and the acceptance of unrigorous reasonings is predicated on the assumption that they could be turned into or suggest proofs. Thus, there is a clear sense in which inconsistent practices as I have defined them are not classical. Again, this does not mean that there has never been an inconsistent Framework-Agent pair before the last 70 years; my point is just that inconsistent mathematicians appear to have never been essential - at least, qua inconsistent - to the larger practices they were embedded in.[10]

Let us spell out how the inconsistent status of some of the practices we have discussed appears from the viewpoint of this framework.[11] Let us start from the dialetheic mathematics of [Weber, 2021a].[12] The entire work is driven by the desire to mathematically ground dialetheic metaphysics, and therefore to answer questions about concepts which Weber takes to be inconsistent, e.g. set and boundary (Q). Some contradictions - e.g. the classical paradoxes - are theorems, insofar as they are derived via **subDLQ** from the axioms of naive set theory (S); these inconsistent theorems are used in proofs to derive further theorems (R). All conditions are satisfied.

The antinomic mathematics of [Asenjo, 1989] and [Asenjo, 1996] is another example of inconsistent practice satisfying all conditions.[13] Every branch starts with questions concerning an antinomic variant of of some consistent concept, e.g. set or number (Q). Some contradictions expressing the basic properties of such antinomic concepts are postulated in, e.g. antinomic numbers being strictly less than themselves (S); they can then be used in proofs to derive theorems (R), although the appropriate logic for proofs is not always known beforehand (e.g. only the positive fragment is fixed in [Asenjo, 1996]).

The study of impossible pictures in [Mortensen, 2010] is slightly less radical.[14] The goal is to capture the cognitive inconsistency generated by impossible pictures (Q). This is cashed in by taking faithful descriptions to contain some mutually

[10]There is a certain risk in drawing such conclusions: insofar as explicitly accepting contradictions was never popular amongst mathematicians, it would not be too surprising for fruitful instances of acceptance to be forgotten by recorded history - for their contribution to be minimized, or erased altogether. This is a question for the historians. Still, my point remains more or less unchanged: if the contribution of inconsistent Framework-Agent pairs was systematically hidden away, then such hiding is a fundamental part of classical practices, which then serves as a further way to distinguish them from inconsistent ones.

[11]I do not want to claim that what follows is the *only* possible reading of these practices. It may be that some of the agents involved in these practices understand them differently; but I am less interested here in exegesis than I am in showing that certain kinds of inconsistent practices could exist (insofar as there is at least one agent interpreting the frameworks this way, i.e. myself).

[12]This was discussed mainly in Section 3.1.

[13]This was discussed in Section 3.2.

[14]This was discussed in Section 3.6.

inconsistent statements (S). However, these statements are obtained consistently, and they are the end goal; they are not themselves used in further proofs (¬R). Still an inconsistent practice, but slightly less so.

In the model theory of paraconsistent logics, inconsistent models are in principle no different from any other nonstandard models. Theorems are mutually consistent statements about the models (¬S), and proofs use the consistent tools of model theory (¬R).[15] Now, inconsistent models may feature in a Framework-Agent pair as mere instruments - e.g. for the sake of non-triviality proofs - in which case we may conclude (¬Q): they are but a tool to prove something about what interests the Agent, which may well be consistent. However, this kind of pair is likely to eventually become embedded in (or spawn) a practice where the goal of the enterprise is to study inconsistent models in their own rights; then, insofar as inconsistent models are so called because they *represent* inconsistent mathematical structures, we have (Q). Eventually, some standard or canonical model might be identified amongst the inconsistent ones, in which case we would arguably have (R) and (S) as well, insofar as what can be proven in the model starts being identified with what is accepted in the practice sans qualification.

Maybe more controversially, I have argued that it makes sense to consider [Meyer, 2021a]'s relevant arithmetic $\mathbf{R}^\sharp$ an inconsistent practice as well.[16] Theorems are mutually consistent statements derivable in the logic **R** from an appropriate version of the Peano axioms (¬S). However, some of these theorems are conditionals with false antecedents; and their proofs are grounded in contradiction, since we know the antecedents are false and we are looking at them from the perspective of what we know is true (R). Another way to put this is that what happens within inconsistent models is an integral part of $\mathbf{R}^\sharp$, even if it only shows up within its conditional theorems. The fact that conditional theorems play this role is of course true in general, but $\mathbf{R}^\sharp$ is special insofar as the question of what follows from the false is actively pursued *and* has a nontrivial answer, unlike e.g. in PA. Insofar as we can frame this as a question about what happens in inconsistent models, we could say (Q) holds as well.[17]

It is worth noting that the proposed conditions are in principle mutually independent. First, we can have practices where accepted statements are consistent and not about anything inconsistent, yet they are proven using

[15] Assuming the metalanguage is classical, as it is in most of the literature (e.g. everything discussed in Section 3.3); if not, as in e.g. [Badia et al., 2022], then we might have (R) or (S) as well.

[16] See Section 3.4.

[17] By the same lights, (Q) appears to be satisfied by work like [Arruda and Batens, 1982] and [Carnielli and Coniglio, 2013], where the existence of inconsistent objects is always purely hypothetical.

inconsistent statements that are not themselves accepted. An example might be the Chunk&Permeate version of the calculus from [Brown and Priest, 2004], where both the statement that infinitesimals are zero and the statements that infinitesimals are nonzero are used in obtaining the output (R), but the statements are not themselves accepted (¬S), and in fact no questions are asked about infinitesimals themselves, since they are not the real object of study (¬Q).[18] On a bigger scale, we can imagine a whole new conception of mathematics where consistent consequences are all that really matters even if the methods used to get them can be inconsistent and carry proof status.[19] Presumably this would quickly evolve into a larger practice involving many other pairs interested in studying the inconsistent methods themselves, but this needs not influence the pairs focusing on the consistent goals.

Second, a practice can accept some inconsistent statements - as theorems, even - without any proofs relying on inconsistent statements, and without inconsistency being an object of study. This might be the case for the Normalized Naive Set Theory in [Istre, 2017]: contradictory statements about e.g. the Russell set can be derived (S), but by design they cannot themselves be used *together* in further proofs, because any such derivation would be non-normal and therefore lack proof status (¬R).[20] We could also imagine the driving purpose of NNST to be the mere grounding of classical mathematics, with no interest in the inconsistent sets themselves (¬Q).

Finally, a practice might be about inconsistent concepts without there being any mutually inconsistent statements in what is accepted or in proofs. One example could be the classical model theory of paraconsistent logics, as I have already discussed. Consider also the case of impossible pictures: early theories of them accept no contradictions (¬S), let alone make use of them in proofs (¬R), but the subject matter - which the driving questions refer to - may still be understood as inconsistent, as exemplified e.g. by [Cowan, 1974] presenting his work as an *"analysis of impossible figures"* (Q). Now, any such pair might be open to a reading in which the inconsistency is *explained away* by the consistent description; but this is not necessary.[21]

[18]This was discussed in Section 2.6. Note that this is a typical way in which inconsistent mathematics can be applied to the world without requiring dialetheism, according to [McCullough-Benner, 2020].

[19]This is somewhat analogous to Hilbert's formalism, where meaning is reserved not to the consistent as opposed to the inconsistent, but to the finitary as opposed to the infinitary.

[20]This was discussed in Section 3.1.

[21]Bridging principles explaining the connection to what is being modelled - which should well be accepted - may be thought to necessarily bring in mutually inconsistent statements in the case of an inconsistent subject matter, but I think this might be avoided. For example, the inconsistency to be modelled may be ineffable.

If we wanted to rank practices depending on how inconsistent they are, we could assign an *inconsistency degree* to a Framework-Agent pair counting the number of conditions it satisfies. So, for example, both the Chunk&Permeate calculus and Cowan's theory of impossible pictures have inconsistency degree 1, while Weber's work has inconsistency degree 3: this tracks with the perceived radicalness of the latter approach, and the reticence to count the former as inconsistent mathematics. We could then say that the inconsistency degree of a practice is the ratio of inconsistent over total pairs (inconsistent pairs being weighed by their degree) belonging to the practice. To further refine the classification, we could also count the number of witnesses: intuitively, a practice involving one inconsistent theorem is less inconsistent than a practice involving many. One might also want to weigh the conditions differently, and say for example that type-S inconsistencies should have the largest impact on the overall inconsistency of the practice. I leave such considerations for future work.

Before moving on, it may be fruitful to revisit the relationship between inconsistent mathematics and *constructive* mathematics, which can also be seen as related to a certain kind a practice: namely, one that requires *proofs* (but not unrigorous reasonings) to be constructive. We can now see that there is nothing particularly surprising about the asymmetry between the relative agreement on constructive reasoning and the inhomogeneous multiverse of paraconsistent logics discussed in Chapter 2. Constructive practices are defined in terms of a restriction on admissible proofs, which in turn suggests a clear idea of what such proofs should look like, often down to formal syntax.[22] In contrast, inconsistent mathematics is in principle happy to accept any kind of proof: it is fundamentally about semantics, about the interpretation of our practices, and semantics is not obviously reducible to any kind of formal semantics, let alone one that is sound and complete for some particular formal system.[23]

There are also similarities. Neither constructive nor inconsistent practices force an abandonment of classical mathematics: in fact, both can be seen as an *extension* of it. Constructive proofs have potential value for every mathematician, constructivist or not, even if of course the question of whether a certain result can be proven constructively is far more urgent for a constructivist; conversely, results

[22]This was to some degree the case even for old-school intuitionism, despite the lack of (and distaste for) formalization: Brouwer's proof of his so-called bar theorem involves one of the first mathematical applications of what we would now call *proof-theoretic* techniques (see [Posy, 2020, ch.2]). That being said, Brouwer rejected the idea that there could be any formal definition of what an admissible proof is.

[23]Note that this is quite in tune with Mortensen's thesis that inconsistent mathematics differs from classical and constructive mathematics insofar as it extends, rather than restrict, the logical space. See Section 1.2.

proven via nonconstructive means routinely serve as an inspiration for constructive refinements or variations.[24] Similarly, in classical mathematics inconsistent reasonings are often used to obtain consistently verifiable results, and intuitively inconsistent concepts can be a first step towards consistent practices, as with the inconsistency of naive sets eventually leading to ZFC and the iterative hierarchy. More generally, most if not all work in inconsistent mathematics seems to be in principle open to consistentization, so classical mathematics can make good use of it. Conversely, from the viewpoint of inconsistent practices, the classical theorems and questions are a perfectly valid starting point. So the three different kinds of practices, far from being at war, can fundamentally support each other.

Another thing that all these practices have in common is that what determines their membership to a certain class (i.e. classical, constructive, or inconsistent) is not situated, strictly speaking, in the mathematical text; rather, the practitioners determine the practice, and in particular determine where the practice sits. A constructive proof can still be a proof in all kinds of practices; it is the priority given to that kind of proof that makes the practice constructive. Similarly, an inconsistent piece of reasoning can be accepted in all kinds of practices: if it is taken to be a proof then the practice is inconsistent, otherwise it is not.

6.2 Inconsistent agents

Is there such a thing as *the* practice of inconsistent mathematics? We could take it to be constituted by the web of all inconsistent Framework-Agent pairs floating around in the last fifty years.[25] As incompatible as different projects may have been, certain methods have been around the block, and results from one pair have often been repurposed in pairs completely different in both framework and agent. Think of the finite models of arithmetic: they went from being a mere tool for proving nontriviality of $\mathbf{R}^\sharp$ in [Meyer, 1976], to being the object of model-theoretic investigation in [Meyer and Mortensen, 1984], to being a potential replacement for the standard model in [Priest, 1994] and [Van Bendegem, 1994], to finally being treated by [Weber, 2021a] as something that needs to be *excluded* in order to preserve the kind of reasoning dialetheic mathematics needs.

Such an all-encompassing perspective may well be the right way to go for the purpose of socio-historical analysis. Still, this broadly conceived, the field has little to offer in terms of common goals, shared methodological constraints, or even shared accepted results. The landscape of inconsistent practices is simply too

[24] [Bauer, 2017] provides examples in both directions.

[25] Of course these pairs did not come out of thin air, but are related to classical pairs; I set this aside for simplicity's sake.

inhomogeneous, even within what is nominally the same project (e.g. the search for a naive set theory); and there is hardly any prevailing conception.[26] Of course the field is still young, and it might be that eventually at least one somewhat cohesive community - a *canonical agent*, if you will - shall emerge; in fact, this might well be a necessary condition for inconsistent mathematics to break into the mainstream.[27] I am interested in what such a community could look like.

Say that an agent is *inconsistent* if they belong to an inconsistent Framework-Agent pair. Since the inconsistency of a pair depends not only on the framework but also on the agent themself, it is largely up to the agent to identify their pair (and themself) as inconsistent.[28] Furthermore, it may be the case that an agent is simultaneously inconsistent in one practice and not the other: nothing prevents, say, Weber from engaging in classical algebraic geometry as a before-bed hobby. Every agent comes with their own metamathematical views (or lack thereof), which restricts the range of pairs that they will generate: they could rule out some logics, fix the inconsistency of some concepts, etc. The views of an inconsistent agent are then the natural place to look for more pointed conceptions of inconsistent mathematics.

So, what distinguishes a classical agent from an inconsistent agent? From the cognitive point of view, there does not seem to be much of a difference. Granted, maybe a realist who recognizes true contradictory theorems needs to be able to grasp the truth of a contradiction, which might be said to not be a universal skill depending on what we take "grasping the truth" to be. There is certainly a philosophical tradition of claiming that it is literally impossible to sincerely believe

[26] On a personal note, it is just this frustrating lack of an overarching perspective that led to the writing of this dissertation. The idea of just "doing some inconsistent mathematics" - or worse, "finding the right inconsistent mathematics" - sounded more and more meaningless to me the more I learned about the field; until I eventually decided that my time would be better spent building a meaning myself.

[27] There is already a clearer sense of sub-communities in the field of paraconsistent logic, which may be roughly linked to the kinds of logic under investigation. For example, it might be possible to (fuzzily) single out a relevant community, an adaptive community, an LFI community, and so on, where the members of each community share at least some perspective on methodology and goals. But even then, [Aberdein and Read, 2009] make the point that e.g. all kinds of incompatible relevant agents appear in the literature, leading to deep disagreements on what the field should look like.

[28] Agents do not however have all this power on the views associated to the *practices* they partake in. For example, an agent may be inconsistent in a consistent practice, insofar as their pair is not essentially involved. A first approximation of the views associated to a practice may be obtained by taking the intersection of all (pertinent) views of the participating agents, although it's generally not that simple - just because agents within the same practice disagree on something, it doesn't mean that the disagreement is irrelevant to the practice. In fact, a plurality of views may itself be an explicit feature of the practice.

a contradiction.[29] Still, in general I do not see any reason why a classical agent would qua classical lack the necessary cognitive abilities to be an inconsistent agent and viceversa.

The main difference between classical and inconsistent agents lies in their metamathematical views. At a first glance, we have the following views concerning the level at which inconsistencies can be accepted:

- *model-theorists* accept inconsistent nonstandard models;
- *theorists* accept inconsistent theorems;
- *scientists* accept inconsistent applications;
- *foundationalists* accept inconsistent foundations.

Orthogonally, we have the following views concerning admissible ways to reason:

- *monists* take some fixed paraconsistent logic to underlie the best formalization of mathematical reasoning;
- *pluralists* accept any paraconsistent logics satisfying certain conditions;
- *nihilists* put no principled restriction on the logic.[30]

In Chapter 4, I suggested inconsistent mathematics is best characterized as the activity of giving inconsistent reinterpretations. In terms of practices, this could be understood as saying that the core of inconsistent mathematics is not *following* an inconsistent practice, but rather the activity of *turning* practices inconsistent (or, if they already were inconsistent, to add more inconsistency). This could mean reinterpreting either the theories, tools, or goals of a practice, corresponding to the three conditions I gave for inconsistent practices.[31] A third dimension thus concerns when it is admissible to inconsistentize:

- *desperate* agents only inconsistentize when they see no rational alternative;
- *opportunistic* agents are happy to inconsistentize whenever the original practice is struggling by its own standards;

[29] Starting at least from Aristotle: *"It is impossible for any one to believe the same thing to be and not to be"* [Ross, 1953, Book Γ, Ch.3, lines 1005b23 onwards].

[30] A nihilist agent needs not come with a clear definition of logic for the position to be coherent. This is partly because a clear demarcation between logical and non-logical reasoning needs not be particularly relevant to a given practice, and partly because the agent will still be asked to justify their choice in any particular instance. Note that this is just the nihilist attitude from Section 2.8.

[31] One could also frame this "meta-practice" as itself a practice in the usual sense. I just find this way of putting things a bit clearer.

- *curious* agents believe we can inconsistentize whenever we feel like it;
- *righteous* agents believe we should inconsistentize whenever we can.

Note that all of these views (and combinations thereof) are nonclassical, at least to the extent that they are currently not driving any mainstream mathematics. Most of these views are bound to generate inconsistent practices, although not all of them. For example, monism and pluralism may ground a practice of studying *fragments* of classical mathematics; such fragments might have natural inconsistent models, but the models may not be the object of investigation themselves. And, of course, desperate agents may pass as classical for the time being, the only difference being that they are open to the possibility that inconsistency may some day be a rational option.

This classification of views is not meant to be exhaustive, nor are the views themselves complete: for example, theorists may differ on how they understand mathematical truth, and so whether they consider themselves dialetheists or not. Many debates from mainstream philosophy of mathematics are neutral with respect to practice - inconsistent or not - in the sense that disagreement on such issues does not lead agents to produce significantly different kinds of mathematics.[32] For this reason I will stick to the three dimensions above for the purpose of classifying practices.

The relationship between frameworks, agents, and their views can be quite fluid. First, the views held by an agent within a practice need not be adopted by the same agent universally. For example, the same agent may be desperate when it comes to basic arithmetic, but - as a naive set theorist - righteous around set theory; or work with a fixed logic in one practice and any logic whatsoever in another. In particular, being a monist, pluralist, or nihilist agent in no way requires a commitment to logical monism, pluralism, or nihilism as a philosophical position; it depends on what the role of logic is - and what "logic" means - in the practice at hand.[33]

Second, frameworks created for a view may be repurposed towards another view, by replacing the agent in a pair with a different one. As a concrete example, we could think of Mortensen as having replaced Meyer as the interpreter of the framework of nonclassical model theory, switching the main research goal from a conservative grounding of classical informal reasoning to an exploration of inconsistent structures in themselves and their applications to the world. In fact, the same agent could replace themself by simply changing their mind about how to interpret the framework: agents are (usually) not divinely endowed with a fixed

[32] One exception is the constructivism / non-constructivism debate.

[33] And, for that matter, on how we understand the philosophical positions.

neverchanging stance, nor is any given stance necessarily linked with a strongly held belief.[34]

Let us see some examples, noting how different arguments for inconsistent mathematics (in the sense of Chapter 1) fit different views best. The argument from pure mathematics most naturally goes together with curiosity. For example, recall the definition by [Mortensen, 2017] of inconsistent mathematics as *"the study of the mathematical theories that result when classical mathematical axioms are asserted within the framework of a (non-classical) logic which can tolerate the presence of a contradiction"* (p.1). This position is curious, since any classical theory is up for grabs; it is mostly model-theoretic, since classical axioms are consistent and asserting them in most paraconsistent logics will not generate contradictions;[35] and it is is essentially nihilist, since any paraconsistent logics will do.[36] To make it foundational, we could simply focus on classical foundational theories instead; to make it scientific, we could consider the applications of classical theories as well. Asenjo's antinomic mathematics drops the condition that the axioms have to be classical and puts some extra conditions on the underlying logic, therefore it counts as pluri-curious theoretic.

Obviously, the argument from foundations supports foundationalist views. Desperate foundationalism may be the reaction of some agents in the face of a deep foundational crisis, for example Peano Arithmetic turning out inconsistent: accept the inconsistency, and explore ways to make the current practices incorporate it coherently.[37] Pluri-righteous foundationalists may be convinced that we need an inconsistent foundation, so everything should be inconsistentized at least enough to fit in; but many different logics could be adequate. The recent quest for a working naive set theory can be seen under this light.[38] Mono-righteous foundationalists are pretty much the same except they would have a preferred logic.

The argument from subject matter appears to support theoretic and scientific opportunism. It's not that we want inconsistency at all costs, or even particularly care for it; rather, inconsistent theories simply describe some things better

[34]Meyer himself had no problems collaborating with Mortensen on his model-theoretic investigations, e.g. in [Meyer and Mortensen, 1984].

[35]Contraclassical logics might make an exception.

[36]I call this nihilist rather than pluralist because the requirement that the logic be paraconsistent is simply there to guarantee that the theories in question have non-trivial inconsistent models, and thus that the practice is indeed inconsistent.

[37]This is hardly a necessary reaction. One could simply conclude that informal arithmetic needs a different, yet still classical, formalization. Or one could conclude that, say, consistent constructivism is the way to go.

[38]Presumably such an enterprise would not go as far as nihilism, since the conditions brought by the chosen foundational theory and goals would limit the space of available logics (for example, the logic underlying naive comprehension should be able to avoid the Curry paradox).

than consistent theories do. Similarly for the arguments from philosophy of mathematics: consistent theories cannot seem to support certain projects that we might be interested in, like logicism or finitism, so we should turn to inconsistent ones. Such views are most likely monist or pluralist, since the subject matter or projects in question will put some limits on which logics are acceptable; for example, strict finitism needs a logic that is able to support finite models.

The argument from invalidity naturally supports model-theoretic righteous pluralism: the only admissible logics are those which avoid the alleged problem with classical logic,[39] but there is no immediate reason why this should lead to any inconsistent theorems or applications. One example of such a view is the [Plumwood, 1993b] plan of un-dualizing classical logic: any replacement should avoid the five classical laws of dualism, yet there is no push to prove any contradiction.

A righteous nihilistic scientific perspective can be glimpsed in the following quote: *"Perhaps we should encourage social and exact scientists to carry their latent and explicit ideological commitments through mathematics' obscure transformations"* [Wagner, 2017, p.126]. This process is bound to inconsistentize our commitments sooner or later - either internally or with respect to other beliefs of ours - which serves the purpose of challenging bias and suggesting new conceptual possibilities; the view is nihilistic insofar as fixing a logic is an afterthought.

More generally, consider queer incomaths. The argument from liberation suggests that in order to highlight and disrupt mathematics' naturalization of dualisms we *should* inconsistentize as much as possible, aiming not only at real world applications (as Wagner suggests) but also at abstract structures - both because the latter can inspire the former, and because to treat the abstract as inherently classical contributes to the naturalization of dualistic thought. As I argued in Section 2.8, this should make use of whatever logic we find appropriate. Hence, queer incomaths is theoretic righteous nihilistic scientific.

6.3 Inconsistent alternatives

Having distinguished inconsistent mathematics from classical mathematics in both practices and accompanying views, we may want to ask how big the gap really is. There are two popular questions concerning any piece of mathematics breaking

[39] Of course, depending on the invalidity charge, this may also lead in directions that have nothing to do with inconsistent mathematics. This could happen even if the replacement logic is paraconsistent: for example, [Tennant, 2017] takes core theories - based on a paraconsistent "core logic" - to have *no* inconsistent models.

with tradition: whether or not it is a genuine *alternative*, and whether or not it is a *revolution*. I will discuss these in turn.

Against the background of classical mathematics, [Bloor, 1991, ch.6] asks the question: *"can there be an alternative mathematics?"*. Bloor offers no precise characterization of alternativeness, but he does provide a few hints: *"An alternative mathematics would look like error or inadequacy. A real alternative to our mathematics would have to lead us along paths where we were not spontaneously inclined to go. At least some of its methods and steps in reasoning would have to violate our sense of logical and cognitive propriety. Perhaps we would see conclusions being reached with which we simply did not agree. Or we would see proofs accepted for results with which we agreed, but where the proofs did not seem to prove anything at all. [...] An alternative mathematics might also be embedded in a whole context of purposes and meanings which were utterly alien to our mathematics. [...] The 'errors' in an alternative mathematics would have to be systematic, stubborn and basic. Those features which we deem error would perhaps all be seen to cohere and meaningfully relate to one another by the practitioners of the alternative mathematics. [...] The practitioners would have to proceed in what was, to them, a natural and compelling way"* (p.108).

The existence of alternative mathematics is philosophically significant for several reasons. The literature has mostly focused on the way in which alternative mathematics can be evidence for mathematics being relative to social norms;[40] alternativeness here expresses a degree of variation from classical mathematics which is strong enough to serve not only as a counterexample to absolutist philosophies of mathematics, but also as an objection to the teaching of mathematics as unique and non-negotiable.[41] Particular kinds of alternatives have also been argued to provide evidence for logical pluralism and for the historical contingency of mathematics.[42]

What makes any discussion of alternative mathematics difficult is classical mathematics's tendency to either reappropriate any alleged alternative as one of its own, or write it off as worse or pseudo-mathematics.[43] In fact, while I have been using the expression "classical mathematics" throughout this dissertation, this is already a linguistic concession to the non-orthodoxy; the standard picture is that mathematics *just is* classical mathematics, and everything else is either behind

[40] Aside from the aforementioned [Bloor, 1991], see also [Ernest, 1998]. Neither author rejects the idea that mathematics is *objective*; rather, they argue that mathematical objectivity is fundamentally social, a (particular) form of institutionalized belief.

[41] This was emphasized in [Burton, 1995] and [Ohara, 2006].

[42] On the former connection see e.g. [Shapiro, 2014] and [Caret, 2021]; on the latter, see [Van Bendegem, 2016].

[43] This was a big part of the argument from liberation: see Section 1.8.

the times or not really mathematics.[44] This is enforced by taking the features of classical mathematics to just be what constitutes mathematics, so that alternatives are excluded by definition. For example, the orthodoxy demands that any piece of mathematics be interpretable in ZFC for it to be legitimate; but if something is interpretable in ZFC, then in some sense it is classical mathematics after all, and thus not really an alternative. This is not just a definitional matter, but it has very real consequences: it determines what kind of work gets to be published, and imposes a hierarchy of epistemic advantage at the educational level.[45]

Bloor's own examples of alternative mathematics are taken from the history of mathematics. For example, the concept of number went through some very deep changes across millennia: this involved not only the extension of the concept (e.g. the late inclusion of 0 and 1), but also the associated metaphysics, which in Pythagorean times generated mathematical notions like the *eidos* (shape) of a number. We could imagine an alternative history where the Pythagorean conception persisted, so that contemporary mathematicians would still be talking about eidos today. The same proofs have also been interpreted very differently by different cultures: for example, while to the Ancient Greeks the proof that $\sqrt{2}$ is not rational showed that it is not a number, it is now taken to show that it is an irrational number.[46]

The problem with this kind of examples is that it is easy for the absolutist to reject them as historical dead ends or distractions. What the defender of alternative mathematics needs to show is that the rejection was not the inevitable march of progress; simply pointing at different conceptions of mathematics across history does little to show that it could have evolved differently, or that what we have now is not an objective improvement. This applies just as well to the usual alleged historical examples of inconsistent mathematics. Suppose we do take seriously the claim that, say, the early calculus was inconsistent. We may not be "spontaneously inclined" to understand derivatives like that nowadays; but enough mathematicians at the time were, much like they were eventually spontaneously inclined to move on. Rather than calling the early calculus an alternative mathematics, why not think of it as *"just steps on the path to the necessary modern notions"* [Ernest, 1998, p.253]?[47]

[44]According to [Posy, 2020, p.2], the expression "classical mathematics" was introduced by [Brouwer, 1908] as a foil to intuitionism.

[45]On this last point, see e.g. [Tanswell and Rittberg, 2020]. Such systematic delegitimization of alternative ways of knowing is hardly limited to mathematics: on its effect on philosophy, see [Dotson, 2012b].

[46]Bloor also sketches a tale of how some society might conclude from the same proof that $\sqrt{2} = \frac{p}{q}$ where p and q are both odd and even (p.124). Food for thought to the inconsistent mathematician!

[47]A similar worry is expressed in [Van Bendegem, 2016].

After voicing the above worry, Ernest proposes intuitionistic mathematics as a much more straightforward instance of alternativeness. *"Intuitionist concepts from the logical connectives 'not' and 'there exists' to the concepts of 'set', 'spread', and the 'continuum' differ in meaning and in logical and mathematical outcomes from the corresponding classical concepts, where such exist. Intuitionist axioms and principles of proof are also different [...] Intuitionist mathematics has its own body of truths [...] which do not appear in classical mathematics, and it also rejects the bulk of classical mathematics. Finally, since the time of Brouwer, intuitionism has always had a cadre of respected adherent mathematicians who are committed to intuitionism [...] and reject classical mathematics"* (p.253).

Now, claims of incommensurability have sometimes been disputed on the grounds that intuitionistic logic is formally intertranslatable with classical logic: the former is straightforwardly a sublogic of the latter, while the latter is recovered within the former by means of the so-called double negation translation(s).[48] However, *"this argument first of all ignores the different meanings, traditions, and discursive practices associated with these two versions of mathematics, and it identifies them solely with the formal representations of their knowledge. Second, it adopts "formal intertranslatability in principle" as an equivalence relation between such knowledge domains. This approach is so powerful that if transposed to the domain of natural languages it would assert that all languages are essentially the same"* (p.254). In other words, alternativeness lies with *agents*, not with frameworks. It does not matter - and it should not matter, on pains of trivializing the discussion - whether or not the frameworks are formally intertranslatable, as long as the respective agents are interpreting them in substantially different ways.

Can an inconsistent practice be alternative in virtue of its logic? It is clear that not *every* change of logic will mark alternativeness. Paraconsistent logics that take themselves to agree with classical logic in consistent contexts - i.e. most of them - have for the most part been used to suggest *extensions* of classical mathematics rather than alternatives, insofar as classical mathematics has always excluded inconsistency by fiat. Not even the idea that theorems must be provable in some extension of ZFC is really questioned; the only disagreement concerns

[48] Details can be found in [Aberdein and Read, 2009, Sect. 2.1]. These authors do in fact argue that intuitionistic logic is a genuine alternative to classical logic, although they do not extend the discussion to the status of the respective mathematics.

the consistency status of the extension in question.[49] This may mark a change from current practice, but it does little to counteract the standard picture; it may simply be understood as the next rational step in the necessary development of mathematics, and it is in fact generally sold as such.

One might think the situation is different when we consider practices with underlying *inconsistent* logics. However, as I argued in Section 4.2, tautologies may well be inert with respect to the practice; in other words, not every logical truth has to be among the accepted statements.[50] Furthermore, it is possible even for *formal* theories with underlying inconsistent logics to not be themselves inconsistent, as long as the logics in question do not validate Weakening-like principles the likes of $A \to (B \to A)$.[51] So not only do inconsistent logics not force alternative practices; they do not even force inconsistent ones!

This is not to say that a paraconsistent logic *cannot* underlie a genuine alternative. The dialetheic mathematics of [Weber, 2021a] appears to support a straightforward analogy with intuitionistic mathematics. Mimicking Ernest's defense of the alternativeness of intuitionism, we can note that dialetheic concepts from the logical connectives 'not' and 'implies' to the concepts of 'set' and 'continuum' differ in meaning and in logical and mathematical outcomes from the corresponding classical concepts. Dialetheic axioms and principles of proof are also different (naive comprehension, lack of contraction, etc.). Dialetheic mathematics has its own body of truths which do not appear in classical mathematics (e.g. the paradoxes) and it also rejects the bulk of classical mathematics (e.g. the Cantor-Bernstein theorem, and with it the whole theory of

[49]Whether the extension is presented as axiomatic (as in [Carnielli and Coniglio, 2013]) or model-theoretic (as in [Priest, 2017]) is besides the point. Mathematicians work in structures which cannot be axiomatically characterized in a nice formal language *all the time*. This harks back to my argument in Section 4.3 that **LP**-based naive set theory is sufficiently equivalent to ZFC from the perspective of the classical mathematician.

[50]This does not depend on said logical truths being inconsistent: for example, the classical tautology $(A \to B) \vee (B \to A)$ plays no role whatsoever in mathematical practice, and I can at least anecdotally claim that a mathematics degree can be obtained without being aware of it. A similar point is made in [Meyer and Slaney, 1989, p.277]: *"we see little reason to claim that [practical] theories are all regular: there is no more compulsion for physicists or gymnasts to assert truths of logic than for logicians to learn gymnastic"*. I am told this is a "polemical" comment; presumably this signals a dearth of actual objections.

[51]A variety of examples can be found in [Mangraviti and Tedder, 2023]. This requires understanding theories in a non-Tarskian sense, otherwise logical truths are included by definition. But this is quite natural in practice, as the previous footnote suggests; and besides, why should we expect nonclassical practices to stick to a classical notion of theory?

cardinality).[52] Of course the community is still extremely small, but there is no particular reason why this could not change with time. So I would say that Weber's dialetheic mathematics is at least as alternative as intuitionistic mathematics.

[Van Bendegem, 2005] suggests that both intuitionistic mathematics and inconsistent mathematics fail to qualify as *really* alternative, insofar as they fail to radically change what it means to do mathematics. *"Although some can indeed claim to be alternatives compared with standard classical mathematics, they share too many properties: they all focus on mathematical theories and mathematical proofs, there is an underlying logic implying a standard picture of the nature of a proof, all concepts are sharply delineated"* (p.351). Current inconsistent practices are "merely" replacing one formal system with another, without really questioning the role of formal systems in mathematics or even the notion of formal system itself. It might sound radical to accept contradictory theorems or drop contraction, but formally this reduces to - and, in fact, is presented as - a change of which axioms to adopt and which rules to follow, and the freedom of trying out different rules or axioms has been acknowledged as a part of mathematics at least since the time of Hilbert. Now, it could be that, when transferred back to the *informal* level, some of the formal changes proposed by inconsistent mathematicians would end up deviating from the standard picture to such a degree that our very idea of mathematical practice would be affected; but this is yet to be seen.[53]

One bold answer to all this could be to bite the bullet and argue that inconsistent mathematics is really alternative precisely because it can never truly abandon the formal. Despite the current state of the literature, however, I am not aware of anyone having explicitly argued that inconsistent mathematics *cannot* be informal, nor can I imagine much of a reason why that would be the case. Sometimes the point is made that discovering inconsistent mathematics requires a formal approach because there is no shared intuitive way of reasoning with true inconsistencies. We have already seen several counterexamples;[54] but even if the formalism had been essential to the original context of discovery in all cases, there is no reason to think that informal intuition would *never* develop. Such a failure would, if anything, have to be blamed on the formalism itself.

[52]Much like in the case of intuitionistic mathematics, this does not mean that results analogous to the classical ones could not be recovered; but formulations and proofs would have to be quite different, and the original results are not considered acceptable *until* appropriate reformulations have been found, which of course may well be never.

[53]To be fair, the current state of affairs may be an instance of the catch-22 described above: there is certainly a risk that sufficiently alternative mathematics may not be taken seriously qua mathematics *unless* it was presented as a formal system in the usual sense, which inherently makes it less alternative. On the other hand, roughly 99% of the inconsistent mathematics literature is the product of logicians, and logicians love their formal systems.

[54]Like the antinomic set theory of [Asenjo, 1996], or the entirety of Chapter 5.

Another argument against the alternativeness of inconsistent mathematics was given by [Azzouni, 2007]. The thesis here is that the core of contemporary mathematics is its *mechanical recognizability*, while the choice of any particular logic is completely secondary: *"Contemporary mathematics [...] substitutes for classical logic (the tacit canon of logical principles operative in [earlier] mathematics), proof procedures of any sort (of logic) whatsoever provided only that they admit of the (in principle) mechanical recognition of completely explicit proofs"* (p.19). Most of the inconsistent mathematics proposed until now is formal in this sense, so by Azzouni's lights there is nothing really alternative about it.[55]

Now, theories built on adaptive logics appear to be an exception to Azzouni's criterion, insofar as one cannot mechanically check whether a given statement has been derived indefeasibly or not by simply looking at its proof;[56] this suggests that an inconsistent-adaptive mathematics could in fact constitute a really alternative mathematics. That being said, most current adaptive theories come with classical recapture both motivationally and as a technical result, and with no substantial incommensurability to speak of the claim of alternativeness seems somewhat undermined.

So what might a real alternative look like? [Van Bendegem, 2005] sketches three proposals. The first is *vague mathematics*, where mathematical predicates are allowed to be vague: this way we can prove for example that there is a number that is neither *small* nor *large*, and that small numbers have *few* prime factors. In such a mathematics vague statements would be perfectly acceptable as theorems, and it would be acceptable to reason directly with vague terms.[57] The second example is *random mathematics*, where the idea of proof is discarded altogether and pieces of empirical knowledge are glued together in whatever way provides consistency. It can be shown that this would - given enough time - suffice to retrieve as many classical theorems as we want. The procedure is deeply non-monotonic, since statements may change their truth-value with every new discovery. Finally, we have *open* or *non-compact mathematics*, which - as the name suggests - contradicts the (classical) compactness theorem stating that if every finite subset of a theory has a model then the theory has a model. The trick is that only finitely many formulas are evaluated at any given time, and so theories can admit local, essentially finite models which cannot be extended to models of every sentence in the theory.[58]

[55] Even in Istre's NNST, where the set of proofs is conjectured to be undecidable, on can still mechanically check whether a given proof is normal, so valid proofs are (in principle) recognizable.

[56] See Section 2.5.

[57] This can be made precise with a supervaluation semantics, although presumably the point is that we could just as well do without it.

[58] See [Van Bendegem, 2000], [Van Bendegem, 2016], and [Van Bendegem, 2002] for more about vague, random, and non-compact mathematics respectively.

Based on these examples, I can think of at least one way to see inconsistent mathematics - understood as queer incomaths - as really alternative. Each of Van Bendegem's alternatives reject one core tenet of mathematics as we know it today: precision, proof, and compactness. Another classical tenet is *stability*: established results are never called into question, and in fact *"a curious game is played to try to identify the historical development with an internal logical development. A number of examples in the present-day mathematics of theorems express the idea that in a particular domain all has been said that could be said and that hence this domain is completed"* [Van Bendegem, 2016, p.229]. Following Van Bendegem's naming convention, queer incomaths is a kind of *unstable mathematics* to the extent that it rejects stability: established results should be constantly put into question, and rejecting the absolutist cumulative picture should be an *active* part of practice.[59] Furthermore, since mathfucking is nihilistic and requires (even in principle) no formal validation, Azzouni's hard core of mechanical recognizability is also rejected.[60]

Does queer incomaths satisfy Bloor's original requirements? It is certainly not a given that the reasoning be intelligible to the classicist: in fact, being to some degree unintelligible is the *opening move* for any piece of queer incomaths, although it is certainly possible to suggest and explore translations after the fact. Furthermore, since different agents engaged in queer incomaths may struggle just as much to understand one another, queer incomaths is reminiscent of one of Bloor's own proposals: *"it could be that lack of consensus was precisely the respect in which the alternative was different to ours. For us agreement is of the essence of mathematics. An alternative might be one in which dispute was endemic"* [Bloor, 1991, p.108].[61]

6.4 Inconsistent revolutions

Orthogonally to whether inconsistent mathematics is alternative or not, there is the question of whether or not it should *replace* classical mathematics; in other words,

[59]The specification is important: there are many purely theoretical arguments against the orthodoxy which do not suggest the need for an active disturbance, and therefore do not generate an alternative by themselves. Furthermore, *practice* is already by itself not particularly cumulative, since entire branches of mathematics are routinely forgotten and definitions change all the time. Queer incomaths is not alternative in experiencing shifts; it is alternative in chasing them.

[60]Mechanical recognizability is not per se incompatible with queer incomaths, and it could be enforced. However, this would be merely for ease of communication, and at the risk of undermining the intended liberatory effects. I think the decision should be left to the particular agents.

[61]Of course, Bloor was interested in *actual* alternatives, and - to my knowledge - there is no community practising queer incomaths yet. But a girl can dream...

whether it comes with the intent or requirement of a *revolution*.[62]

The existence of revolutions in mathematics is controversial, first and foremost because of some confusion on what the term "revolution" really means.[63] The discussion starts by analogy with the kind of revolutions that [Kuhn, 1970] famously pointed at in the history of science, like the rejection of phlogiston theory in chemistry or the move from Newtonian to Einstenian mechanics in physics. However, not every major development is a revolution. [Crowe, 1967] makes a useful distinction: *"In a transformational event, an accepted theory is overthrown by another theory, which may be old or new. In such an event, there is a struggle in which both sides more or less understand each other, but still sharply disagree. At the conclusion of the event, an area of science has been transformed. In a formational event, an area of science is not transformed, but is formed. The discovery or theory that produces this effect is usually new, and by definition overthrows and replaces nothing"* (pp.123-124). While sometimes formational events - e.g. the discovery of X-rays - are talked about as revolutionary, most of the literature expects revolutions to be transformational to some degree.

Given an understanding of revolutions as transformational, the mainstream opinion appears to be that expressed by Crowe's Tenth Law: *"Revolutions never occur in mathematics"* [Crowe, 1975]. This is because *"a necessary characteristic of a revolution is that some previously existing entity (be it king, constitution, or theory) must be overthrown and irrevocably discarded"* (p.165); yet mathematics is classically understood as *cumulative*, to the extent that no mathematical results are ever rejected.

Now, even if we accept that no *results* are ever rejected, mathematics can reject other things, and those rejections can be significant enough to warrant the name of revolution. This is something Crowe himself points out: *"revolutions may occur in mathematical nomenclature, symbolism, metamathematics (e.g. the metaphysics of mathematics), methodology (e.g. standards of rigour), and perhaps even in the historiography of mathematics"* (p.166). [Dauben, 1984] proposes two paradigmatic examples of such revolutions: the discovery of incommensurable magnitudes and the advent of transfinite set theory, both of which deeply affected the language, methodology, and metaphysics of mathematics.[64]

Inconsistent mathematics often suggests significant conceptual changes. This is most obvious for naive set theories, which literally argue in favor of a

[62] It goes without saying that only time can tell whether or not it will be a *successful* revolution.

[63] For an overview of the topic, see [Gillies, 1992].

[64] [Dauben, 1992] adds to the list Cauchy's introduction of new standards of rigour and Robinson's nonstandard analysis. The latter example is a bit bizarre to me, since nonstandard analysis has not replaced standard analysis at all; but it does have the features of something that someday *could*, so it is at least a potential revolution.

different conception of set. More generally, one could say that the acceptance of inconsistent theorems affects our very conceptions of mathematical truth and existence, by rejecting consistency as a necessary condition for the former and sufficient condition for the latter. However, in principle such changes need not affect contemporary practices at all; for example, classical mathematicians may keep working in a copy of the classical universe within the naive universe, and never have any reason to look outside. More generally, as long as inconsistent mathematics presents itself as a conservative extension of the classical universe, we seem to be dealing with a formational event: new areas of mathematics are created, but old areas are not really influenced and may keep doing their thing.[65]

Can inconsistent mathematics be revolutionary in transformational ways as well? One thing we might look for is revolutions which affect *methods*. This is arguably the case for Dauben's examples above. [Cohen, 1985] adds Cartesian geometry to the list: while none of Euclid's theorems were rejected, his synthetic *methods* were, not because they were deemed invalid but because they were deemed obsolete in light of the far more effective algebraic approach. Is inconsistent mathematics suggesting a replacement of classical methods in virtue of their being obsolete? The only actual example I can think of is the use of inconsistent models to easily prove nontriviality, certainly far more straightforward than any transfinite proof theory.[66] However, this line of argument relies on the assumption that what the classical mathematician is really interested in when searching for consistency proofs is nontriviality. While it is of course the case that classically consistency and nontriviality can be identified, the fact remains that the classical mathematician does not merely think that their theories should be nontrivial; they think that they should be *consistent*. So inconsistent mathematics is not making the usual methods obsolete; it is simply answering a different question.[67]

Another typical feature of revolutions one might try to focus on is *incommensurability*. [Bueno, 2007] argues that the history of mathematics is filled with episodes of genuine incommensurability, much like Kuhn argued the history of science is. For example, Cauchy's theorem that the sum of a convergent series of continuous functions is continuous holds in nonstandard analysis, but not in standard analysis, so incompatible conceptions of the real line are at play;

[65] In the specific context of revolutions in logic, [Aberdein and Read, 2009] call this kind of revolution *paraglorious*. This is in contrast to *glorious* revolutions, where the character and significance of a theory changes completely; and *inglorious* revolutions, where some stuff is lost for good.

[66] Compare, say, [Meyer, 1976] with [Gentzen, 1936].

[67] Of course, if the nontriviality proof lead to a consistency proof that *would* be an improvement. But to my knowledge there are no results of this sort yet.

similarly, the well-ordering principle as conjectured by Cantor fails, but it was proven by Zermelo via a change in both the conception of set and the meaning of the principle itself.[68] This is not to say that all these different practices could not be encompassed within a common framework: for example, both standard and nonstandard analysis are expressible (and relatable to each other) in ZFC. But when we take them both seriously, incommensurability ensues.

Maybe even more controversially, [Pourciau, 2000] argues against Crowe that Kuhnian revolutions in the strongest sense are possible in mathematics, and intuitionistic mathematics is a (failed) example. As already discussed in the previous section, some classical theorems are either untrue or unintelligible within the intuitionistic paradigm; many questions are rejected (e.g. the Continuum Hypothesis or the problem of consistency), while many new ones take their place (e.g. whether a given number is constructible). Formal intertranslatability is not really an issue, since no translation can be both meaning-preserving and truth-preserving. Of course, intuitionistic mathematics has failed to overcome classical mathematics; but this is a historical contingency rather than an indication that revolutions can never occur.

It seems fair to say that Weber's dialetheic mathematics is roughly as incommensurable with classical mathematics as intuitionistic mathematics is, for the same reasons it is just as alternative.[69] All that being said, another important feature of revolutions is their providing an answer to a recognized *anomaly*. We would expect mathematics to be in at least *some* trouble in order to justify a paradigm change: as [Kuhn, 1970] puts it, a *"sense of malfunction that can lead to crisis is prerequisite to revolution"* (p.92).[70] Intuitionism arose at a time of perceived crisis, when many mathematicians found the set-theoretic paradoxes to be a genuine anomaly in need of a solution. But the paradoxes have long stopped being scary, and Pourciau suggests this is precisely why intuitionism still has not triumphed despite [Bishop and Bridges, 2012] having addressed most of the problems with Brouwer's original presentation: the crisis intuitionism was born

[68] Such examples could themselves be taken as generating inconsistency. Usually the old paradigm gets eventually discarded, so it is a merely temporary phenomenon; but one could also keep both conceptions active.

[69] Note that this has little to do with the choice of metalanguage: regardless of which tools we deem appropriate to think or talk *about* a theory, inconsistent mathematics may remain revolutionary in virtue of how different it is to work *within* it. A revolution needs not affect our standard ways to think about theories; one way to put this is that a revolution may occur in mathematics without thereby occurring in model theory.

[70] The literature does contain examples of alleged revolutions which do not appear to have been predated by anomalies, e.g. the discovery of X-rays and Robinson's nonstandard analysis. But the first is a mere formational event; while the second is hardly a successful revolution (yet), and I would hazard a guess that the lack of pertinent anomalies is a contribution here too.

to answer is simply no longer there, so there is no pressure to upset the status quo.[71] Inconsistent mathematics appears to be in the same situation. Are there open problems that classical mathematicians do not currently know how to solve? Certainly so. But there is no shared pessimistic attitude that classical mathematics cannot eventually find ways to solve them, nor has any evidence been provided that inconsistent mathematics can solve them. In this sense, rejection of classical mathematics in favor of inconsistent mathematics is simply unjustified on Kuhnian grounds.

It could be objected that classical mathematicians are merely failing to recognize the problem, like [Bishop, 1975] does on behalf of constructivism. For example, the dialetheic mathematician could argue that mathematics *should* be concerned about the fact that classical topology introduces an unexplainable asymmetry in the way we understand boundaries. This line of argument might be strengthened via an appeal to a past where such problems were in fact considered important: the charge then becomes that mathematicians were too quick to *abandon* the problem once it became clear that a solution was not classically available. Relevant arithmetic, for example, can be framed along these lines as an answer to the old problem of proving the nontriviality of arithmetic with finitary methods. It should also be mentioned that right now most mathematicians are not even aware that these solutions exist; whether awareness would suffice to change their minds is an empirical fact that might take some time to assess.

Still, regardless of whether or not we take such issues to carry the weight that revolutionaries assign to them, it is simply an observation that the (mainstream) mathematical community does *not* appear to be experiencing any "sense of malfunction" due to them.[72] To dismiss the alleged problems as non-mathematical would, admittedly, be begging the question; but it is also not at all clear that mathematics would suffer by ignoring them, because there is no indication that solving them would provide any benefit to practices that do not already believe in

[71] Pourciau is not alone in taking Brouwer's work in intuitionistic mathematics to be a confused misstep on the way to Bishop's constructive mathematics: see e.g. [Bauer, 2017]. It should however be noted that intuitionistic mathematics building on Brouwer's work, which is even more directly contraclassical, is still being developed: see [Posy, 2020] and references therein.

[72] For that matter, it is controversial whether this was *ever* the case, or whether it is merely an anachronistic and overblown projection of foundational worries coming from a few influential 20th century mathematicians. For example, [Fowler, 1999] extensively argues against the folk belief (so to speak) that the discovery of incommensurables was much of a crisis at all for Early Greek mathematics. Even when it comes to the discovery of the set-theoretic paradoxes, it would be a gross misrepresentation to say that most mathematicians thought mathematics to be genuinely *endangered* by them, insofar as their existence had no influence whatsoever on their work one way or the other; finding a paradox-free foundation was just another interesting (to some) open problem. If every problem-generating surprise constituted a crisis in mathematics, we would have a revolution every five minutes.

their intrinsic worth. This does not mean that inconsistent solutions to nonclassical questions are not worth pursuing; the point is simply that, as long as inconsistent mathematics has nothing to offer to classical practices *qua classical* - no new answers to currently accepted questions - there can be no Kuhnian inner push for a revolution.

Now, of course inconsistent agents do not need to be recognized from mainstream mathematics in order for their practice to be legitimate. They are always free to do their own thing regardless of their influence on mathematics as a whole, much like intuitionistic mathematicians are mostly doing nowadays. But at that point talk of revolution would be quite an overstatement: after all, as already discussed, nothing much about the status quo needs to change for inconsistent mathematics to be accepted as an *independent* branch.[73]

Whither the inconsistent revolution? Let us try a different perspective. [Dunmore, 1992] argues that mathematics is characterized by having revolutions only at the meta-level, while the object-level gets reinterpreted in order to be preserved: *"mathematics, unlike the natural sciences, appears to grow very largely by accumulation of results, with no radical overthrowing of theories by alternatives. But what do change in revolutionary ways are the implicit metamathematical views of the community that generate and guide their research programmes"* (p.224). We can read this as saying that revolutions occur in agents rather than in frameworks, because frameworks can in principle always survive the change in agents by being reinterpreted appropriately.[74] The question then is: are inconsistent agents revolutionary?

It will not suffice to say that inconsistent agents belong to inconsistent practices. We know that inconsistent practices are always strictly nonclassical in either questions, proof methods, or theorems; however, inconsistentization does not force a *rejection* of the source practice. In fact, historically it is the case that apparent inconsistentizations have always been eventually assimilated back into the mainstream as consistent: as [Muntersbjorn, 2007] points out, it was not uncommon to introduce contradictory-looking formalism (say, $\sqrt{-1}$) only for future generations to fill it with (consistent) content. Inconsistent mathematics could be seen as a natural extension of this process, brought on by the new formal sensibilities of current times making it harder to hide any inconsistency; if so, inconsistentization is just one step towards the real insight awaiting in a future consistent reassimilation.[75] From this perspective, while inconsistent mathematics

[73] Although funding might be an issue.

[74] This might be a way of reading some of Bloor's historical examples of alternative mathematics as genuine revolutions.

[75] To be clear, this *is* a proper extension: after all, by definition inconsistent agents take inconsistencies seriously, rather than as formal nonsense to be explained at a later time.

could certainly *lead* to particular conceptual revolutions, it would not be itself a revolution in the sense of constituting too substantial a break from previous ways in which such conceptual revolutions were achieved.

Let us consider the three dimensions of inconsistent agents more closely. Is any coordinate intrinsically revolutionary? When it comes to which inconsistencies are accepted, it does not seem so. Inconsistent models, theorems, and applications could all happily exist on top of classical mathematics. Inconsistent foundationalists may sound like an exception; but either the proposed foundations are aimed at classical practices, in which case they must countenance classical recapture and nothing is rejected, or they are founding something new, which can be seen as a purely formational event. It might also be seen as more, of course; but that is not intrinsic to being a foundationalist agent.[76]

Monist agents are largely old news: the only change is that the logic of choice is paraconsistent rather than classical. I am tempted to follow Azzouni in not finding such an attitude particularly novel: the study of a particular formal system already constitutes a monist practice, and this can certainly be done with no revolutionary intent (or impact, for that matter) whatsoever. The situation is quite different for pluralist and nihilist agents. On one hand, we can simply think of a mathematical logician being interested in all sorts of formal systems, and so being in principle open to all kinds of logics. On the other hand, when we look at more local practices, a pluralist/nihilist attitude means having a question whose answer may be given by means of many different logics. This sounds like a revolution in the way mathematical problems are approached: it is certainly not the case that mathematicians allow themselves a range of logics to choose from in trying to solve a given problem (nor, in fact, do they generally think about the logic they are using at all). Of course this has nothing to do with inconsistent mathematics specifically, and so with the logics in questions being paraconsistent; any sort of pluralism would lead to the same conclusion. But inconsistent mathematics provides plenty of instances: for example, [da Costa, 1964], [Sylvan et al., 1982], and [Carnielli and Coniglio, 2016] all present a *hierarchy* of logics which may be used to address a given mathematical question, with no particular logic being singled out as the correct one. [Mortensen, 1995] and [Mortensen, 2010] arguably exemplify the nihilist attitude, since all kinds of logics are up for grabs.

Finally, consider attitudes on when to inconsistentize. Desperate and opportunistic agents can in principle spend their lives undercover as classical agents; they may bring over inconsistent Kuhnian revolutions if and when the

[76]I expanded on this in Section 1.4.

time comes, but there is nothing particularly revolutionary about their attitude.[77] Meanwhile, curious agents are essentially just keeping an open mind to formational events. What about righteous agents? Following [Kitcher, 1984, ch.9], we can see changes in historical practices as essentially driven by inner problem-solving pressures: some of the usual mechanisms are question-answering, question-generation, generalization, rigorization, and systematization. All of these processes either directly solve open problems, or provide tools that are expected to solve more open problems. A duty to inconsistentize thus signals a clear break from the mainstream, which - depending on the practice - involves no such tendency or worse an opposite tendency to consistentize (as part of rigorization).[78] Righteous agents are revolutionary to the extent that they push for a new kind of interpractice transition, and in particular one that is not guided by internal problem-solving needs.[79]

The point can be pushed even further if we focus on queer incomaths. The mainstream position is that mathematics is, so to speak, ethics-free:[80] any ethical issues, if present at all, concern bad applications and have therefore nothing to do with the practice of mathematics itself, which is and should be driven entirely by epistemic considerations - the only goal is the growth of mathematical knowledge.[81] This is not to say that non-epistemic factors, e.g. personal interests and funding opportunities, never interfere; the point is that they are treated as mere interference. On the other hand, the argument from liberation suggests a need for supplementary ethical considerations: mathfucking is not (primarily) meant as a contribution to the growth of mathematical knowledge, but rather as a disruption of

[77] As an aside, if we follow Kuhn in denying that a paradigm shift can ever be decided on purely rational grounds, there may well be no way to ever convince a desperate agent to take steps toward an inconsistent revolution: in this sense desperate agents are the same as classical agents, only a bit more delusional about it.

[78] The connection between rigorization and consistentization has always been part of mainstream practice, but it is hardly necessary. In fact, all the processes indicated by Kitcher still make sense for inconsistent practices.

[79] This does not, by itself, imply the rejection of stability which makes queer incomaths really alternative. As I already mentioned, foundationalists can be righteous in believing that we should inconsistentize everything so as to fit it within an inconsistent foundation, but this hardly prevents them from taking said foundation to be perfectly stable.

[80] This is critically discussed e.g. by [Ernest, 2018].

[81] Sometimes aesthetic considerations come into play as well, although they are usually taken to be related to the epistemic ones.

the socially harmful naturalization of classical mathematics.[82] Roughly, we could then say that practitioners of queer incomaths are revolutionary insofar as their mathematical work is primarily guided by ethical, rather than epistemic, values.[83] The revolution - which, of course, is as far from a Kuhnian revolution as it could possibly be - then leads us towards a new age of *ethical mathematics*.[84]

To conclude this section, let me quickly hark back to the last section to suggest that an ethical mathematics needs not be alternative in any strong sense; that is rather a consequence of *how* practice is reformed in order to reflect the new values at play. It would not be much of an alternative if we decided to, say, exclude quaternions from mathematics on the grounds that commutativity is divine law and divine law determines good. In fact, the revolutionary side and the alternative side of queer incomaths have different sources: queer incomaths is (really) alternative because it rejects stability, while it is revolutionary because it pushes for a nihilistic methodology and new, ethically motivated standards of practice change.

6.5 Conclusion

We have seen that practices associated with inconsistent mathematics are nonclassical insofar as they may accept inconsistent statements, proofs relying on inconsistent statements, or inconsistent concepts as an object of research for their own sake. While the collection of all such practices is exceedingly inhomogeneous, the many ways in which the metamathematical views of inconsistent agents can combine - e.g. when it comes to what kinds of inconsistencies to accept, which logics to adopt, and how often to inconsistentize - suggest many possible ways to narrow down a conception of the field.

[82]In making this point to people, I have sensed some resistance to my linking the ethical and the political this way. In fact, more than once I have been asked - not by ethicists, mind you - what the argument from liberation has to do with ethics. Now, I am not an ethicist, and do not claim to have much of a theory of what an individual should do when faced with these issues; most of the time I can barely tell what I'm supposed to do myself. I am merely adopting the following basic "feminist" stance: systemic oppression exists, it sucks, and it is morally good to work against it.

[83]To be more precise, I think both classical mathematics and queer incomaths can be understood as having ethical *and* epistemic aspects, but in the case of classical mathematics any ethical aspects are *derived* from the epistemic ones: the pursuit of (classical) mathematical knowledge, like the pursuit of any scientific knowledge, may be considered a moral duty either in itself or for the sake of improving the human condition. Conversely, mathfucking can have an epistemic upshot insofar as it brings into focus new perspectives and possibilities, and there is room to rephrase its goals in terms of counteracting epistemic injustice; I will say a bit more on this in Chapter 7. But I take mathfucking to be valuable *regardless* of whether it has such an upshot.

[84]Since the ethical values in question are typically feminist values, one could more specifically call this *feminist mathematics*. I should emphasize that I do not think this is the *only* possible way to conceive of ethical or feminist mathematics; it is but one proposal.

Weber's dialetheic mathematics was shown to be alternative more or less in the way intuitionistic mathematics is, on grounds of conceptual incommensurability and distinct bodies of truths. On the other hand, queer incomaths is really alternative (in Van Bendegem's sense) insofar as it actively rejects a core tenet of contemporary mathematical practice, namely the stability of established results. Most examples of inconsistent mathematics fail to be a transformative revolution in the Kuhnian sense of addressing a sense of malfunction within classical mathematics; nonetheless, pluralists and nihilist agents are revolutionary in their problem-solving methodology, while righteous agents are revolutionary in their standards of practice change. Queer incomaths adds to the list by introducing a novel ethical dimension to the mix.

Chapter 7

Coda: the future of inconsistent mathematics

Inconsistent mathematics can be many things. It can be motivated in many ways; it can rely on all sorts of logics; it can follow a variety of approaches; and different agents can coherently bring in all kinds of views on what direction the field should take.

In the course of this dissertation, I have argued in favor of a particular direction. Inconsistent mathematics can do some good in the world, because it is specially placed to *counteract the naturalization* of classical mathematics. It does not need to search for any one true logic; rather, it can be open to any possible interpretations, and so to *any logic whatsoever* which may serve them. It needs not be reduced to a collection of theories or even of practices - which is just as well, as that would be far too easy for the classicist to reappropriate; rather, it can be a liberatory *agent-dependent activity*. This would be a *real alternative* to classical mathematics, insofar as the stability assumption concerning classical results is rejected; and it would be a *revolution*, insofar as it embodies methods and values which are not currently found anywhere near mathematics. This is queer incomaths.

To conclude, let me say a couple words on how a future in which queer incomaths is taken seriously could look like, and how we could get there.

7.1 Implementing queer incomaths

First of all, I should say queer incomaths does not point to a future in which me and my imaginary followers *"oppress classical mathematicians"*, as an audience member with seemingly no understanding of oppression put it to me once. Classical mathematicians are free to keep doing what they are doing. Not only because they are both the orthodoxy and the ruling majority, so if they consider themselves oppressed by the hot takes of a PhD student they really need to check their privilege; but also because their work is not intrinsically against queer incomaths' aims.[1] Queer incomaths is fundamentally *maths-positive*:[2] it achieves its goal by actively pushing for diversity and anti-absolutism in mathematics, not by making any particular mathematics the enemy. There will always be a need for mathematicians developing the interpretations they produce; and I think that classical interpretations are well worth developing. The problem is when those interpretations become the only norm, when mathematical language becomes that which cannot be challenged, when the classical theorem is forever.

A community practising queer incomaths serves as a protection against absolutism, a reminder that things - even mathematical things, which most of us have been raised to accept as necessary and unquestionable - could be different if we just learned to look at them differently. The importance of this should make its way into the mathematics departments, into the public perception of mathematics, into the teaching of mathematics; and it should be backed up by subversive activity in the form of mathfucking. But this is not meant to erase classical activities; it is meant to denaturalize them, to undermine the dualistic thinking that goes with them, and to counteract the ways in which they harmfully interact with concrete dualisms plaguing our societies. The proposal is that mathematics as a field needs *both* mathfuckers and "standard" problem-solvers. Each individual mathematician may pick their own path depending on their skills and attitudes.[3]

Now, this raises the question of how to push enough mathematicians towards mathfucking. A full investigation of this issue is well beyond the scope of this thesis. Still, very naively speaking, one way to do this might be to introduce courses dedicated to the topic. Such courses would present both the arguments in favor of queer incomaths, and some technical examples of mathfucking. It is of course important that the examples presented in such courses would be presented as just that: examples. Any closed formal theory of mathfucking would obviously be

[1] Let me also clarify, in case there is any need to, that I do not think inconsistent mathematicians are being *oppressed* (qua inconsistent mathematicians) by classical mathematicians not caring about their work. If anything, I find it a bit tasteless to talk about *"longstanding anti-foundationalist and anti-logician attitudes in the literature"* [Weber, 2021a, p.104], or to see the widespread adoption of Zermelo's axioms as a *"warning to any of us when we institute what we think will be 'temporary measures'"* (p.175).

[2] Thanks to Gillian Russell for suggesting this way of putting it.

[3] This division of labor is how [Rowbottom, 2011] resolves the contrast between Kuhn's call for dogmatism in normal times and Popper's belief that constant criticism is at the heart of a healthy science. That being said, mathfucking and Popperian criticism are very different in their aims and methods, the latter being aimed specifically at falsification and - as far as I know - not particularly focused on inconsistent reinterpretations.

self-defeating.

What about other conceptions of inconsistent mathematics? Well, they are also welcome. Regardless of whether a piece of inconsistent mathematics is conceived as queer incomaths or not, it remains to some degree subversive: at the very least it contradicts the mainstream assumption that contradictions should not be purposefully kept around.[4] Obviously any sort of hardcore foundationalism will be incompatible in terms of motivation, but queer incomaths can still find plenty of value in the resulting work, not only on a technical level, but also insofar as it contributes a different way to see the world. There is only disagreement when it is presented as the one true way - or as a candidate for the one true way, which makes little difference here - because queer incomaths is ever skeptical of treating any mathematical truth as definitive.

7.2 Queer incomaths and philosophies of mathematics

While the adoption of queer incomaths is philosophically motivated, this hardly determines a full-fledged philosophy of mathematics. So what would the rise of queer incomaths mean for the philosophy of mathematics at large?

First, a caveat. Since queer incomaths is first and foremost a *proposal* concerning the *practice* of mathematics, it can only be incompatible with a given philosophy of mathematics insofar as that philosophy takes itself to have a normative effect on the practice. To go back to the usual example, it does not bother queer incomaths one bit if Weber is right in taking the one true metaphysics to be given by (something like) his naive set theory to the exclusion of everything else; the incompatibility only arises when this is taken to dictate what mathematicians should or should not do, in which case we would have a restriction on the space of practices which is contrary to the aims of queer incomaths.[5]

Keeping this in mind, I do not think queer incomaths enforces any particular kind of metaphysics, and in fact most of the usual options remain available in one form or the other. In fact, given that a choice of metaphysics may be part and parcel of a given practice, queer incomaths might be seen as encouraging an ever-changing plurality of metaphysical interpretations. Problems only arise when some metaphysics is taken to non-trivially constrain - at the meta-level, so to speak - the space of legitimate practices (e.g. by imposing a certain logic or reduction). This entails no commitment to, say, inconsistent entities; but it does entail that inconsistent practices cannot be excluded on the grounds that inconsistent objects

[4]As I noted in Section 4.6, the same could be said about much nonclassical mathematics, insofar as being nonclassical contradicts the mainstream assumption that mathematics should be classical!

[5]The truth shall not, in fact, set us free.

do not exist according to metaphysics X. In other words, it is in principle coherent with queer incomaths - if, I must say, a bit suspect - to take inconsistent practices to be fictional stories as opposed to the veridical stories of classical mathematics.

As an example, consider the really full-blooded Platonism from [Beall, 1999], which roughly takes any non-trivial description whatsoever to capture a genuine mathematical reality.[6] This is perfectly compatible with queer incomaths, and it remains so even if we understand it in a somewhat reductionist sense, so that descriptions have to be formulated in a specific language; after all, such views are rarely taken to fail just because in practice noone actually formulates mathematics in such a way, as long as a translation can be reasonably assumed to exist. Similarly, sufficiently open-minded fictionalist views will do just fine; any differences are quite irrelevant from the perspective of queer incomaths.[7] Queer incomaths is not committed to the truth or existence of anything it produces; it is merely committed to the value of its characteristic producing act, a value which does not depend on the ontological status of mathematical entities.

Since the argument from liberation appealed in a few places to social constructivist literature, it is maybe worth asking: would a social constructivist metaphysics, taking mathematical entities to be *social* entities whose stability is granted by institutionalized belief, be appropriate for queer incomaths?[8] Well, again, yes as long as it does not devolve into a complete deference to institutions on what counts as legitimate mathematics and what doesn't. Of course, the motivation behind queer incomaths is that institutions *do* have the power to enforce this; so the point is that institutionalized mathematical belief should be constantly challenged. The appealing connection here is that the purported destabilization of mathematical results - i.e. of the shared belief in the uniqueness and absoluteness of those results - goes hand in hand with the destabilization of the ontological status of the associated entities.

When it comes to epistemology, queer incomaths is similarly permissive. Rationalist views of the growth of knowledge remain admissible, insofar as they are not taken to silence alternative ways of knowing: for example, one may accept that the choice to stick with classical logic in mathematics has been rational, and still deny that this should preclude the use of different logics. This does

[6] This is a paraconsistent generalization of the full-blooded Platonism from [Balaguer, 2001], where *consistency* rather than non-triviality is the existence condition.

[7] This is not a commitment to the thesis, which can be found in e.g. [Balaguer, 2001] and [Maddy, 2011], that there is simply no way to decide between (some versions of) realism and anti-realism. Here I am merely claiming that queer incomaths is indifferent to which one (if any) is correct.

[8] One standard reference for this kind of view is [Ernest, 1998]. For a more recent attempt, see [Cole, 2013].

not contradict the argument from liberation for two reasons. First, classical logic may well be recognized as having theoretical virtues like simplicity and precision, despite its support of dualisms. Second, the search for alternative notions of rationality involves going *beyond* the classical male-coded one, but not necessarily *rejecting* it altogether. Even foundationalist projects need not be rejected altogether; what is rejected is the uniqueness and eternal stability of any such foundation, together with the idea that anything not falling under a foundation must be a priori discarded. In short, the restriction on traditional epistemologies is simply that they not be abused for the sake of gatekeeping, e.g. by excluding from the realm of knowledge any piece of mathematics that cannot be reduced to classical formal reasoning.

That being said, there are two kinds of epistemologies which queer incomaths may seem to go particularly well with. The first of these is *standpoint epistemology*. As [Saint-Croix, 2020] puts it, such an epistemology recognizes that *"social categories can generate standpoints that provide their occupants with particular, legitimate epistemic goods"*. This makes it necessary to include and - depending on the context - defer to different standpoints in order to obtain a more complete picture of reality.[9] In order to apply standpoint epistemology to mathematics, one would have to argue that mathematics can be one of those contexts where the standpoint of the knower matters. This is not hard if we think of applications: in so far as, say, standpoints occupied by women qua women lead to an epistemic advantage in matters of sexism, it stands to reason that this advantage would carry over to applications of mathematics in a heavily gendered context. Queer incomaths would then contribute by not restricting occupants of subordinate standpoints to classical mathematics, the rules of which are enforced by dominant standpoints. Different subordinate standpoints may lead to different inconsistentizations, which would come together to expand and correct currently accepted knowledge. Unfortunately I am not aware of any work in this direction, beyond the lovely collection of examples - from pure mathematics! - in [Katz, 2020].

A second natural option might be a *queer epistemology*, aiming to expose the fluidity and instability of knowledge, particularly in regard to identity claims.[10] The connection with queer incomaths is even more immediate here: mathfucking is after all the direct analogue of gender fucking, and it can be similarly understood as a tool to undermine any appearance of fixed knowledge. The discussion by [Tanswell, 2018] of the many reasons why mathematics fails to be a conceptual

[9] The term "standpoint epistemology" has a complicated history. Here I am using it in the sense of [Saint-Croix, 2020], which also happens to be a good source on said history; see also e.g. [Intemann, 2010].

[10] See e.g. [Hall, 2017].

safe space might provide a good start for the application of queer epistemology to mathematics.[11] Again, these suggestions can only remain tentative until such epistemologies have been fully developed qua epistemologies *of mathematics* - a task certainly too ambitious for this thesis.[12]

7.3 Conclusion

Queer incomaths is certainly dependent on what mathematics is and how it interacts with the world *today*. It may be thought that this puts a timer on it. If dualisms fell out of cognitive fashion, would we still need queer incomaths? If, at some miracle point, mathfucking became common enough that it completely undermined the privileged status of classical mathematics, would we still need queer incomaths? I think the answer is yes in both cases, if only in virtue of the fact that as soon as mathfucking stopped the way would be clear for a new fixed paradigm to be sold as the only natural one, and the road from this to more oppressions - even if not necessarily under the guise of dualisms - is all too quick. I do not think there is any safe utopia to tend to, and so I see no obvious reason why queer incomaths should stop being valuable any time soon.[13]

We may conclude on an optimistic note. It is true that, looking at the last fifty years of research, *"happy endings are not the norm in inconsistent mathematics"*.[14] But queer incomaths does not believe in endings, happy or otherwise: rather, it constantly pushes against the limits of categorization, firm in the conviction that we should never impose a limit to how much better the world can and should be.

[11] This was discussed in Section 4.4.

[12] Both standpoint epistemology and queer epistemology turn classical mathematics into a form of *hermeneutical injustice*, insofar as it is the systemic hiding, devaluation, or wilful ignorance of the epistemic possibilities revealed by queer incomaths that contributes to oppression (on this kind of injustice see e.g. [Fricker, 2007, ch.7], [Pohlhaus, 2012], [Dotson, 2012a]). I hope to expand on this perspective in future work.

[13] Then again, I reckon that deeply different conceptions of the field of mathematics may someday become predominant and make much of this dissertation inapplicable. I can't actually read the future.

[14] [Estrada-Gonzalez and Tapia-Navarro, 2021, p.517].

Appendix (a topology primer)

Given a set X, a *topology* on X is a family $\tau \subseteq \mathcal{P}(X)$ such that:

1. $\emptyset, X \in \tau$;
2. $A, B \in \tau \implies A \cap B \in \tau$;
3. $A_i \in \tau$ for every $i \in I \implies \bigcup_{i \in I} A_i \in \tau$.

A pair (X, τ) is called a *(topological) space*; often τ is left implicit. The elements of τ are called *open* sets (in X), and their complements are called *closed* sets. By De Morgan laws, it follows from the definition of topology that finite unions and arbitrary intersections of closed sets are closed. Furthermore, by definition X and $\emptyset$ are always *clopen*, i.e. both open and closed.

A set extending an open set to which $x \in X$ belongs is called a *neighborhood* of x. Open sets can thus be thought of as unions of neighborhoods of their points. Every point has at least one neighborhood, namely X. So-called *separation properties* measure how easy it is to distinguish points given their neighborhoods:

- X is *T0* if, for every pair of distinct points, at least one of them has a neighborhood not containing the other.
- X is *T1* if, for every pair of distinct points, *each* has a neigborhood not containing the other.
- X is *T2* (or *Hausdorff*) if, for every pair of distinct points, one can find two disjoint neighborhoods each containing one of the points.

Every space X admits a *discrete topology* $\mathcal{P}(X)$ which maximally separates points, and a *trivial topology* $\{X, \emptyset\}$ which does not separate points at all. The former is T2, since points can be separated by their (open) singletons; while the latter is not even T0 (unless X is a singleton), since the only neighborhood contains every point.

A family $B \subseteq \tau$ is a *base* for X if nonempty open sets are exactly the unions of elements of B; when a base is fixed, the sets in it are called *basic*. The topology *generated* by a family B is the smallest topology extending B. If B is closed under finite intersections, then it is a base for the topology it generates. For example, the standard topology on the real line $\mathbb{R}$ is generated by the family of open intervals $\{(a, b) : a, b \in \mathbb{R}\}$, which is a base.[1] A space is *second-countable* if it has a countable base. The real line is second-countable, since $\{(a, b) : a, b \in \mathbb{Q}\}$ is also a base; while discrete spaces are second-countable if and only if they are countable, since every base must contain all singletons.

We can define an appropriate notion of equivalence between topological spaces as follows. A function between topological spaces is *continuous* if preimages of open sets are open. A continuous bijection is a *homeomorphism* if its inverse is also continuous. Finally, two spaces are *homeomorphic* if there is a homeomorphism between them. Homeomorphic spaces have essentially the same topology: in particular, they share all topological properties. For example, any two discrete same-sized topological spaces are homeomorphic (via any bijection).

There are natural ways to associate topologies with various set-theoretic constructions. If $Y \subseteq X$, the *subspace topology* on Y induced by (X, τ) is $\tau_Y := \{A \cap Y : A \in \tau\}$; it is easy to check that τ_Y is in fact a topology on Y.[2] The *product topology* on the set-theoretic product $\Pi_i X_i$ with (X_i, τ_i) topological spaces is generated by the sets $\pi_i^{-1}[A]$ with $A \in \tau_i$, where $\pi_i : \Pi_i X_i \to X_i$ is the projection on the i-th coordinate; in other words, the product topology is the coarsest (i.e. smallest) topology making the projections continuous.[3] Given an equivalence relation $\sim$ on X, the *quotient topology* on $X/\sim$ is as follows: a set of equivalence classes in $X/\sim$ is open if and only if the union of those equivalence classes is open in X.

Now, on to some important properties that spaces can have. We say that X is *connected* if $\emptyset$ and X are the only clopens; while we say that X is 0-*dimensional* if it has a base of clopens. Unless the topology is trivial, if X is 0-dimensional then it is not connected. For example, the real line is connected;[4] while the discrete topology is 0-dimensional, since it makes *every* set clopen.

A point is *isolated* if its singleton is open. We say that X is *perfect* if it contains

[1]The notation (a, b) denotes the open interval $\{x : a < x < b\}$. The notation $[a, b]$ denotes the closed interval $\{x : a \leq x \leq b\}$.

[2]Consider the following identities: $\emptyset = \emptyset \cap Y$; $Y = X \cap Y$; $(A \cap Y) \cap (B \cap Y) = (A \cap B) \cap Y$; $\bigcup_i (A_i \cap Y) = (\bigcup_i A_i) \cap Y$.

[3]A handy base for this topology is the family of sets of the form $\Pi_i A_i$ where $A_i \in \tau_i$ and only finitely many of the A_i are proper. This follows from the fact that $\Pi_i A_i \cap \Pi_i B_i = \Pi_i (A_i \cap B_i)$.

[4]Suppose not, and take $a < b$ belonging respectively to a clopen subset C and its complement C' (which must also be clopen). Then one can show that the least upper bound of the set $C \cap [a, b]$ must be in $C \cap C'$, contradiction.

no isolated points. For example, the real line is perfect, since every open interval contains more than one point; while the discrete topology isolates all points.

An *open covering* of a space is a family of open subsets whose union is the whole space. A space is *compact* if every open covering admits a finite subcovering.[5] For example, all spaces with finite topologies (in particular, finite spaces) are compact; the real line is not compact, since e.g. $\{(-n,n) : n \in \mathbb{N}\}$ contains no finite subcovering. Roughly, the intuition behind compactness is that compact spaces are a generalization of finite spaces, and preserve some of their properties. Tychonoff's theorem ensures that the product of arbitrarily many compact spaces is compact with the product topology.[6]

A *pseudometric* on a space X is a map $d : X \times X \to [0,1]$ such that:

- $d(x,x) = 0$;
- $d(x,y) = d(y,x)$;
- $d(x,z) \leq d(x,y) + d(y,z)$.

A *metric* d is a pseudometric with the extra condition that $d(x,y) = 0$ if and only if $x = y$. We say that X is *metrizable* if it admits a metric d which agrees with the topology: formally, this means that the topology must be generated by the family of open balls

$$\{\{y : d(x,y) < r\} : x \in X,\ r \geq 0\}.$$

For example, the real line is metrizable via the usual distance $d(x,y) = |x-y|$; discrete topologies are also metrizable via the discrete metric, which assigns distance 1 to any two distinct points. Note that metrizable spaces are T2: since distinct points have positive distance, open balls of sufficiently small radius (i.e. less than half the distance between the points) are disjoint neighborhoods.

[5] Compact spaces in this sense are sometimes called quasi-compact, with the term compact being saved for quasi-compact T2 spaces.

[6] See [Engelking, 1977, Thm 3.2.4.]. By compact, Engelking means compact T2; however, his proof works either way.

Bibliography

[Abe et al., 2015] Abe, J. M., Akama, S., and Nakamatsu, K. (2015). *Introduction to annotated logics: foundations for paracomplete and paraconsistent reasoning*, volume 88. Springer.

[Aberdein and Read, 2009] Aberdein, A. and Read, S. (2009). The philosophy of alternative logics. In *The Development of Modern Logic*. Oxford University Press.

[Aczel, 1988] Aczel, P. (1988). *Non-well-founded sets*. Center for the Study of Language and Information.

[Anderson and Belnap, 1975] Anderson, A. R. and Belnap, N. D. (1975). *Entailment, Vol. I: The logic of relevance and necessity*. Princeton University Press.

[Anderson et al., 2017] Anderson, A. R., Belnap Jr, N. D., and Dunn, M. J. (2017). *Entailment, Vol. II: The logic of relevance and necessity*. Princeton University Press.

[Arruda and Batens, 1982] Arruda, A. I. and Batens, D. (1982). Russell's set versus the universal set in paraconsistent set theory. *Logique et Analyse*, 25(98):121–133.

[Asenjo, 1954] Asenjo, F. G. (1954). La idea de un cálculo de antinomias. *Seminario Mathematico, Universidad Nacional de La Plata*.

[Asenjo, 1966] Asenjo, F. G. (1966). A calculus of antinomies. *Notre Dame Journal of Formal Logic*, 7(1):103–105.

[Asenjo, 1989] Asenjo, F. G. (1989). Towards an antinomic mathematics. In *Paraconsistent Logic: Essays on the Inconsistent*, pages 394–414. Philosophia Verlag Gmbh.

[Asenjo, 1996] Asenjo, F. G. (1996). Antinomicity and the axiom of choice. A chapter in antinomic mathematics. *Logic and Logical Philosophy*, 4(4):53–95.

[Asenjo, 1998] Asenjo, F. G. (1998). The general concept of antinomicity. *Foundations of Science*, 3(2):429–465.

[Asenjo, 2002] Asenjo, F. G. (2002). The logic of opposition. In *Paraconsistency: The Logical Way to the Inconsistent*, pages 132–163. CRC Press.

[Asenjo and Tamburino, 1975] Asenjo, F. G. and Tamburino, J. (1975). Logic of antinomies. *Notre Dame Journal of Formal Logic*, 16(1):17–44.

[Avron and Zamansky, 2010] Avron, A. and Zamansky, A. (2010). Non-deterministic semantics for logical systems. In *Handbook of Philosophical Logic: Volume 16*, pages 227–304. Springer.

[Ayim, 1995] Ayim, M. (1995). Passing through the needle's eye: Can a feminist teach logic? *Argumentation*, 9:801–820.

[Azzouni, 2007] Azzouni, J. (2007). How and why mathematics is unique as a social practice. In *Perspectives on mathematical practices*, pages 3–23. Springer.

[Badia, 2017] Badia, G. (2017). *The languages of relevant logic: a model-theoretic perspective*. PhD thesis, University of Otago.

[Badia and Tedder, 2018] Badia, G. and Tedder, A. (2018). On elimination of quantifiers in some non-classical mathematical theories. *Mathematical Logic Quarterly*, 64(3):140–154.

[Badia and Weber, 2019] Badia, G. and Weber, Z. (2019). A substructural logic for inconsistent mathematics. In *Dialetheism and Its Applications*. Springer, Cham.

[Badia et al., 2022] Badia, G., Weber, Z., and Girard, P. (2022). Paraconsistent metatheory: new proofs with old tools. *Journal of Philosophical Logic*.

[Balaguer, 2001] Balaguer, M. (2001). *Platonism and anti-platonism in mathematics*. Oxford University Press on Demand.

[Baldwin, 2020] Baldwin, J. T. (2020). The reasonable effectiveness of model theory in mathematics. *Journal of Applied Logics - IfCoLog Journal of Logics and their Application*, 7(6).

[Barnes, 2020] Barnes, E. (2020). Gender and gender terms. *Noûs*, 54(3):704–730.

[Barwise and Feferman, 2017] Barwise, J. and Feferman, S. (2017). *Model-theoretic logics*. Cambridge University Press.

[Batens, 2000] Batens, D. (2000). Minimally abnormal models in some adaptive logics. *Synthese*, 125(1):5–18.

[Batens, 2002] Batens, D. (2002). In defence of a programme for handling inconsistencies. In *Inconsistency in science*, pages 129–150. Springer.

[Batens, 2013] Batens, D. (2013). New arguments for adaptive logics as unifying frame for the defeasible handling of inconsistency. In *Paraconsistency: Logic and Applications*, pages 101–122. Springer.

[Batens, 2019] Batens, D. (2019). Adaptive Fregean set theory. *Studia Logica*, pages 1–37.

[Batens et al., 2009] Batens, D., Clercq, K. D., Verdée, P., and Meheus, J. (2009). Yes fellows, most human reasoning is complex. *Synthese*, 166(1):113–131.

[Batens et al., 1999] Batens, D., De Clercq, K., and Kurtonina, N. (1999). Embedding and interpolation for some paralogics. the propositional case. *Reports Math. Log.*, 33:29–44.

[Bauer, 2017] Bauer, A. (2017). Five stages of accepting constructive mathematics. *Bulletin of the American Mathematical Society*, 54(3):481–498.

[Beall, 1999] Beall, J. (1999). From full blooded platonism to really full blooded platonism. *Philosophia Mathematica*, 7(3):322–325.

[Beall, 2009] Beall, J. (2009). *Spandrels of truth*. Oxford University Press.

[Beall, 2011] Beall, J. (2011). Adding to relevant restricted quantification. *The Australasian Journal of Logic*, 10:36–44.

[Beall, 2013a] Beall, J. (2013a). LP+, K3+, FDE+, and their 'classical collapse'. *The Review of Symbolic Logic*, 6(4):742–754.

[Beall, 2013b] Beall, J. (2013b). Shrieking against gluts: the solution to the 'just true'problem. *Analysis*, 73(3):438–445.

[Beall, 2013c] Beall, J. (2013c). A simple approach towards recapturing consistent theories in paraconsistent settings. *The Review of Symbolic Logic*, 6(4):755–764.

[Beall et al., 2006] Beall, J., Brady, R. T., Hazen, A. P., Priest, G., and Restall, G. (2006). Relevant restricted quantification. *Journal of Philosophical Logic*, 35(6):587–598.

[Beall and Murzi, 2013] Beall, J. and Murzi, J. (2013). Two flavors of Curry's paradox. *The Journal of Philosophy*, 110(3):143–165.

[Bell, 2011] Bell, J. L. (2011). *Set theory: Boolean-valued models and independence proofs*. OUP Oxford.

[Benham et al., 2014] Benham, R., Mortensen, C., and Priest, G. (2014). Chunk and permeate III: the Dirac delta function. *Synthese*, 191(13):3057–3062.

[Berkeley, 1754] Berkeley, G. (1754). *The Analyst; Or, A Discourse Addressed to an Infidel Mathematician: Wherein it is Examined Whether the Object, Principles, and Inferences of the Modern Analysis are More Distinctly Conceived, Or More Evidently Deduced, Than Religious Mysteries and Points of Faith*. J. and R. Tonson and S. Draper.

[Berto, 2007] Berto, F. (2007). *How to sell a contradiction: The logic and metaphysics of inconsistency*. College Publications.

[Bettcher, 2013] Bettcher, T. M. (2013). Trans women and the meaning of 'woman'. In *The Philosophy of Sex, 6th Edition*, pages 233–250. Rowman & Littlefield.

[Bettcher, 2015] Bettcher, T. M. (2015). Intersexuality, transgender, and transsexuality. In *The Oxford Handbook of Feminist Theory*, pages 407–427. Oxford University Press.

[Bishop, 1975] Bishop, E. (1975). The crisis in contemporary mathematics. *Historia Mathematica*, 2(4):507–517.

[Bishop and Bridges, 2012] Bishop, E. and Bridges, D. (2012). *Constructive analysis*. Springer Science & Business Media.

[Bloor, 1991] Bloor, D. (1991). *Knowledge and social imagery*. University of Chicago Press.

[Bogart and Doyle, 1986] Bogart, K. P. and Doyle, P. G. (1986). Non-sexist solution of the ménage problem. *The American mathematical monthly*, 93(7):514–518.

[Bollobás, 1998] Bollobás, B. (1998). *Modern graph theory*. Springer Science & Business Media.

[Bornstein, 1994] Bornstein, K. (1994). *Gender outlaw*. Vintage Books New York.

[Brady, 2004] Brady, R. (2004). On the formalization of the law of non-contradiction. In *The Law of Non-Contradiction. New Philosophical Essays*, pages 41–48. Clarendon Press Oxford.

[Brady, 2006] Brady, R. (2006). *Universal logic*. Center for the Study of Language and Information.

[Brady, 1989] Brady, R. T. (1989). The non-triviality of extensional dialectical set theory. *Paraconsistent Logic*, pages 415–436.

[Brouwer, 1908] Brouwer, L. (1908). De onbetrouwbaarheid der logische principes. *Tijdschrift voor wijsbegeerte*, 2:152–158.

[Brouwer, 1913] Brouwer, L. E. J. (1913). Intuitionism and formalism. *Bulletin of the American Mathematical Society*, 20(2):81–96.

[Brown and Priest, 2004] Brown, B. and Priest, G. (2004). Chunk and permeate, a paraconsistent inference strategy. part I: The infinitesimal calculus. *Journal of Philosophical Logic*, 33(4):379–388.

[Brown and Priest, 2015] Brown, M. B. and Priest, G. (2015). Chunk and permeate II: Bohr's hydrogen atom. *European Journal for Philosophy of Science*, 5(3):297–314.

[Bueno, 2002] Bueno, O. (2002). Can a paraconsistent theorist be a logical monist? In *Paraconsistency: the logical way to the inconsistent*, pages 535–552. Marcel Dekker, Inc.

[Bueno, 2007] Bueno, O. (2007). Incommensurability in mathematics. In *Perspectives on mathematical practices*, pages 83–105. Springer.

[Bueno and da Costa, 2007] Bueno, O. and da Costa, N. C. (2007). Quasi-truth, paraconsistency, and the foundations of science. *Synthese*, 154:383–399.

[Burgess, 2009] Burgess, J. P. (2009). *Philosophical logic*. Princeton University Press.

[Burgess, 2015] Burgess, J. P. (2015). *Rigor and structure*. Oxford University Press, USA.

[Burton, 1995] Burton, L. (1995). Moving towards a feminist epistemology of mathematics. *Educational Studies in Mathematics*, 28(3):275–291.

[Button and Walsh, 2018] Button, T. and Walsh, S. (2018). *Philosophy and model theory*. Oxford University Press.

[Byers, 2010] Byers, W. (2010). *How mathematicians think: Using ambiguity, contradiction, and paradox to create mathematics*. Princeton University Press.

[Cappelen, 2018] Cappelen, H. (2018). *Fixing language: An essay on conceptual engineering*. Oxford University Press.

[Caret, 2021] Caret, C. R. (2021). Why logical pluralism? *Synthese*, 198(20):4947–4968.

[Carnielli and Coniglio, 2013] Carnielli, W. and Coniglio, M. E. (2013). Paraconsistent set theory by predicating on consistency. *Journal of Logic and Computation*, 26(1):97–116.

[Carnielli and Coniglio, 2021] Carnielli, W. and Coniglio, M. E. (2021). Twist-valued models for three-valued paraconsistent set theory. *Logic and Logical Philosophy*, 30:187–226.

[Carnielli et al., 2014] Carnielli, W., Coniglio, M. E., Podiacki, R., and Rodrigues, T. (2014). On the way to a wider model theory: Completeness theorems for first-order logics of formal inconsistency. *Review of Symbolic Logic*, 7(3):548–578.

[Carnielli and Rodrigues, 2019] Carnielli, W. and Rodrigues, A. (2019). An epistemic approach to paraconsistency: a logic of evidence and truth. *Synthese*, 196(9):3789–3813.

[Carnielli and Coniglio, 2016] Carnielli, W. A. and Coniglio, M. E. (2016). *Paraconsistent logic: Consistency, contradiction and negation*. Springer.

[Cartwright, 1983] Cartwright, N. (1983). *How the laws of physics lie*. Oxford Clarendon Press.

[Carvalho, 2004] Carvalho, T. F. (2004). *Sobre o infinitésimo e o cálculo diferencial paraconsistente de da Costa*. PhD thesis, Philosophy and Human Sciences Institute, State University of Campinas.

[Chang and Keisler, 1990] Chang, C. C. and Keisler, H. J. (1990). *Model theory*. Elsevier.

[Cohen, 1985] Cohen, I. B. (1985). *Revolution in science*. Harvard University Press.

[Cohen, 2008] Cohen, P. J. (2008). *Set theory and the continuum hypothesis*. Courier Corporation.

[Cole, 2013] Cole, J. (2013). Towards an institutional account of the objectivity, necessity, and atemporality of mathematics. *Philosophia Mathematica*, 21(1):9–36.

[Collins, 2021] Collins, R. W. (2021). Modeling gender as a multidimensional sorites paradox. *Hypatia*, 36(2):302–320.

[Colyvan, 2008] Colyvan, M. (2008). Who's afraid of inconsistent mathematics? *ProtoSociology*, 25:23–35.

[Coniglio et al., 2020] Coniglio, M. E., Figallo-Orellano, A., and Golzio, A. C. (2020). First-order swap structures semantics for some logics of formal inconsistency. *Journal of Logic and Computation*, 30(6):1257–1290.

[Coppola and Gerla, 2014] Coppola, C. and Gerla, G. (2014). Multi-valued logic for a point-free foundation of geometry. In *Mereology and the Sciences*, pages 105–122. Springer.

[Cotnoir, 2018] Cotnoir, A. J. (2018). Logical nihilism. In *Pluralisms in truth and logic*, pages 301–329. Springer.

[Cowan, 1974] Cowan, T. M. (1974). The theory of braids and the analysis of impossible figures. *Journal of Mathematical Psychology*, 11(3):190–212.

[Cowan and Pringle, 1978] Cowan, T. M. and Pringle, R. (1978). An investigation of the cues responsible for figure impossibility. *Journal of Experimental Psychology: human perception and performance*, 4(1):112.

[Crabbé, 2011] Crabbé, M. (2011). Reassurance for the logic of paradox. *The Review of Symbolic Logic*, 4(3):479–485.

[Crosilla, 2009] Crosilla, L. (2009). Set theory: Constructive and intuitionistic ZF. In *The Stanford Encyclopedia of Philosophy*. The Metaphysics Research Lab.

[Crowe, 1967] Crowe, M. J. (1967). Science a century ago. In *Science and Contemporary Society*, pages 105–126. University of Notre Dame Press Notre Dame, Ind.

[Crowe, 1975] Crowe, M. J. (1975). Ten "laws" concerning patterns of change in the history of mathematics. *Historia Mathematica*, 2(2):161–166.

[da Costa, 1964] da Costa, N. C. (1964). Sur un système inconsistant de théorie des ensembles. *Comptes Rendus Hebdomadaires Des Seances De L'Academie Des Sciences*, 258(12):3144–3147.

[da Costa, 1986] da Costa, N. C. (1986). On paraconsistent set theory. *Logique et Analyse*, 29(115):361–371.

[da Costa, 2000] da Costa, N. C. (2000). Paraconsistent mathematics. In *Frontiers of paraconsistent logic*, pages 166–179. Research Studies Press Ltd., Baldock, Hartfordshire, England.

[da Costa et al., 2004] da Costa, N. C., Bueno, O., and Volkov, A. (2004). Outline of a paraconsistent category theory. In *Alternative Logics. Do Sciences Need Them?*, pages 95–114. Springer.

[Daly, 2017] Daly, H. L. (2017). Modelling sex/gender. *Think*, 16(46):79–92.

[Dauben, 1984] Dauben, J. (1984). Conceptual revolutions and the history of mathematics: two studies in the growth of knowledge (1984). In *Revolutions in mathematics*. Oxford Science Publications.

[Dauben, 1992] Dauben, J. (1992). Appendix (1992): revolutions revisited. In *Revolutions in mathematics*. Oxford Science Publications.

[De Toffoli, 2021] De Toffoli, S. (2021). Groundwork for a fallibilist account of mathematics. *The Philosophical Quarterly*, 71(4):pqaa076.

[De Toffoli and Giardino, 2015] De Toffoli, S. and Giardino, V. (2015). An inquiry into the practice of proving in low-dimensional topology. In *From logic to practice*, pages 315–336. Springer.

[Dembroff, 2018] Dembroff, R. (2018). Real talk on the metaphysics of gender. *Philosophical Topics*, 46(2):21–50.

[Dembroff, 2019] Dembroff, R. (2019). Beyond binary: genderqueer as critical gender kind. *Philosopher's Imprint*.

[Detlefsen, 2013] Detlefsen, M. (2013). *Hilbert's program: an essay on mathematical instrumentalism*. Springer Science & Business Media.

[Detlefsen, 2014] Detlefsen, M. (2014). Duality, epistemic efficiency & consistency. In *Formalism and Beyond: On the Nature of Mathematical Discourse*, pages 1–23. Walter de Gruyter GmbH & Co KG.

[Dotson, 2012a] Dotson, K. (2012a). A cautionary tale: On limiting epistemic oppression. *Frontiers: A Journal of Women Studies*, 33(1):24–47.

[Dotson, 2012b] Dotson, K. (2012b). How is this paper philosophy? *Comparative Philosophy*, 3(1):121–121.

[D'Ottaviano and de Carvalho, 2005] D'Ottaviano, I. M. L. and de Carvalho, T. F. (2005). Da Costa's paraconsistent differential calculus and a transference theorem. In *IICAI*, pages 1659–1678.

[Dubbs, 2016] Dubbs, C. (2016). A queer turn in mathematics education research: Centering the experience of marginalized queer students. *North American Chapter of the International Group for the Psychology of Mathematics Education*.

[Dunmore, 1992] Dunmore, C. (1992). Meta-level revolutions in mathematics. In *Revolutions in mathematics*. Oxford Science Publications.

[Dunn, 1979] Dunn, J. M. (1979). A theorem in 3-valued model theory with connections to number theory, type theory, and relevant logic. *Studia Logica*, 38(2):149–169.

[Dunn, 1980] Dunn, J. M. (1980). Quantum mathematics. In *PSA: Proceedings of the Biennial Meeting of the Philosophy of Science Association*, pages 512–531. Cambridge University Press.

[Eckert, 202x] Eckert, M. (202x). De-centering and genderqueering Val Plumwood's feminist logic. In Cook, R. and Yap, A., editors, *Feminist Philosophy and Formal Logic*. University of Minnesota Press. Forthcoming.

[Eckert and Donahue, 2020] Eckert, M. and Donahue, C. (2020). Towards a feminist logic: Val Plumwood's legacy and beyond. In *Ultralogic as universal?* Springer.

[Eichler, 2018] Eichler, L. (2018). Sacred truths, fables, and falsehoods: Intersections between feminist and native american logics. *APA Newsletter on Native American and Indigenous Philosophy*, 18(1).

[Engelking, 1977] Engelking, R. (1977). *General topology.* Heldermann Verlag.

[Ernest, 1998] Ernest, P. (1998). *Social constructivism as a philosophy of mathematics*. State University of New York Press.

[Ernest, 2018] Ernest, P. (2018). The ethics of mathematics: is mathematics harmful? In *The philosophy of mathematics education today*, pages 187–216. Springer.

[Estrada-González, 2011] Estrada-González, L. (2011). On the meaning of connectives (apropos of a non-necessitarianist challenge). *Logica Universalis*, 5(1):115–126.

[Estrada-González, 2015] Estrada-González, L. (2015). The evil twin: the basics of complement-toposes. In *New Directions in Paraconsistent Logic*, pages 375–425. Springer.

[Estrada-González, 2016] Estrada-González, L. (2016). Prospects for triviality. In *Logical Studies of Paraconsistent Reasoning in Science and Mathematics*, pages 81–89. Springer.

[Estrada-Gonzalez and Tapia-Navarro, 2021] Estrada-Gonzalez, L. and Tapia-Navarro, M. (2021). "A smack of irrelevance" in inconsistent mathematics? *The Australasian Journal of Logic*, 18(5):503–523.

[Fallis, 2003] Fallis, D. (2003). Intentional gaps in mathematical proofs. *Synthese*, 134(1/2):45–69.

[Falmagne and Hass, 2002] Falmagne, R. J. and Hass, M. (2002). *Representing reason: Feminist theory and formal logic*. Rowman & Littlefield Publishers.

[Ferguson, 2012] Ferguson, T. M. (2012). Notes on the model theory of Demorgan logics. *Notre Dame Journal of Formal Logic*, 53(1):113–132.

[Ferguson, 2017] Ferguson, T. M. (2017). Dunn–Priest quotients of many-valued structures. *Notre Dame Journal of Formal Logic*, 58(2):221–239.

[Ferguson, 2019a] Ferguson, T. M. (2019a). Inconsistent models (and infinite models) for arithmetics with constructible falsity. *Logic and Logical Philosophy*, pages 1–19.

[Ferguson, 2019b] Ferguson, T. M. (2019b). Variations on the collapsing lemma. In *Graham Priest on Dialetheism and Paraconsistency*. Springer.

[Ferguson, 2020] Ferguson, T. M. (2020). The Keisler–Shelah theorem for QmbC through semantical atomization. *Logic Journal of the IGPL*, 28(5):912–935.

[Ferguson, 2023] Ferguson, T. M. (2023). From excluded middle to homogenization in Plumwood's feminist critique of logic. *Australasian Journal of Logic*. Forthcoming.

[Ferguson and Ramirez-Camara, 2021] Ferguson, T. M. and Ramirez-Camara, E. (2021). Episodes in model-theoretic xenology: Rationals as positive integers in R♯. *The Australasian Journal of Logic*, 18(5):428–446.

[Ferreirós, 2015] Ferreirós, J. (2015). *Mathematical knowledge and the interplay of practices*. Princeton University Press.

[Finsler and Ziegler, 1996] Finsler, P. and Ziegler, R. (1996). *Finsler Set Theory: Platonism and Circularity; Translations of Paul Finsler's Papers on Set Theory with Introductory Comments*. Springer Science & Business Media.

[Fletcher, 2017] Fletcher, S. C. (2017). Against the topologists: Essay review of new foundations for physical geometry, tim maudlin. *Philosophy of Science*, 84(3):595–603.

[Fontanella, 2019] Fontanella, L. (2019). How to choose new axioms for set theory? In *Reflections on the Foundations of Mathematics*, pages 27–42. Springer.

[Fowler, 1999] Fowler, D. H. (1999). *The mathematics of Plato's Academy: a new reconstruction*. Oxford University Press on Demand.

[Fraenkel, 1922] Fraenkel, A. A. (1922). The notion of "definite" and the independence of the axiom of choice. In *From Frege to Gödel: a source book in mathematical logic, 1879-1931*, pages 284–289. Harvard University Press.

[Fraenkel et al., 1973] Fraenkel, A. A., Bar-Hillel, Y., and Levy, A. (1973). *Foundations of set theory*. Elsevier.

[Francez, 2021] Francez, N. (2021). *A View of Connexive Logics*. College Publications.

[Francis, 1987] Francis, G. (1987). *A topological picturebook*. Springer.

[Franks, 2009] Franks, C. (2009). *The autonomy of mathematical knowledge: Hilbert's program revisited*. Cambridge University Press.

[Franks, 2015] Franks, C. (2015). Logical nihilism. In *Logic Without Borders*, pages 147–166. De Gruyter.

[Frege, 1884] Frege, G. (1884). *The foundations of arithmetic: A logico-mathematical enquiry into the concept of number*. Northwestern University Press.

[Frege, 1893] Frege, G. (1893). *The basic laws of arithmetic*. University of California Press.

[Fricker, 2007] Fricker, M. (2007). *Epistemic injustice: Power and the ethics of knowing*. Oxford University Press.

[Friedman and Meyer, 1992] Friedman, H. and Meyer, R. K. (1992). Whither relevant arithmetic? *Journal of Symbolic Logic*, pages 824–831.

[Garavaso, 2016] Garavaso, P. (2016). The woman of reason: On the re-appropriation of rationality and the enjoyment of philosophy. In *Meta-Philosophical Reflection on Feminist Philosophies of Science*, pages 185–202. Springer.

[Gentzen, 1936] Gentzen, G. (1936). Die widerspruchsfreiheit der reinen zahlentheorie. *Mathematische annalen*, 112(1):493–565.

[Gillies, 1992] Gillies, D. (1992). *Revolutions in mathematics*. Clarendon Press.

[Girard and Weber, 2015] Girard, P. and Weber, Z. (2015). Bad worlds. *Thought: A Journal of Philosophy*, 4(2):93–101.

[Goldblatt, 2014] Goldblatt, R. (2014). *Topoi: the categorial analysis of logic*. Elsevier.

[Gonshor, 1986] Gonshor, H. (1986). *An introduction to the theory of surreal numbers*. Cambridge University Press.

[Goodman, 1978] Goodman, N. (1978). *Ways of worldmaking*. Hackett Publishing.

[Goodship, 1996] Goodship, L. (1996). On dialethism. *Australasian Journal of Philosophy*, 74(1):153–161.

[Gouvêa, 1997] Gouvêa, F. Q. (1997). *p-adic Numbers: An Introduction*. Springer.

[Gowers et al., 2008] Gowers, T., Barrow-Green, J., and Leader, I. (2008). *The Princeton companion to mathematics*. Princeton University Press.

[Grosholz, 2007] Grosholz, E. (2007). *Representation and productive ambiguity in mathematics and the sciences*. Oxford University Press on Demand.

[Hall, 2017] Hall, K. Q. (2017). Queer epistemology and epistemic injustice. In *The Routledge handbook of epistemic injustice*, pages 158–166. Routledge.

[Halperin and Schwartz, 1952] Halperin, I. and Schwartz, L. (1952). *Introduction to the Theory of Distributions*. University of Toronto Press.

[Harman, 1986] Harman, G. (1986). *Change in view: Principles of reasoning*. The MIT Press.

[Hart, 1993] Hart, C. G. (1993). "Power in the service of love": John Dewey's Logic and the dream of a common language. *Hypatia*, 8(2):190–214.

[Haslanger, 2006] Haslanger, S. (2006). What good are our intuitions: Philosophical analysis and social kinds. In *Aristotelian Society Supplementary Volume*, volume 80.

[Hass, 1999] Hass, M. (1999). Can there be a feminist logic? In *Is feminist philosophy philosophy?*, pages 190–201. Northwestern University Press.

[Hazen and Pelletier, 2018] Hazen, A. P. and Pelletier, F. J. (2018). Second-order logic of paradox. *Notre Dame Journal of Formal Logic*, 59(4):547–558.

[Heck, 1993] Heck, R. G. (1993). The development of arithmetic in frege's grundgesetze der arithmetik. *Journal of Symbolic Logic*, 58(2):579–601.

[Helmholtz, 1876] Helmholtz, H. v. (1876). The origin and meaning of geometrical axioms. *Mind*, 1(3):301–321.

[Heyninck et al., 2018] Heyninck, J., Verdée, P., and Heeffer, A. (2018). Handling inconsistencies in the early calculus. *Journal of Philosophical Logic*, 47(3):481–511.

[Hilbert, 1927] Hilbert, D. (1927). The foundations of mathematics. In *From Frege to Gödel: a source book in mathematical logic, 1879-1931*, pages 464–479. Harvard University Press.

[Hinnion, 2003] Hinnion, R. (2003). About the coexistence of "classical sets" with "non-classical" ones: A survey. *Logic and Logical Philosophy*, 11:79–90.

[Hodges, 1993] Hodges, W. (1993). *Model theory*. Cambridge University Press.

[Humberstone, 2020] Humberstone, L. (2020). Explicating logical independence. *Journal of Philosophical Logic*, 49(1):135–218.

[Incurvati, 2020] Incurvati, L. (2020). *Conceptions of Set and the Foundations of Mathematics*. Cambridge University Press.

[Intemann, 2010] Intemann, K. (2010). 25 years of feminist empiricism and standpoint theory: Where are we now? *Hypatia*, 25(4):778–796.

[Istre, 2017] Istre, E. (2017). *Normalized naive set theory.* PhD thesis, University of Canterbury.

[Jech, 2003] Jech, T. (2003). *Set theory*. Springer.

[Jennings and Schotch, 2009] Jennings, R. E. and Schotch, P. K. (2009). *On preserving: Essays on preservationism and paraconsistent logic*. University of Toronto Press.

[Jockwich-Martinez, 2021] Jockwich-Martinez, S. (2021). Algebra-valued models for LP-set theory. *Australasian Journal of Logic*, 18(7):657–687.

[Jockwich-Martinez et al., 2021] Jockwich-Martinez, S., Tarafder, S., and Venturi, G. (2021). ZF and its interpretations. Unpublished manuscript, available at https://www.researchgate.net/publication/360938319_ZF_AND_ITS_INTERPRETATIONS.

[Kanamori, 2008] Kanamori, A. (2008). *The higher infinite: large cardinals in set theory from their beginnings*. Springer Science & Business Media.

[Katz, 2020] Katz, B. (2020). Queering mathematics. https://blogs.ams.org/inclusionexclusion/2020/11/20/queering-mathematics/ last accessed on 23.11.22.

[Kaye, 1991] Kaye, R. (1991). *Models of Peano arithmetic*. Clarendon Press Oxford.

[Kechris, 2012] Kechris, A. (2012). *Classical Descriptive Set Theory*. Graduate Texts in Mathematics. Springer New York.

[Kennedy, 2013] Kennedy, J. (2013). On formalism freeness: Implementing Gödel's 1946 princeton bicentennial lecture. *Bulletin of Symbolic Logic*, 19(3):351–393.

[Kennedy, 2020] Kennedy, J. (2020). *Gödel, Tarski and the Lure of Natural Language: Logical Entanglement, Formalism Freeness*. Cambridge University Press.

[Kitcher, 1984] Kitcher, P. (1984). *The nature of mathematical knowledge*. Oxford University Press on Demand.

[Kleiner and Movshovitz-Hadar, 1994] Kleiner, I. and Movshovitz-Hadar, N. (1994). The role of paradoxes in the evolution of mathematics. *The American mathematical monthly*, 101(10):963–974.

[Kock, 2006] Kock, A. (2006). *Synthetic differential geometry*. Cambridge University Press.

[Kouri Kissel, 2018] Kouri Kissel, T. (2018). Logical pluralism from a pragmatic perspective. *Australasian Journal of Philosophy*, 96(3):578–591.

[Kuhn, 1970] Kuhn, T. (1970). *The structure of scientific revolutions*. University of Chicago Press.

[Lakatos, 2015] Lakatos, I. (2015). *Proofs and refutations: The logic of mathematical discovery*. Cambridge University Press.

[Lakoff and Núñez, 2000] Lakoff, G. and Núñez, R. (2000). *Where mathematics comes from*. New York: Basic Books.

[Larvor, 2012] Larvor, B. (2012). How to think about informal proofs. *Synthese*, 187(2):715–730.

[Leitgeb, 2009] Leitgeb, H. (2009). On formal and informal provability. In *New waves in philosophy of mathematics*, pages 263–299. Springer.

[Libert, 2005] Libert, T. (2005). Models for a paraconsistent set theory. *Journal of Applied Logic*, 3(1):15–41.

[Lloyd, 1993] Lloyd, G. (1993). *The man of reason:" Male" and" female" in western philosophy*. Routledge.

[Logan, 2019] Logan, S. A. (2019). Notes on stratified semantics. *Journal of Philosophical Logic*, 48:749–786.

[Logan and Leach-Krouse, 2021] Logan, S. A. and Leach-Krouse, G. (2021). On not saying what we shouldn't have to say. *The Australasian Journal of Logic*, 18(5):524–568.

[Longino, 2002] Longino, H. E. (2002). *The fate of knowledge*. Princeton University Press.

[Löwe and Tarafder, 2015] Löwe, B. and Tarafder, S. (2015). Generalized algebra-valued models of set theory. *The Review of Symbolic Logic*, 8(1):192–205.

[Maddy, 2011] Maddy, P. (2011). *Defending the axioms: On the philosophical foundations of set theory*. Oxford University Press.

[Maddy, 2019] Maddy, P. (2019). What do we want a foundation to do? In *Reflections on the Foundations of Mathematics*, pages 293–311. Springer.

[Mancosu, 2016] Mancosu, P. (2016). *Abstraction and infinity*. Oxford University Press.

[Mancosu et al., 2021] Mancosu, P., Galvan, S., and Zach, R. (2021). *An Introduction to Proof Theory: Normalization, Cut-elimination, and Consistency Proofs*. Oxford University Press.

[Mangraviti, 2023] Mangraviti, F. (2023). The liberation argument for inconsistent mathematics. *Australasian Journal of Logic*. Forthcoming.

[Mangraviti and Tedder, 2023] Mangraviti, F. and Tedder, A. (2023). Consistent theories in inconsistent logics. *Journal of Philosophical Logic*. Published online.

[Marcos, 2005] Marcos, J. (2005). Nearly every normal modal logic is paranormal. *Logique et Analyse*, 48(189/192):279–300.

[Mares, 2004] Mares, E. D. (2004). *Relevant logic: A philosophical interpretation*. Cambridge University Press.

[Martínez, 2018] Martínez, A. A. (2018). *Negative math: How mathematical rules can be positively bent*. Princeton University Press.

[Mathematical Reviews and ZbMATH, 2020] Mathematical Reviews and ZbMATH (2020). Mathematics Subject Classification - MSC2020. https://zbmath.org/classification/ last accessed on 08.12.22.

[McCall, 1967] McCall, S. (1967). Connexive class logic. *The Journal of Symbolic Logic*, 32(1):83–90.

[McCullough-Benner, 2020] McCullough-Benner, C. (2020). Representing the world with inconsistent mathematics. *The British Journal for the Philosophy of Science*, 71(4):1331–1358.

[McKubre-Jordens and Weber, 2017] McKubre-Jordens, M. and Weber, Z. (2017). Paraconsistent measurement of the circle. *Australasian Journal of Logic*, 1(10):14.

[McVitie and Wilson, 1971] McVitie, D. G. and Wilson, L. B. (1971). The stable marriage problem. *Communications of the ACM*, 14(7):486–490.

[Meadows and Weber, 2016] Meadows, T. and Weber, Z. (2016). Computation in non-classical foundations? *Philosophers' Imprint*, 16(13):1–17.

[Meheus, 2002] Meheus, J. (2002). How to reason sensibly yet naturally from inconsistencies. In *Inconsistency in science*, pages 151–164. Springer.

[Mendonça and Carnielli, 2018] Mendonça, B. R. and Carnielli, W. A. (2018). Fraïssé's theorem for logics of formal inconsistency. *Logic Journal of the IGPL*, 28(5):1060–1072.

[Meyer, 2021a] Meyer, R. (2021a). Arithmetic formulated relevantly. *The Australasian Journal of Logic*, 18(5):154–288.

[Meyer, 2021b] Meyer, R. (2021b). The consistency of arithmetic. *The Australasian Journal of Logic*, 18(5):289–379.

[Meyer, 1976] Meyer, R. K. (1976). Relevant arithmetic. *Bulletin of the Section of Logic of the Polish Academy of Sciences*, 5:133–137.

[Meyer and Mortensen, 1984] Meyer, R. K. and Mortensen, C. (1984). Inconsistent models for relevant arithmetics. *The Journal of Symbolic Logic*, 49(3):917–929.

[Meyer and Mortensen, 1987] Meyer, R. K. and Mortensen, C. (1987). Alien intruders in relevant arithmetic. Technical report, Australian National University.

[Meyer and Slaney, 1989] Meyer, R. K. and Slaney, J. K. (1989). Abelian logic (from A to Z). In *Paraconsistent logic: Essays on the inconsistent*, pages 245–288. Philosophia Verlag.

[Meyer and Sylvan, 1977] Meyer, R. K. and Sylvan, R. (1977). Extensional reduction—I. *The Monist*, pages 355–369.

[Michael, 2016] Michael, M. (2016). On a "most telling" argument for paraconsistent logic. *Synthese*, 193(10):3347–3362.

[Mortensen, 1987] Mortensen, C. (1987). Inconsistent nonstandard arithmetic. *The Journal of symbolic logic*, 52(2):512–518.

[Mortensen, 1988] Mortensen, C. (1988). Inconsistent number systems. *Notre Dame J. Formal Log.*, 29(1):45–60.

[Mortensen, 1989] Mortensen, C. (1989). Anything is possible. *Erkenntnis*, 30(3):319–337.

[Mortensen, 1990] Mortensen, C. (1990). Models for inconsistent and incomplete differential calculus. *Notre Dame Journal of Formal Logic*, 31(2):274–285.

[Mortensen, 1995] Mortensen, C. (1995). *Inconsistent mathematics*. Dordrecht: Kluwer.

[Mortensen, 1997] Mortensen, C. (1997). Peeking at the impossible. *Notre Dame Journal of Formal Logic*, 38(4):527–534.

[Mortensen, 2000] Mortensen, C. (2000). Prospects for inconsistency. In *Frontiers of paraconsistent logic*, pages 203–208. Research Studies Press LTD.

[Mortensen, 2002] Mortensen, C. (2002). Toward a mathematics of impossible pictures. In *Paraconsistency*, pages 468–477. CRC Press.

[Mortensen, 2006] Mortensen, C. (2006). An analysis of inconsistent and incomplete Necker cubes. *The Australasian Journal of Logic*, 4:216–225.

[Mortensen, 2009a] Mortensen, C. (2009a). Inconsistent mathematics: Some philosophical implications. In *Philosophy of Mathematics*, pages 631–649. Elsevier.

[Mortensen, 2009b] Mortensen, C. (2009b). Linear algebra representation of Necker cubes I: The crazy crate. *The Australasian Journal of Logic*, 7:1–9.

[Mortensen, 2009c] Mortensen, C. (2009c). Linear algebra representation of Necker cubes II: The Routley functor and Necker chains. *The Australasian Journal of Logic*, 7:10–25.

[Mortensen, 2010] Mortensen, C. (2010). *Inconsistent geometry*. College Publications.

[Mortensen, 2013a] Mortensen, C. (2013a). Arithmetic starred. In *Paraconsistency: Logic and Applications*, pages 309–314. Springer.

[Mortensen, 2013b] Mortensen, C. (2013b). Motion perception as inconsistent. *Philosophical Psychology*, 26(6):913–924.

[Mortensen, 2017] Mortensen, C. (2017). Inconsistent mathematics. In *The Stanford Encyclopedia of Philosophy (Fall 2017 Edition)*. The Metaphysics Research Lab.

[Mortensen, 2019] Mortensen, C. (2019). Implication principles in Routley arithmetic. In *Ultralogic as Universal?*, pages 185–194. Springer.

[Mortensen, 2022] Mortensen, C. (2022). *The Impossible Arises: Oscar Reutersvärd and His Contemporaries*. Indiana University Press.

[Muntersbjorn, 2007] Muntersbjorn, M. (2007). Mathematical progress as increased scope. In *Perspectives On Mathematical Practices*, pages 107–117. Springer.

[Nash, 1956] Nash, J. (1956). The imbedding problem for riemannian manifolds. *Annals of mathematics*, pages 20–63.

[Nelson, 1949] Nelson, D. (1949). Constructible falsity. *The Journal of Symbolic Logic*, 14(1):16–26.

[Nelson, 1959] Nelson, D. (1959). Negation and separation of concepts in constructive systems. *Constructivity in mathematics*, 39:205–225.

[Nelson, 2015] Nelson, E. (2015). Inconsistency of primitive recursive arithmetic. *arXiv preprint arXiv:1509.09209*.

[Nye, 1990] Nye, A. (1990). *Words of power: A feminist reading of the history of logic*. Routledge.

[Nye, 2002] Nye, A. (2002). Predicate logic and natural kinds. *Representing reason: Feminist theory and formal logic*, page 191.

[Oddsson, 2021] Oddsson, H. V. (2021). Paradefinite Zermelo-Fraenkel set theory: A theory of inconsistent and incomplete sets. Master's thesis, University of Amsterdam.

[O'Donovan, 2007] O'Donovan, R. (2007). Pre-university analysis. In *The Strength of Nonstandard Analysis*, pages 395–401. Springer.

[Øgaard, 2020] Øgaard, T. F. (2020). Farewell to suppression-freedom. *Logica Universalis*, 14(3):297–330.

[Ohara, 2006] Ohara, Y. (2006). On the significance of alternative mathematics in a school context. *Research bulletin of Naruto University of Education*, 21:175–182.

[Oliver, 1991] Oliver, P. (1991). What do girls know anyway?': Rationality, gender and social control. *Feminism & Psychology*, 1(3):339–360.

[Olkowski, 2002] Olkowski, D. E. (2002). Words of power and the logic of sense. *Representing Reason: Feminist Theory and Formal Logic*, pages 117–129.

[Omori, 2015] Omori, H. (2015). Remarks on naive set theory based on LP. *The Review of Symbolic Logic*, 8(2):279–295.

[Omori, 2016] Omori, H. (2016). From paraconsistent logic to dialetheic logic. In *Logical Studies of Paraconsistent Reasoning in Science and Mathematics*, pages 111–134. Springer.

[Omori, 2020] Omori, H. (2020). From logics of formal inconsistency to logics of formal classicality. *Logic Journal of the IGPL*, 28(5):684–711.

[Omori and Weber, 2019] Omori, H. and Weber, Z. (2019). Just true? on the metatheory for paraconsistent truth. *Logique et Analyse*, 248:415–433.

[Paris and Pathmanathan, 2006] Paris, J. B. and Pathmanathan, N. (2006). A note on Priest's finite inconsistent arithmetics. *Journal of Philosophical Logic*, 35(5):529–537.

[Paris and Sirokofskich, 2008] Paris, J. B. and Sirokofskich, A. (2008). On LP-models of arithmetic. *The Journal of Symbolic Logic*, 73(1):212–226.

[Parsons, 1977] Parsons, C. (1977). What is the iterative conception of set? In *Logic, foundations of mathematics, and computability theory*, pages 335–367. Springer.

[Passmann, 2021] Passmann, R. (2021). Should pluralists be pluralists about pluralism? *Synthese*, 199(5):12663–12682.

[Penrose, 1992] Penrose, R. (1992). On the cohomology of impossible figures. *Leonardo*, pages 245–247.

[Plumwood, 1967] Plumwood, V. (1967). Some false laws of logic. *Australasian Journal of Logic*. Forthcoming.

[Plumwood, 1989] Plumwood, V. (1989). Do we need a sex/gender distinction? *Radical Philosophy*, 51(2):2–11.

[Plumwood, 1993a] Plumwood, V. (1993a). *Feminism and the Mastery of Nature*. Routledge.

[Plumwood, 1993b] Plumwood, V. (1993b). The politics of reason: Towards a feminist logic. *Australasian Journal of Philosophy*, 71(4):436–462.

[Pohlhaus, 2012] Pohlhaus, G. (2012). Relational knowing and epistemic injustice: Toward a theory of willful hermeneutical ignorance. *Hypatia*, 27(4):715–735.

[Posy, 2020] Posy, C. J. (2020). *Mathematical intuitionism*. Cambridge University Press.

[Pourciau, 2000] Pourciau, B. (2000). Intuitionism as a (failed) Kuhnian revolution in mathematics. *Studies in History and Philosophy of Science Part A*, 31(2):297–329.

[Priest, 1979] Priest, G. (1979). The logic of paradox. *Journal of Philosophical logic*, 8(1):219–241.

[Priest, 1991] Priest, G. (1991). Minimally inconsistent LP. *Studia Logica*, 50(2):321–331.

[Priest, 1994] Priest, G. (1994). Is arithmetic consistent? *Mind*, 103(411):337–349.

[Priest, 1997] Priest, G. (1997). Inconsistent models of arithmetic part I: Finite models. *Journal of Philosophical Logic*, 26(2):223–235.

[Priest, 2002] Priest, G. (2002). Paraconsistent logic. In *Handbook of philosophical logic*, pages 287–393. Springer.

[Priest, 2003] Priest, G. (2003). On alternative geometries, arithmetics, and logics; a tribute to Łukasiewicz. *Studia Logica*, 74(3):441–468.

[Priest, 2006a] Priest, G. (2006a). *Doubt Truth to be a Liar*. Oxford University Press.

[Priest, 2006b] Priest, G. (2006b). *In contradiction*. Oxford University Press.

[Priest, 2014a] Priest, G. (2014a). Contradictory concepts. In *Logic, Reasoning, and Rationality*, pages 197–215. Springer.

[Priest, 2014b] Priest, G. (2014b). Logical pluralism: another application for Chunk and Permeate. *Erkenntnis*, 79(2):331–338.

[Priest, 2017] Priest, G. (2017). What if? The exploration of an idea. *The Australasian Journal of Logic*, 14(1):54–127.

[Priest and Sylvan, 1989a] Priest, G. and Sylvan, R. (1989a). Applications of paraconsistent logic. In *Paraconsistent logic: Essays on the inconsistent*, pages 367–393. Philosophia Verlag.

[Priest and Sylvan, 1989b] Priest, G. and Sylvan, R. (1989b). The philosophical significance and inevitability of paraconsistency. In *Paraconsistent logic: Essays on the inconsistent*, pages 483–539. Philosophia Verlag.

[Priest and Sylvan, 1992] Priest, G. and Sylvan, R. (1992). Simplified semantics for basic relevant logics. *Journal of Philosophical Logic*, 21(2):217–232.

[Prokhovnik, 2002] Prokhovnik, R. (2002). *Rational woman: A feminist critique of dichotomy*. Manchester University Press.

[Raman-Sundström, 2015] Raman-Sundström, M. (2015). A pedagogical history of compactness. *The American mathematical monthly*, 122(7):619–635.

[Rav, 2007] Rav, Y. (2007). A critique of a formalist-mechanist version of the justification of arguments in mathematicians' proof practices. *Philosophia Mathematica*, 15(3):291–320.

[Restall, 1992] Restall, G. (1992). A note on naive set theory in LP. *Notre Dame Journal of Formal Logic*, 33(3):422–432.

[Restall, 1993] Restall, G. (1993). Simplified semantics for relevant logics (and some of their rivals). *Journal of Philosophical Logic*, 22(5):481–511.

[Restall, 2002] Restall, G. (2002). *An introduction to substructural logics*. Routledge.

[Restall, 2013] Restall, G. (2013). Assertion, denial and non-classical theories. In *Paraconsistency: Logic and applications*, pages 81–99. Springer.

[Restović, 2023] Restović, I. (2023). Feminist logic, literally. *Australasian Journal of Logic*. Forthcoming.

[Ripley, 2013] Ripley, D. (2013). Paradoxes and failures of Cut. *Australasian Journal of Philosophy*, 91:139 – 164.

[Ripley, 2015] Ripley, D. (2015). Naive set theory and nontransitive logic. *Review of Symbolic Logic*, 8(3).

[Ripley, 2018] Ripley, D. (2018). On the 'transitivity' of consequence relations. *Journal of Logic and Computation*, 28(2):433–450.

[Robinson, 2016] Robinson, A. (2016). *Non-standard analysis*. Princeton University Press.

[Ross, 1953] Ross, W. (1953). *Aristotle's Metaphysics*. Oxford: Clarendon Press.

[Rowbottom, 2011] Rowbottom, D. P. (2011). Kuhn vs. Popper on criticism and dogmatism in science: A resolution at the group level. *Studies in History and Philosophy of Science Part A*, 42(1):117–124.

[Russell, 2018] Russell, G. (2018). Logical nihilism: Could there be no logic? *Philosophical Issues*, 28(1).

[Russell, 2020] Russell, G. (2020). Logic: A feminist approach. In *Philosophy for Girls: An Invitation to the Life of Thought*, pages 79–98. Oxford University Press.

[Russell, 2023] Russell, G. (2023). From anti-exceptionalism to feminist logic. *Hypatia: A Journal of Feminist Philosophy*. Forthcoming.

[Saint-Croix, 2020] Saint-Croix, C. (2020). Privilege and position: Formal tools for standpoint epistemology. *Res Philosophica*, 97(4):489–524.

[Salamon, 2021] Salamon, G. (2021). *Queer Theory*. Oxford University Press.

[Sankappanavar and Burris, 2012] Sankappanavar, H. P. and Burris, S. (2012). *A course in universal algebra*. NSERC.

[Scharp, 2013] Scharp, K. (2013). *Replacing truth*. OUP Oxford.

[Scharp and Shapiro, 2017] Scharp, K. and Shapiro, S. (2017). Revising inconsistent concepts. *Reflections on the Liar*, pages 257–280.

[Schlimm, 2012] Schlimm, D. (2012). Mathematical concepts and investigative practice. In *Scientific Concepts and Investigative Practice*, pages 127–147. Berlin, de Gruyter GmbH.

[Shapiro, 2014] Shapiro, S. (2014). *Varieties of logic*. OUP Oxford.

[Shores, 2016] Shores, C. (2016). Dialetheism in the structure of phenomenal time. In *Logical Studies of Paraconsistent Reasoning in Science and Mathematics*, pages 145–157. Springer.

[Slaney, 2022] Slaney, J. (2022). Models of relevant arithmetic. *Australasian Journal of Logic*, 18(4).

[Slaney, 1987] Slaney, J. K. (1987). Reduced models for relevant logics without WI. *Notre Dame Journal of Formal Logic*, 28(3):395–407.

[Smith, 2013] Smith, P. (2013). *An introduction to Gödel's theorems*. Cambridge University Press.

[Standefer, 2022] Standefer, S. (2022). What is a relevant connective? *Journal of Philosophical Logic*, 51(4):919–950.

[Straßer, 2014] Straßer, C. (2014). *Adaptive logics for defeasible reasoning*. Springer.

[Sylvan, 1977] Sylvan, R. (1977). Ultralogic as universal? *Relevance Logic Newsletter*, 2:50–90, 138–175.

[Sylvan, 1979] Sylvan, R. (1979). Dialectical logic, semantics and metamathematics. *Erkenntnis*, 14:301–331.

[Sylvan, 2019] Sylvan, R. (2019). *Ultralogic as universal?* Springer.

[Sylvan and Meyer, 1976] Sylvan, R. and Meyer, R. K. (1976). Dialectical logic, classical logic, and the consistency of the world. *Studies in Soviet Thought*, 16:1–25.

[Sylvan and Plumwood, 1972] Sylvan, R. and Plumwood, V. (1972). The semantics of first degree entailment. *Noûs*, 6(4):335–359.

[Sylvan and Plumwood, 1985] Sylvan, R. and Plumwood, V. (1985). Negation and contradiction. *Revista colombiana de matematicas*, 19(1-2):201–230.

[Sylvan et al., 1982] Sylvan, R., Plumwood, V., Meyer, R. K., and Brady, R. T. (1982). *Relevant logics and their rivals*. Ridgeview Pub Co.

[Tanswell, 2015] Tanswell, F. (2015). A problem with the dependence of informal proofs on formal proofs. *Philosophia Mathematica*, 23(3):295–310.

[Tanswell, 2016] Tanswell, F. S. (2016). Saving proof from paradox: Gödel's paradox and the inconsistency of informal mathematics. In *Logical studies of paraconsistent reasoning in science and mathematics*, pages 159–173. Springer.

[Tanswell, 2018] Tanswell, F. S. (2018). Conceptual engineering for mathematical concepts. *Inquiry*, 61(8):881–913.

[Tanswell and Rittberg, 2020] Tanswell, F. S. and Rittberg, C. J. (2020). Epistemic injustice in mathematics education. *ZDM*, 52(6):1199–1210.

[Tao, 2007] Tao, T. (2007). What is good mathematics? *Bulletin of the American Mathematical Society*, 44(4):623–634.

[Tarafder and Chakraborty, 2015] Tarafder, S. and Chakraborty, M. K. (2015). A paraconsistent logic obtained from an algebra-valued model of set theory. In *New Directions in Paraconsistent Logic*, pages 165–183. Springer.

[Tarski, 1983] Tarski, A. (1983). *Logic, semantics, metamathematics: papers from 1923 to 1938*. Hackett Publishing.

[Tedder, 2015] Tedder, A. (2015). Axioms for finite collapse models of arithmetic. *Review of Symbolic Logic*, 8(3).

[Tedder, 2021] Tedder, A. (2021). Consistency and decidability in some paraconsistent arithmetics. *The Australasian Journal of Logic*, 18(5):473–502.

[Tennant, 2014] Tennant, N. (2014). Logic, mathematics, and the a priori, part II: Core logic as analytic, and as the basis for natural logicism. *Philosophia Mathematica*, 22(3):321–344.

[Tennant, 2017] Tennant, N. (2017). *Core logic*. Oxford University Press.

[Terui, 2004] Terui, K. (2004). Light affine set theory: a naive set theory of polynomial time. *Studia Logica*, 77(1):9–40.

[Thomas, 2013] Thomas, M. (2013). Recapturing ZFC in naive set theory via shrieking. Unpublished manuscript, available at `https://sites.google.com/a/uconn.edu/morgan-thomas/home`.

[Thomas, 2014] Thomas, M. (2014). Expressive limitations of naïve set theory in LP and minimally inconsistent LP. *The Review of Symbolic Logic*, 7(2):341–350.

[Turing, 1936] Turing, A. M. (1936). On computable numbers, with an application to the entscheidungsproblem. *J. of Math*, 58(5):345–363.

[Urquhart, 1984] Urquhart, A. (1984). The undecidability of entailment and relevant implication. *The Journal of Symbolic Logic*, 49(4):1059–1073.

[Van Bendegem, 1994] Van Bendegem, J. P. (1994). Strict finitism as a viable alternative in the foundations of mathematics. *Logique et Analyse*, 37(145):23–40.

[Van Bendegem, 2000] Van Bendegem, J. P. (2000). Alternative mathematics: The vague way. *Synthese*, 125(1):19–31.

[Van Bendegem, 2002] Van Bendegem, J. P. (2002). Inconsistencies in the history of mathematics: The case of infinitesimals. In *Inconsistency in Science*, pages 43–57. Springer.

[Van Bendegem, 2005] Van Bendegem, J. P. (2005). Can there be an alternative mathematics, really? In *Activity and Sign*, pages 349–359. Springer.

[Van Bendegem, 2016] Van Bendegem, J. P. (2016). Contingency in mathematics: two case studies. In *Science as it could have been: discussing the contingency/inevitability problem*, pages 223–239. University of Pittsburgh Press.

[Van Heijenoort, 1967] Van Heijenoort, J. (1967). *From Frege to Gödel: a source book in mathematical logic, 1879-1931*. Harvard University Press.

[van Stigt, 1990] van Stigt, W. P. (1990). *Brouwer's intuitionism*. North-Holland Amsterdam.

[Verdée, 2012] Verdée, P. (2012). Strong, universal and provably non-trivial set theory by means of adaptive logic. *Logic Journal of IGPL*, 21(1):108–125.

[Verdée, 2013] Verdée, P. (2013). Non-monotonic set theory as a pragmatic foundation of mathematics. *Foundations of science*, 18(4):655–680.

[Vermeir, 1999] Vermeir, T. (1999). Inconsistency-adaptive arithmetic. *Logique et analyse*, 42(167/168):221–241.

[Vickers, 2013] Vickers, P. (2013). *Understanding inconsistent science*. OUP Oxford.

[von Neumann, 1925] von Neumann, J. (1925). An axiomatization of set theory. In *From Frege to Gödel: a source book in mathematical logic, 1879-1931*, pages 393–413. Harvard University Press.

[Wagner, 2017] Wagner, R. (2017). *Making and breaking mathematical sense: Histories and philosophies of mathematical practice*. Princeton University Press.

[Weber, 2010a] Weber, Z. (2010a). Extensionality and restriction in naive set theory. *Studia Logica*, 94(1):87–104.

[Weber, 2010b] Weber, Z. (2010b). Transfinite numbers in paraconsistent set theory. *The Review of Symbolic Logic*, 3(1):71–92.

[Weber, 2012] Weber, Z. (2012). Transfinite cardinals in paraconsistent set theory. *The Review of Symbolic Logic*, 5(2):269–293.

[Weber, 2021a] Weber, Z. (2021a). *Paradoxes and inconsistent mathematics*. Cambridge University Press.

[Weber, 2021b] Weber, Z. (2021b). Relevant arithmetic and mathematical pluralism. *The Australasian Journal of Logic*, 18(5):569–596.

[Weber, 2022] Weber, Z. (2022). *Paraconsistency in mathematics*. Cambridge University Press.

[Weber and Cotnoir, 2015] Weber, Z. and Cotnoir, A. J. (2015). Inconsistent boundaries. *Synthese*, 192(5):1267–1294.

[Weir, 2004] Weir, A. (2004). There are no true contradictions. In *The Law of Non-Contradiction: New Philosophical Essays*, pages 385–417. Oxford University Press.

[Weir, 2015] Weir, A. (2015). A robust non-transitive logic. *Topoi*, 34(1):99–107.

[Whittle, 2005] Whittle, S. (2005). Gender fucking or fucking gender? In *Queer theory*, pages 115–129. Catherine Belsey. New York: Palgrave Macmillan.

[Wright, 1975] Wright, C. (1975). On the coherence of vague predicates. *Synthese*, 30(3):325–365.

[Wright, 1997] Wright, C. (1997). On the philosophical significance of Frege's theorem. In *Language, Thought, and Logic: Essays in Honour of Michael Dummett*, pages 201–244. Oxford University Press.

[Yaacov et al., 2008] Yaacov, I. B., Berenstein, A., Henson, C. W., and Usvyatsov, A. (2008). Model theory for metric structures. *London Mathematical Society Lecture Note Series*, 350:315.

[Yeh and Rubel, 2020] Yeh, C. and Rubel, L. (2020). Queering mathematics: Disrupting binary oppositions in mathematics pre-service teacher education. In *Borders in mathematics pre-service teacher education*, pages 227–243. Springer.

[Zamansky, 2019] Zamansky, A. (2019). On recent applications of paraconsistent logic: an exploratory literature review. *Journal of Applied Non-Classical Logics*, 29(4):382–391.

[Zermelo, 1908] Zermelo, E. (1908). Investigations in the foundations of set theory I. In *From Frege to Gödel: a source book in mathematical logic, 1879-1931*, pages 199–215. Harvard University Press.

[Zermelo, 1930] Zermelo, E. (1930). On boundary numbers and domains of sets: New investigations in the foundations of set theory. *Ernst Zermelo. Collected works*, pages 401–430.

Curriculum Vitae

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Current position

PhD student at the Institut für Philosophie I, Ruhr-Universität Bochum (1.09.2019-today)

Research interests

Paraconsistent logics and inconsistent mathematics; feminist logic and epistemology; philosophies of mathematical practice; topological and descriptive set-theoretic perspectives on model theory

Education

2018-2019 Logic Year at the ILLC, University of Amsterdam
2017 Master's Degree in Mathematics at the University of Turin with the thesis *The isomorphism relation of classifiable theories*, final mark 110/110 cum laude
2015 Bachelor's Degree in Mathematics at the University of Turin

Awards

2018 "Premio AILA 3+2" for outstanding Master's theses in Mathematical Logic

Papers

2022 "Consistent theories in inconsistent logics" (w/ A. Tedder), *Journal of Philosophical Logic*, published online.
"The liberation argument for inconsistent mathematics", *Australasian Journal of Logic*, forthcoming.
2020 "A descriptive Main Gap theorem" (w/ L. Motto Ros), *Journal of Mathematical Logic*, Vol. 21, No. 01, 2050025 (2021).

Talks

2023 *The role of logic in epistemic injustice*, Colloquium “Logic and Epistemology", Bochum, 20.04

Queer incomaths: inconsistent mathematics as a critical maths kind, Perspectives on Logic and Philosophy, Bochum, 10.01

2022 *Consistent theories in inconsistent logics*, New Directions in Relevant Logic, online, 10.11

Rethinking inconsistent mathematics, Australasian Association for Logic Conference 2022, online, 22.06

A Plumwoodian argument for inconsistent mathematics, 5th SILFS Postgraduate Conference on Logic and Philosophy of Science, University of Milano-Bicocca, 16.06

2021 *Matematica inconsistente*, Welcome Home 2021, University of Turin, 21.12

2017 *The isomorphism relation of classifiable shallow theories*, XXVI meeting of the Italian Association for Logic and its Applications (AILA), Padova, 26.9

Organized events

2023 *Perspectives on Logic and Philosophy*, Bochum, 09-11.01

2021 *Non-classical Modal and Predicate Logics*, 3rd edition (member of the Organizing Committee), Bochum, 23-26.11

Teaching

2022 *Philosophy of Alternative Mathematics* (w/ F. De Martin Polo), Ruhr-Universität Bochum

2020-2021 *Introduction to Model Theory I, II, III* (teaching help), Ruhr-Universität Bochum

Service to the profession

Editor for the Logic section of the Diversity Reading List in philosophy (2023-today)

Referee for:
Annals of Pure and Applied Logic
Australasian Journal of Logic (x2)
New Directions in Relevant Logics, eds. Standefer et al, Springer