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THE OBJECTIVITY OF MATHEMATICS

ABSTRACT. The purpose of this paper is to apply Crispin Wright's criteria and various axes of objectivity to mathematics. I test the criteria and the objectivity of mathematics against each other. Along the way, various issues concerning general logic and epistemology are encountered.

The account of objectivity in Crispin Wright's *Truth and objectivity* (1992) is more comprehensive than any other that I know of, providing a wealth of detailed insight into the underlying concepts. As Wright sees things, objectivity is not a univocal notion. There are different notions or axes of objectivity, and a given chunk of discourse can exhibit some of these and not others. The aim of this paper is to test mathematics against Wright's various criteria of objectivity. The terrain is interesting, and a number of important issues and debates are engaged along the way.

There are different ways to look at the project. If Wright's criteria are plausible, then we are examining the extent to which mathematics is objective. But of course the antecedent of this conditional is up for discussion, and every conditional has a contrapositive. Some philosophers and mathematicians have it that mathematics is not objective. The early intuitionists, L. E. J. Brouwer and Arend Heyting, for example, took mathematical objects, and mathematical truth, to be mind-dependent. Brouwer, who is always difficult to interpret, articulated a Kantian view that mathematics is tied to the pure intuition of time. Although he held, or seemed to hold, that mathematics is the same for every possible mind, it is crucial to his intuitionism that mathematics is not independent of the mind in general.[1] So for Brouwer, mathematics is not objective, in at least some sense of the term. Heyting is quite explicit about the non-objectivity of mathematics:

... we do not attribute an existence independent of our thought, i.e., a transcendental existence, to the integers or to any other mathematical objects ... mathematical objects are by their very nature dependent on human thought.

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Their existence is guaranteed only insofar as they can be determined by thought. They have properties only insofar as these can be discerned in them by thought ... (Heyting 1931, pp. 52–53)

... Brouwer's program ... consisted in the investigation of mental mathematical construction as such, ... a mathematical theorem expresses ... the success of a certain construction ... In the study of mental mathematical constructions, "to exist" must be synonymous with "to be constructed" ... In fact, mathematics, from the intuitionist point of view, is a study of certain functions of the human mind. (Heyting 1956, pp. 1,8,10)

If there are any contemporary non-cognitivists, or projectivists, about mathematics, presumably they would also hold that mathematics is not objective. From perspectives like these, the aim of the present paper is to see how well Wright's criteria for objectivity fare on what is supposed to be a clear non-objective discourse.

For what it is worth, I confess to the opposite intuition that mathematics is objective. For me, proving a theorem, or exploring the nature of a given mathematical structure, feels more like discovery than invention, more like learning a fact than manifesting a non-cognitive stance. I realize, of course, that this intuition may be little more than a prejudice, subject to correction in light of philosophical theorizing (or incredulous stares) and further articulation of the notion of objectivity. And I do not expect readers to share this attitude. In this paper, the intuition, the intuitive notion of objectivity, and Wright's criteria are all tested against each other.

In rough terms, my conclusion is that mathematics comes out objective on every one of Wrights's tests. In less rough terms there are a few limited areas where, from a certain perspective on controversial philosophical side issues, one can maintain that some highly foundational mathematical matters fail some of Wright's tests for objectivity. Those potential exceptions aside, someone attracted to a non-objective account of mathematics needs to find fault with Wright's criteria, or with the present application thereof.

On the other hand, the potential divergence from Wright's criteria go to the heart of the matter concerning the notion of objectivity, as independence from human judgement. We need a motivated method for deciding whether we have counterexamples to Wright's criteria, or whether the discourse in question is in fact non-objective, at least in part, or both. In general, science and mathematics are human activities, and seem to embody, at least in part, human criteria of success. To what extent does that undermine objectivity? In some cases, it will prove hard to be definitive, since delicate issues in epistemology are broached, but I hope to shed some light on the troublesome notions.[2]

According to Wright, the first hurdle for a non-objective account of any discourse is the centerpiece of Michael Dummett's anti-realism, the principle of epistemic constraint (EC). Relatively early in the book, the principle is schematized thus:

If P is true, then evidence is available that it is so. (p. 41)

A bit later, (EC) is reformulated as a biconditional:

$P \leftrightarrow P$ may be known. (p. 75)

If we understand the notion of "evidence" in the first version as something like "conclusive evidence, sufficient for knowledge", and if only truths are knowable, then the two formulations come to the same thing. In any case, differences between the two formulations will not matter here.

It seems to follow from the very meaning of the word "objective" that if epistemic constraint fails for a given area of discourse – if there are true propositions in that area whose truth cannot become known – then that discourse can only have a realist, objective interpretation. Early on, Wright writes:

> To conceive that our understanding of statements in a certain discourse is fixed ... by assigning them conditions of potentially evidence-transcendent truth is to grant that, if the world co-operates, the truth or falsity of any such statement may be settled beyond our ken. So ... we are forced to recognise a distinction between the kind of state of affairs which makes such a statement acceptable, in light of whatever standards inform our practice of the discourse to which it belongs, and what makes it actually true. The truth of such a statement is bestowed on it independently of any standard we do or can apply ... Realism in Dummett's sense is thus one way of laying the essential groundwork for the idea that our thought aspires to reflect a reality whose character is entirely independent of us and our cognitive operations. (p. 4)

Wright points out, however, that the converse can fail: epistemic constraint is not sufficient for the failure of objectivity. Even if a given discourse is epistemically constrained, it is consistent to

> ... retain the idea that [the] discourse is representational, and answers to states of affairs which, on at least some proper understandings of the term, are independent of us. For example, in shifting to a broadly intuitionistic conception of, say, number theory, we do not immediately foreclose on the idea that the series of natural numbers constitutes a real object of mathematical investigation, which it is harmless and correct to think of the theoretician as exploring. (p. 5)

When epistemic constraint does hold, then Wright's other criteria – width of cosmological role, the Euthyphro contrast, and cognitive command – come into play. Here lies the multi-dimensional notion of objectivity.

The dialectic of this paper is similar to that of an attorney defending a client against a charge of returning a borrowed item broken. The lawyer declares that his client never borrowed the item, and even if he did, the item was returned in good working order. And if the item was broken when returned, it was broken when borrowed. I argue, first, that mathematics is not epistemically constrained: there are unknowable mathematical truths. But this is a delicate matter concerning what is meant by "knowable". Can we idealize, and, if so, how much? Depending on how some empirical and conceptual matters turn out, there may be a sense in which one can coherently maintain – at some cost – that at least a certain class of mathematical truths are all knowable. So I will concede epistemic constraint, properly construed, for the sake of argument, in order to consider Wright's other criteria for objectivity. This done, we will see that mathematics passes every test – up to some highly articulated areas on the fringes. Mathematics is not response-dependent, or judgement-dependent; it exhibits cognitive command; and it has a remarkably wide cosmological role.

As noted above, however, the highly articulated areas on the fringes are of central importance to epistemology, the notion of objectivity, and to the plausibility of Wright's criteria. This is where things get interesting. Let's get to details.

1. EPISTEMIC CONSTRAINT

Plausibly, the standard of knowledge in mathematics is *proof*. One cannot claim to know a proposition (or sentence) P unless she has a proof of P, or at least good grounds for believing that such a proof exists. That's how the enterprise works. So, as a first shot, the principle of epistemic constraint is, or entails, that all mathematical truths are provable. Concerning epistemology, of course, this cannot be the whole story, since one cannot prove everything. Proofs have to start somewhere. We have to deal with axioms. How are those known?

The status of axioms will occupy us in Section 3 below. For now, we ask if there are, or can be, unprovable non-axiomatic mathematical truths. This depends on what one means by "knowable" or "provable". Who is doing the knowing, and the proving? What processes and criteria are they to use? What *is* a proof?

There is a sense in which it is all but obvious that there are unknowable mathematical truths – that epistemic constraint fails badly. Let p and q be prime numbers, each of which is greater than $10^{100,000}$. If their product $n = pq$ were written in standard arabic, decimal notation, it would have at least 200,000 digits. Consider the proposition, or sentence, P that n has exactly two prime factors, each of which is at least $10^{100,000}$ – where n is written in decimal notation. By hypothesis, P is true. Yet it is extremely unlikely that any human being can know P. That is, no one can have compelling evidence for P. Indeed, it is unlikely that anyone can even *understand* P. To be sure, there is a simple algorithm that will result in a verification of P. A bright ten-year-old can grasp the algorithm, and can even get started executing it, given the first few digits of n. But no one can complete the procedure in his or her lifetime, or the lifetime of humanity generally, before the sun goes cold. This is so even if the person is allowed to use a computer, or a bank of computers – given the limits imposed by physics concerning transmission of information at the speed of light and the stability of the very small.

An opponent might retort that there is no such proposition, or sentence, as P. The quick rejoinder to this is that existence of P is a theorem. The proposition that there is no largest prime number is an easy consequence of even intuitionistic arithmetic. So it is a theorem of intuitionistic arithmetic that there are prime numbers greater than $10^{100,000}$. It follows that there is a proposition like P, although, of course, we cannot write down a sentence expressing it in our lifetimes. Conceivably, I did not set the bound high enough, but surely there is *some* limit to the size of numbers that humans can grasp and work with. We are finite creatures, and even the universe seems to be finite. If so, then there are prime numbers that cannot be represented even using all available material in the universe.[3]

So our current opponent must reject even intuitionistic arithmetic. To be sure, one can deny the existence of a proposition (or sentence) like P by denying that there are such large numbers. This is to adopt a strict finitism, holding that there are only finitely many natural numbers. Even this is not enough to secure a (strict) version of epistemic constraint. It is more or less feasible to come up with prime numbers with, say, 150 digits, but it is not feasible, so far as we know, to factor numbers with 300 digits. The techniques for encrypting Internet transactions depend on this. Suppose that we randomly write down a 300 digit number which, as it happens, has exactly two prime factors of over 100 digits each. It is plausible, almost certain, that no one can know that this number has exactly two prime factors, or what its prime factorization is. To maintain epistemic constraint, our opponent must claim that there simply is no fact concerning the prime factors of this number.[4]

In short, one can maintain epistemic constraint for simple arithmetic only by imposing severe restrictions on what is knowable, restrictions that cut much deeper than intuitionism. A finitism this strict would cripple mathematics as we know it. I have no interest in engaging that opponent here, with apologies to any readers inclined that way. I want to know if *mathematics*, as we more or less know it now, is objective.

Of course, the examples used here are not particularly interesting. No mathematician is likely to find the propositions gripping, and wonder what their truth values may be. Who cares whether that particular number is prime? It seems to me that what counts as *interesting* mathematics is clearly a human matter, and is not objective. Isn't it a matter of what *we* happen to find interesting? In any case, if there is something like an objective notion of interestingness, or interest-worthiness, I am not particularly concerned with it here. The same issue could be raised for any other discipline. When we wonder if, say, physics is objective, we do not focus on statements that human physicists happen to find interesting. Rather, we focus on what the discipline is studying – the supposedly external micro-physical universe – and how the practitioners go about studying this subject matter, their standards for correct assertion, and the like. It is close to a truism that any conscious, voluntary human endeavor is guided, in part, by what we humans find interesting and important. If we cannot separate out and ignore such factors, then there is little objectivity anywhere.

The standard response to considerations of feasibility is to claim that they invoke too narrow a notion of knowability. It is a hallmark of all varieties of intuitionism and constructivism that all mathematical truths are knowable. As noted, the original exponents, Brouwer and Heyting, hold that the essence of mathematics is mental construction. So it is impossible for there to be truths that are not accessible to construction, and thus unknowable. Brouwer (1948, pp. 90) wrote:

> The … point of view that there are no non-experienced truths … has found acceptance with regard to mathematics much later than with regard to practical life and to science. Mathematics rigorously treated from this point of view, including deducing theorems exclusively by means of introspective construction, is called intuitionistic mathematics …

Something similar holds for Erret Bishop's (1967) constructivism. Dummett's intuitionism is grounded in his semantic anti-realism. He argues, on broadly linguistic grounds, that all truths are knowable (see also Tennant 1987, 1997). These intuitionists are well-aware of the considerations of feasibility.

From the side of classical mathematics, and staunch realism, Kurt Gödel held, on rationalistic grounds, that every unambiguous mathematical proposition is either provable or refutable (Wang (1974, pp. 324–325, see also Wang 1987):

> … human reason is [not] utterly irrational by asking questions it cannot answer, while asserting emphatically that only reason can answer them … [T]hose parts of mathematics which have been systematically and completely developed … show an amazing degree of beauty and perfection. In those fields, by entirely unsuspected laws and procedures … means are provided not only for solving all relevant problems, but also solving them in a most beautiful and perfectly feasible manner. This fact seems to justify what may be called "rationalistic optimism".

Tennant (1997, pp. 166) dubs this view "Gödelian optimism". The opening of Hilbert's celebrated "Mathematical problems" lecture (1900) is an enthusiastic endorsement of optimism (see McCarty 2005):

> However unapproachable these problems may seem to us and however helpless we stand before them, we have, nevertheless, the firm conviction that the solution must follow by … logical processes … This conviction of the solvability of every mathematical problem is a powerful incentive to the worker. We hear the perpetual call: There is the problem. Seek its solution. You can find it … for in mathematics there is no ignorabimus.

Like the intuitionist, our optimist is surely aware of the above, rather obvious considerations concerning feasibility. When the intuitionist and the optimist declare that all mathematical truths are knowable, they do not mean knowable-in-our-lifetime, or knowable-using-available-resources. But what do they mean? The modal suffix "able", in "knowable", invokes idealizations on normal human abilities. For a start, these include the idealizations involved in the mathematical notion of computability. It is assumed that the knowing mathematician has unlimited time, materials, memory, and attention span. So, for example, if it is (actually) known that a mathematical proposition turns on the result of an effective calculation, then, for the optimist and the intuitionist alike, either the truth of the proposition is knowable or the falsity of the proposition is knowable. For example, for any natural number $n > 1$, either it is knowable that n is prime or it is knowable that n is composite. The proposition P broached above is also knowable (as is one of either Q or $\neg Q$ in note 4 above).

In developing his semantic anti-realism, Tennant (1997, Chapter 5) makes a detailed defense of the idealizations noted so far. To summarize,

> ... one would want to say that the primality of any given number is surely knowable, in the sense of "knowable" that we are concerned here to explicate. For no conceptual leap is involved when we try to conceive what kind of fact it would be that ... a huge number is prime. It is just that it would take much longer to establish it as a fact, that is all ... One does not have to imagine any essential change to the human cognitive repertoire, or new modes of sensory access to the external world, or telepathic ability, or anything incongruous or out of keeping with our current cognitive apparatus. We are equipped, right now, to perform tasks such as applying Eratosthenes' sieve. The sheer size of the number whose primality is in question is neither here nor there when it comes to our ability to conceive the kind of fact that it is (or would be) for some gargantuan number N ... to be prime. (p. 145)
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> ... the actual limits to effective human thought ... that we are thinking of ourselves as transcending here are not limits to the kind of thinking we may do, but only limits on how much of that kind of thinking one could do. The thinking is all of one uniform kind. (p. 147)

As Tennant notes, Wittgensteinian considerations of rule-following come to the fore at this point. For an anti-realist (or optimistic) account to be completely satisfactory, its advocate should say something about how given rules *determine* an outcome. I propose to put such issues aside here, and just take for granted the notion of effective decidability, assuming that it is determinate. This gives truth values to the simple propositions in question, but does not guarantee that the discourse passes the letter of epistemic constraint, as that notion bears on objectivity. It depends on the nature – and objectivity – of this determinacy.

In any case, the idealizations involved in the intuitionistic and optimistic assertion of epistemic constraint go well beyond the usual idealizations of computability, well beyond what is needed for the determinacy of effective operations. Tarski's theorem, which is acceptable to the intuitionist, is that arithmetic truth is not arithmetically definable.[5] A fortiori, that there is no Turing machine that produces all and only the truths of arithmetic. According to epistemic constraint, then, there is no Turing machine that produces all and only the knowable truths of arithmetic. So, up to Church's thesis, the human ability to know all truths does not consist in following a single, prescribed algorithm, or doing deductions in a single formal deductive system. The full assertion of epistemic constraint is not a simple matter of "more of the same", to use Tennant's phrase, since there is no uniform procedure to extrapolate.

Dummett (1963) points out that arithmetic truth is what he calls "indefinitely extensible". Formally, for any effective delineation of arithmetic truth, there is a truth that is beyond the delineation. The same goes for any *arithmetic* delineation of arithmetic truths. Fix an effective method of Gödel numbering, and suppose that $\mathbf{\Phi}(x)$ is a formula in the language of arithmetic such that for each natural number n, if $\mathbf{\Phi}(n)$ then the sentence coded by n is true. Let $\mathbf{\Psi}$ be a fixed-point for $\neg\mathbf{\Phi}(x)$ so that $\mathbf{\Psi} \equiv \neg\mathbf{\Phi}(\ulcorner\mathbf{\Psi}\urcorner)$ is provable in elementary arithmetic. Assume $\mathbf{\Phi}(\ulcorner\mathbf{\Psi}\urcorner)$. Then, by hypothesis $\mathbf{\Psi}$ is true, and thus so is $\neg\mathbf{\Phi}(\ulcorner\mathbf{\Psi}\urcorner)$. This is a contradiction. So $\neg\mathbf{\Phi}(\ulcorner\mathbf{\Psi}\urcorner)$, and thus $\mathbf{\Psi}$. So under the hypothesis that all of the $\mathbf{\Phi}$'s are true, there is a true sentence that is not in the extension of $\mathbf{\Phi}$. So the $\mathbf{\Phi}$s do not exhaust the truths.

This, of course, is not an objection to either optimism or intuitionism. Gödel (1951, p. 310) was quite explicit that his view is inconsistent with the thesis that the knowable propositions can be codified by a mechanical procedure:

> … the following disjunctive conclusion is inevitable: Either mathematics is incompletable in [the] sense that its evident axioms can never be comprised in a finite rule, that is to say, the human mind (even within the realm of pure mathematics) infinitely surpasses the powers of any finite machine, or else there exist absolutely unsolvable … problems …

In other words, either the mechanistic thesis is false, or else optimism is false, and there are unknowable propositions of arithmetic, even granting the idealization of "knowable". The outspoken physicist Roger Penrose (1996, Section 4.2) follows suit, adopting optimism over mechanism:

> I had vaguely heard of Gödel's theorem prior [to my first year of graduate school], and had been a little unsettled by my impressions of it … I had been disturbed by the possibility that there might be true mathematical propositions that were in principle inaccessible to human reason. Upon learning the true form

of Gödel's theorem … I was enormously gratified to hear that it asserted no such thing; for it established, instead, that the powers of human reason could not be limited to any accepted preassigned system of formalized rules.

Is the human mind really that powerful? In a discussion of Gödel's optimism, George Boolos (1995) asks why "should there *not* be mathematical truths that cannot be given any proof that human minds can comprehend?" That is the point of contention here. The issue of optimism is not exactly an empirical matter, since we are not dealing with actual human minds, as above. It is hard to see how any empirical finding – a statistical study, for example – could be even relevant to the issue.

One might well wonder what is involved in idealizing the human mind to the extent that every mathematical truth is knowable. Of course, one can always envision or postulate the existence of some mind-like entities that somehow know every truth of (say) elementary arithmetic. Thought experiments like this are easy. But one can wonder what this has to do with what *humans* can know, and thus with Wright's criterion of epistemic constraint.

So much for optimism. Intuitionists, of all stripes, also hold that the powers of the human mind outstrip any Turing machine or effective deductive system. They are unanimous in rejecting any sort of formalism. By "knowable", they do not mean "derivable in a fixed formal system". But what do they mean? The intuitionist rules out unknowable truths on a priori, conceptual grounds concerning the nature of mathematical objects or the nature of truth. For the Dummettian anti-realist, truth itself has an epistemic component. If there can be no proof, then there can be no truth either. So one might think that intuitionism itself has no consequences concerning the powers or the limitations of the human mind. Of course, intuitionism does demand serious revisions to mathematics itself (see Section 3 below), although not quite as serious of those demanded by the strict finitist. It seems that the more human the idealizations are, the less mathematics is subject to epistemic constraint.[6]

Since, as noted above, the Tarskian results concerning the extreme complexity of arithmetic truth are acceptable to the intuitionist, she surely owes us *some* account of the extensive idealizations involved in the notion, showing how there is some natural extension of human abilities that delivers all and only the truths of mathematics. And, again, how are these highly idealized abilities related to what real life humans can and cannot come to know?

Burden of proof issues are notoriously intractable. I suggest that in this case, the burden is on the optimist and intuitionist, to articulate the idealizations in a way that does not beg any questions and makes it plausible that all truths are knowable in some suitably idealized sense. Let me just register skepticism toward this project.

Returning more directly to our main theme, it is not clear how the highly idealized notions of knowability bear on objectivity. Notice that when objectivity is broached for other areas of discourse, and a philosopher decides that all truths in that discourse are knowable, he often does not invoke idealizations at all. Someone who asserts that there are no unknowable facts about color, or no unknowable facts about what is funny, is speaking of the abilities of ordinary, flesh and blood human beings. To be sure, the discussion often invokes a notion of "standard conditions" under which judgements are to be made. In the case of color, for example, one sometimes speaks of normal lighting conditions. But we are speaking here of the conditions of judgement, not the state of the judge. We envision statistically ordinary human beings making judgements under ideal circumstances. Moreover, the invoked conditions are ones that real life judges sometimes find themselves in, at least approximately. We can reasonably speculate about what we would judge in counterfactual conditions, provided that the conditions are not *too* counterfactual.

In other cases, discussions of objectivity do invoke some idealization involved in the judge herself. Consider, for example, a view that an action is morally right if it would be judged so by someone disinterested and free from prejudice.[7] It is plausible that the proper conditions never fully arise, and perhaps never could arise. Assume, for example, that there is a deep psychological barrier to becoming completely free of prejudice. Along similar lines, Peter Menzies (1998) proposes a judgement-dependent account of modality. The idea is that a sentence or situation is possible just in case it is conceivable to someone who does not suffer from any recognized limitations, given our practices of correction. It is an idealization since humans typically do suffer from some relevant limitations. Perhaps we have to suffer such limitations.

Even so, on accounts like these, the idealizations can reasonably be *approximated* by (some) real human beings. On the ethical view in question, we make fallible moral judgements by trying to free ourselves of bias or limitation, or by speculating on how we would judge if free from bias. Concerning modality, Menzies (1998, p. 272) writes:

It is indeed true that we can never be certain that we are in ideal conditions: no matter how hard we try to overcome our cognitive limitations, we can never be certain that we have succeeded. All the same, in many cases we can be reasonably confident that we are in conditions that are close to ideal, or close enough for the purposes at hand. For example, suppose that you carry out a simple thought experiment in which you suppose that you pursued a different career; and on the basis of this thought experiment, you arrive at the conclusion that it is possible that you pursued a different career. You can be reasonably confident of your modal conclusion in a case like this, because you can be reasonably confident that you do not have any of the limitations that would discredit a claim to have successfully conceived this situation.

With even the most elementary parts of arithmetic, the idealizations needed to maintain that all truths are knowable cannot even be approached, or approximated, by real people. The idealizations go beyond anything invoked in the other test cases. Even on the most basic level, before we worry about connectives and quantifiers, there are infinitely many true equations made up of numerals and the signs for the basic functions. We can know only a small finite number of those. We cannot approximate, or approach direct knowledge of very many more.

Nevertheless, I hereby concede epistemic constraint for the sake of argument, so that we can invoke Wright's other criteria for objectivity. For effectively decidable propositions, the idealizations we need are those invoked in the mathematical treatment of computability, provability, and the like. But I insist on those idealizations, at least. It is a dilemma. If we do not idealize sufficiently, then epistemic constraint obviously fails, in which case mathematics comes out objective and Wright's other criteria do not apply. We can stop here. If we are to invoke the other criteria at all, then we must apply them to idealized human knowers, and not to flesh and blood specimens. I will have more to say on the required idealizations as we go, especially in Section 4, on response-dependence. To repeat, if we are talking about real mathematics, as practiced by human beings proving and publishing theorems, then epistemic constraint fails, and the subject is objective.

2. COSMOLOGICAL ROLE

Wright (1992, p. 196) defines a discourse to have "*wide cosmological role* … just in case mention of the states of affairs of which it consists can feature in at least some kinds of explanation of contingencies which are not of that sort – explanations whose possibility is not guaranteed merely by the minimal truth aptitude of the associated discourse" (p. 198). The width of cosmological role is "measured by the extent to which citing the kinds of states of affairs with which [the discourse] deals is potentially contributive to the explanation of things *other than*, or *other than via*, our being in attitudinal states which take such states of affairs as object" (p. 196). Wright argues that cosmological role is what underlies (or should underlie) arguments for objectivity in terms of best explanation.

The idea here is that a discourse is apt for a realist construal – on this axis – if statements in that discourse figure in explanations provided within a wide range of discourses, including those well beyond the discourse in question. To take a mundane example, Wright points out that the wetness of some rocks can explain "my perceiving, and hence believing, that the rocks are wet", "a small ... child's interest in his hands after he has touched the rocks", "my slipping and falling", and "the abundance of lichen growing on them" (Wright 1992, p. 197). So statements about rock wetness figure in explanations concerning perception, belief, the interests of children, the human abilities to negotiate terrain, and lichen growth.

Statements about rock wetness thus have wide cosmological role and, of course, it is most natural to regard that discourse as objective. Wet rocks are not wet because we perceive them or judge them to be wet. By way of contrast, Wright argues that moral discourse fails to have wide cosmological role: moral "states of affairs" do not figure in explanations of non-moral matters.[8]

Despite the word "cosmological" in the title of the constraint, Wright is quick to add that the explanations involved do not have to be causal. Otherwise, only science-like discourses that traffic in causality would have a chance of passing the test. If we take Wright's formulation of the constraint at face value, this one looks like a no-brainer. Mathematics figures in explanations of all sorts of phenomena throughout the sciences and everyday discourse. Wright (1992, pp. 198–199) himself provides one such example:

> ... it is notable ... that the citation of mathematical facts does contribute, seemingly, to other kinds of explanation than those which are of or via propositional attitudes. (It is because a prime number of tiles have been delivered, for instance, that a contractor has trouble using them to cover, without remainder, a rectangular bathroom floor, even if he has never heard of prime numbers and never thought about how the area of a rectangle is determined.)

As far as I know, the best explanation of why rain forms into drops begins with an account of surface tension, and then adds the mathematical fact that a sphere is the largest volume contained by a given surface area. A high school physics or chemistry text provides hundreds of further examples.[9]

So mathematics does figure in explanations well beyond mathematics itself, and, as Wright notes, in most of the cases, we cannot simply invoke propositional attitudes concerning mathematical propositions instead of the propositions themselves. Mathematics passes the letter of wide cosmological role, with flying colors. The opponent of objectivity might complain that the criterion of cosmological role was not formulated properly. It is not a matter of whether mathematics in fact figures in explanations of non-mathematical phenomena, but whether mathematics *must* figure in such explanations. In analogy with deflationism about truth, our opponent might claim that mathematics is dispensable in explanations: any relevant explanation that invokes mathematics can be reformulated so that it does not invoke mathematics.

This rejoinder is hostage to the success of a program, like that of Hartry Field (1980), for formulating nominalistic versions of all successful scientific theories, and then showing that mathematics is conservative over such theories. If such a program could be worked out, then the explanations in question could indeed be reformulated in nominalistic terms, and would not invoke any mathematics. But at this point, the success of the nominalistic program is, at most, a promissary note.

One might attempt to eliminate mathematics from explanations of physical phenomena by formulating categorical second-order characterizations of the relevant mathematical structures, and then invoking the logical consequences of these characterizations. For example, instead of speaking about the surface area of spheres, one speaks of the relevant semantic, model-theoretic consequences of the axioms of analytic geometry. One has to add a non-logical assumption that the universe is big enough to model the structure in question (the continuum in this case), but the rest of the "explanation" takes place within the model theory of second-order logic.[10]

My view, for what it is worth, is that this move does not eliminate the mathematics from the explanation, since mathematics is bound up with second-order logical consequence (see Shapiro 1991). In addition, a logicist will not be bothered, or surprised, by this possibility. It shows that (second-order) logic itself has wide cosmological role. Since, for the logicist, mathematics is part of logic, mathematics also has wide cosmological role, as above. A philosopher who thinks that second-order logic is not mathematics, and that mathematics has some content that goes beyond second-order logic, can coherently maintain that this extra content (whatever it might be) does not have wide cosmological role. The explanation in question turns only on second-order logic and the assumption about the size of the universe. This is one of the less interesting places where an opponent of objectivity (via Wright's criteria) might have a little wriggle room. She can claim that the content of mathematics that goes beyond that of the semantic, model-theoretic second-order consequence relation fails the criterion of cosmological role, as reformulated here so far.

A more serious concern here is that the plausibility of cosmological role as a criterion of objectivity depends on the nature of explanation. What is it to explain something? Here, of course, we broach one of the most fundamental issues in the philosophy of science, general epistemology, and perhaps metaphysics. There is nothing close to a consensus in the extensive literature on explanation, and, unfortunately, it will not do here to leave things at a basic or pre-theoretic level.[11] Intuitively, to explain something is to give a reason for it. Although dictionaries are often poor sources of philosophical insight, we might note that according to *Webster's twentieth century unabridged dictionary*, to explain is to "make plain, clear, or intelligible; to clear of obscurity". Understood this way, explanation is context sensitive. What makes for an explanation – what makes for clarity, intelligibility, and lack of obscurity – depends on the interests, goals, and background assumptions of the person asking for the explanation. What will make the matter intelligible for her? When asked for an explanation of a devastating fire in a building, a chemist might be satisfied to learn that it was caused by the combustion of exposed gasoline. This particular fact does not clear the matter up sufficiently for an arson inspector.

Returning to mathematics, it may be that even if a nominalist program somehow succeeds, the mathematics-free versions of the scientific theories may not explain very much. That is, the replaced theory may not make the non-mathematical explananda "plain, clear, or intelligible" or "clear of obscurity", as Webster puts it. The reason is that the proposed nominalistic "explanation" itself might be too obscure – unplain, unclear, and unintelligible – for anyone to understand it. I presume that one cannot claim that a sentence makes something intelligible if she cannot understand the sentence. Mathematics often finds ways to state its propositions in concise, readily understandable terms, even if those ways amount to little more than codings of non-mathematical matters (see note 9).

This observation questions the accuracy of cosmological role as a criterion of *objectivity*. It is hard to see how the fact – if it is a fact – that human beings sometimes find mathematical explanations clear and intelligible should count in favor of the objectivity of mathematics. What do *our* interests and goals have to do with objectivity? I thought that objectivity is related to what is *independent of* human interests and goals.

One response to this observation would be to reformulate the criterion of cosmological role. Perhaps we can replace explanations for real flesh and blood human beings with explanations-in-principle. In effect, we invoke the aforementioned idealizations needed to get epistemic constraint on the table. What would our idealized counterparts find "plain, clear, or intelligible" and "clear of obscurity"? Given the (apparent) interest-relativity of explanation, I am not sure how feasible this maneuver is. I presume that our interests are tied to the limited kinds of beings were are, and to the finite kind of environment we find ourselves in. What *are* the interests of our highly idealized counterparts? Perhaps things are not completely hopeless on this front. To stick to the idealizations related to effective computability, we envision beings who have the interests and goals that living human beings would have if they did not have the limitations of time, memory, attention span, etc.

On the philosophical front, however, this move does not help much. To repeat, if explanation is in fact tied to *interests*, whether they be ours or those of our idealized counterparts, then we can and should wonder what it has to do with objectivity. To address this matter fully would take us too far afield – well beyond my own competence. I will rest content with a brief account of what is needed to sustain cosmological role as a criterion of objectivity. Then we can see what it would take for mathematics to pass the test.

One option is to argue, or merely assert, that a proposition or an area of discourse is objective – independent of human (or idealized human) judgement and the like – just because it figures in what we human beings (or our idealized counterparts) find clear and intelligible, and free from obscurity. This would be a pre-established harmony, suggesting that our minds are tuned into the ultimate fabric of the universe. If we get it, then the universe must be so, independent of us and our judgements and interests. I do not know how to argue for or against this, but it strikes me as hubris.

A second option is to insist (or posit, or argue) that there is an objective concept of explanation, distinct from (but related to) the interest-relative, psychological notion that involves interests. Similar ideas have a long pedigree in the history of philosophy. The Aristotelian notion of final cause seems related to, perhaps identical with, what may be called objective explanation. The final cause of a substance is its underlying *reason* or purpose. Another precursor was Bernard Bolzano's (1837) ground-consequence relation. Given the relevant background scientific theory, we can infer the temperature in a room from the height of mercury in a thermometer placed there, and vice versa. But the ground-consequence relation is asymmetric. According to Bolzano, the temperature is the ground and the mercury-height the consequence, and not vice versa. The temperature is the reason for the height of the mercury, not the other way around. A third precursor is Frege's (1884, Section 3) notion of the "ultimate ground", or justification of true and knowable propositions. These figure in his accounts of analyticity and a priority:

> The aim of proof is, in fact, not merely to place the truth of a proposition beyond all doubt, but also to afford us insight into the dependence of truths upon one another. After we have convinced ourselves that a boulder is immovable, by trying unsuccessfully to move it, there remains the further question what is it that supports it so securely?

In line with Frege's staunch anti-psychologism, he understood these grounding relations to be objective. It is not a matter of what *we* find compelling, but of how the truths themselves are structured. For further examples, at least some of the extensive literature on explanation is also directed toward objective matters. Consider, for example, accounts that turn on the notion of a law of nature, or a law-like generalization.

If a notion of objective grounding or objective explanation could be made out, then someone might define an area of discourse to have wide cosmological role (in the proper sense) just in case "mention of the states of affairs of which it consists" features in explanations of the proper sort – explanations that give the formal or final cause, or the ground, or the ultimate justification – of matters in a wide variety of discourses. It is easy to see how wide cosmological role, in this sense, is relevant to objectivity. But the burden is to articulate the relevant notion of explanation. And, right now anyway, I do not see how this might go without a prior notion of objectivity on board. I suspect that we have to know what objectivity amounts to before we can articulate the proper notion of ground or objective explanation.

For a different tactic, someone might leave the notion of explanation at an intuitive level, perhaps accepting its psychological underpinning: to explain is to make clear, plain, or intelligible. Nevertheless, the tactic goes, an explanation is not satisfying, or should not be, unless there is *some* objective relationship between the explanation and what it explains. A good explanation needs to engage the non-human world at some level. There is not too much hubris involved in this claim, or at least not as much with others. On such a view, the original formulation of the width of cosmological role is too crude. It is not the case that *everything* that occurs in an explanation is relevant to objectivity, perhaps for reasons given above.

Along what may be similar lines, Steven Yablo (2005) argues that mathematics plays only a representational role in science, and, presumably, in scientific explanations. The idea is that mathematics allows us to state or express features of the non-mathematical world – features that are difficult or perhaps impossible to express otherwise (again, see note 9). Arguably, what is *merely* representational in this sense need not be objective, and is not really explanatorily in the relevant way. For example, just about any conceivable explanation is going to consist of words, but, pre-established harmony aside, it does not follow that words are somehow objectively bound up with the workings of the universe. It does not follow from these observations, however, that other aspects of explanation are explanatorily relevant, and perhaps bear on objectivity.

But what are those aspects? A philosopher who is inclined this way has the burden of articulating which parts of an explanation are explanatorily relevant, in the proper sense, and which are explanatorily superfluous, playing only a representational role. For this account to be helpful here, she must do this articulation without presupposing that we already have a clear account of what objectivity is.

We have broached another very general issue. Since antiquity, philosophers and scientists have been trying to figure out how the world is, in itself, independent of the minds, conventions, representation schemes, etc., of the human knowers. Clearly, our best theories and explanations are due to both the nature of the non-human world and the nature of human knowers. As John Burgess and Gideon Rosen (1997, p. 240) put it, "our theories of life and matter and number are to a significant degree shaped by our character, and in particular by our history and our society and our culture". A perennial issue in philosophy concerns the role of each of these two factors in our theories. Burgess and Rosen ask, "to what extent does the way we are, rather than the way the world … is, shape our mathematical and physical and biological theories of the world?" Among contemporary philosophers, a widely held view, championed by W. V. O. Quine, Hilary Putnam, and Donald Davidson (and Burgess and Rosen), is that there is no way to sharply separate the "human" and the "world" contributions to our theorizing. The same goes for whatever it is that we find clear, intelligible, and free from obscurity. There is no God's eye view of the world to be had. In present terms, this speaks against any hope for *complete* objectivity, at least as far as cosmological role is concerned.

I propose to leave this topic until the various burdens are further discharged. To summarize, the present conclusion is that mathematics easily passes the letter of the criterion of wide cosmological role, but that it is not clear how this relates, one way or the other, to the objectivity of mathematics.

3. COGNITIVE COMMAND

Suppose that the purpose of a given area of discourse is to describe mind-independent features of some mind-independent reality. It follows that if two people disagree about something in that area, then at least one of them has *misrepresented* that reality. In typical cases, one of them has a cognitive shortcoming. Suppose, for example, that two people disagree over whether there is one dog or two running in a given field. Then at least one of the people did not look carefully enough, has faulty eyesight or memory, an obstructed view, etc. On the other hand, if a discourse does not serve to describe a mind-independent realm, then disagreements in that discourse need not involve cognitive shortcoming on the part of either party. To take one of Wright's favorite examples, two people can disagree about what is funny without either of them having any cognitive shortcoming. One of them may have a warped sense of humor, or no sense of humor, but there need be nothing wrong with her *cognitive* faculties.[12] A non-cognitivist about ethics would say the same about moral disagreements.

Wright (1992, p. 92) writes that

> A discourse exhibits Cognitive Command if and only if it is a priori that differences of opinion arising within it can be satisfactorily explained only in terms of "divergent input", that is, the disputants working on the basis of different information (and hence guilty of ignorance or error …), or "unsuitable conditions" (resulting in inattention or distraction and so in inferential error, or oversight of data, and so on), or "malfunction" (for example, prejudicial assessment of data … or dogma, or failings in other categories already listed).

In other words, if cognitive command fails, then (cognitively) blameless disagreement is possible, or at least it cannot be ruled out a priori.

In some areas, the viability of cognitive command as an axis of objectivity depends on some delicate and controversial issues in epistemology.[13] Suppose, for the sake of argument (at least), that ordinary science exhibits something in the neighborhood of Quine's underdetermination of theory by data. So it is possible for two scientists to reach reflective equilibrium, but come to conflicting conclusions, because they made different tradeoffs in the process. Suppose that one of them, William, says P, and the other, Karen, says $\neg P$ – and assume that each attaches the same meaning to P. Assume also that no further available data will knock either of them out of reflective equilibrium, and thus break the tie. In other words, we assume that the overall epistemic situation will not improve, and perhaps cannot improve. Their theories are empirically equivalent, and score as rough equals on overall theory assessment.

On the combination of assumptions in play here, there may be nothing to fault either scientist. Each has followed the proper methodology flawlessly, and so each displays no cognitive shortcoming – or so it seems. They just come to different conclusions using the same, fallible methods. So, if the foregoing, admittedly simplified assumptions are possible, then cognitive command seems to fail for science. Blameless disagreement *is* possible. There are a number of competing philosophical conclusions one might draw in this case, with perhaps no clear winner. There may be conflicting reflective equilibria one level up, concerning the overall philosophical situation.

First, one might conclude that science is not (completely) objective, at least concerning the matter that separates Karen and William.

On this view, there simply is no fact of the matter, independent of the intellectual lives of scientists, whether P or $\neg P$ is true. This is consonant with the conclusion reached at the very end of the previous section, concerning our inability to separate the "human" and the "world" contributions to our theorizing. Perhaps Karen and William come to different judgements concerning the simplicity of a given theory, or that one of them opts for a simpler theory at the expense of some other criterion. How does *that* decision bear on the *objective* truth of the respective theories? Do we know, or believe, that the universe is simple? It seems rather that simplicity sits on the "human" side of the mix.

An extreme version of this first option would be to deny that there is a fact of the matter – objective or otherwise – whether P or $\neg P$. A less extreme version would allow that there is a fact of the matter, but that it is not objective – not independent of the human theoretical situation. One might wax relativist, holding that P is true for William and $\neg P$ is true for Karen.

Either version of this first option flies in the face of a strong intuition that science is objective. There is a physical world, not of our making, and our two scientists, William and Karen, are trying to describe its mind-independent properties. At least one of them is mistaken (perhaps faultlessly). Of course, this intuition is not sacrosanct, and many philosophers have turned to views like instrumentalism and constructive empiricism (not to mention idealism) in response to various considerations, including the underdetermination of theory by data and the role of simplicity in scientific methodology. Our first option does not go nearly that far, proposing only that at times, science is not completely objective.

Still, let us keep an intuitive realism for science on the table, at least for purposes of exploration, and consider some other options. A philosopher might respond to underdetermination by claiming that epistemic constraint fails for science. We just do not know, and in light of the standoff, cannot know which of our two scientists is right. So the truth in question is unknowable. Suppose, for example, that Karen is the one that got things right. In the scenario in question, she just got lucky. If $\neg P$ is in fact unknowable, then, of course, Karen does not know that $\neg P$. No one does. On this combination of views, we can rely on Wright's argument that the criteria of cosmological role, cognitive command, and the Euthyphro contrast apply only if the discourse is epistemically constrained. The failure of cognitive command is simply irrelevant to the matter of objectivity: science is objective just because it is epistemically unconstrained.

There is a third option, which also enjoys some intuitive plausibility. One might think that epistemic constraint has not been ruled out by our scenario of blameless disagreement, even assuming the objectivity of science. Suppose, again, that it is our second scientist, Karen, who is in fact correct: $\neg P$ is true. One might argue that, lucky or not, she *does* know $\neg P$. By hypothesis, $\neg P$ is true. Moreover, Karen believes $\neg P$, and this belief was arrived at by correctly following the best scientific procedure available. And, presumably, this procedure is reliable. What more does it take for (albeit fallible) knowledge?

Of course, things are not straightforward here – are they ever? We have broached delicate matters in epistemology. We have known since at least Gettier that justified, true belief is not sufficient for knowledge, especially in defeasible areas like science. Karen's warrant for $\neg P$ consists of its place in her overall theory, found to be in reflective equilibrium, meeting all relevant scientific criteria. On the assumptions in question, there might be a defeater to her warrant for believing $\neg P$, namely William's having come to the opposite conclusion using the same methodology (with different tradeoffs along the way). If Karen were to become aware of William's overall account, she probably should retract her claim to know that $\neg P$.

Well, even if Karen should not *claim* to know $\neg P$ once she gets to know William's account of things, perhaps she knows $\neg P$ anyway. Maybe the potential defeater – William's theory – does not undermine Karen's justification, on whatever the correct account of justification is. By hypothesis, the counter-evidence is misleading. Or perhaps some sort of reliabilism is correct, and Karen has knowledge of $\neg P$ simply because her belief was generated by a reliable (albeit fallible) mechanism, scientific method itself. For Karen, at least, there are no Gettier-style defeaters. There is, of course, no consensus on the underlying epistemological issues, and I cannot settle the issues here.

To summarize, an advocate of this third option maintains the intuition that science is objective and that epistemic constraint holds for it; and hopes (or argues) that the correct epistemology cooperates with these conclusions. In this case, blameless disagreement is nevertheless possible. So if the third option prevails, then cognitive command is not a reliable indicator of objectivity, contra one of the themes of Wright (1992). Cognitive command fails for one of the paradigms of objectivity.

On yet another hand – we are up to four – maybe we can maintain that cognitive command holds after all. If Karen does know $\neg P$, then perhaps the other scientist in the scenario, William, does exhibit a cognitive shortcoming. He believes an objective falsehood, P, and, by hypothesis, $\neg P$ is knowable, since Karen knows it. What else does it take to exhibit a cognitive shortcoming?

We can, of course, define our terms as it pleases us, but it does not seem appropriate or useful to extend the notion of "cognitive shortcoming" that far. By hypothesis, both scientists acted in the epistemically most responsible manner possible. Neither can be faulted. As noted above, William was simply unlucky. Should this bad luck count as a cognitive shortcoming, as a blameworthy disagreement? If it is not William's fault, then why is he to be blamed, just because he happens to believe something whose negation is knowable?

This fourth orientation is explicitly ruled out by Wright's later work. Shapiro and Taschek (1996) argue on lines similar to those just given on behalf of William and Karen that, in general, epistemic constraint entails cognitive command. In other words, if all truths in an area of discourse are knowable, then there can be no blameless disagreement in that area. This suggests that cognitive command is not really a different axis of objectivity, over and above epistemic constraint. In his detailed reply, Wright (2001) proposed that the quantifiers in the formulation of cognitive command should be understood constructively, via intuitionistic logic. For cognitive command to hold, it is not enough that blameless disagreement be ruled out. By the argument in Shapiro and Taschek (1996), that much happens whenever epistemic constraint holds. The proper criterion for cognitive command is that whenever there is disagreement, then it is possible to *assign blame* to one or the other participant. The view

that cognitive command holds whenever conflict of opinion is possible ... demands, in the presence of [epistemic constraint] that there be an *identifiable* shortcoming in [one of the] conflicting opinions ... So although indeed in position to rule out the suggestion that any disagreement is cognitively blameless ... we remain ... unentitled to the claim that there will be a cognitive shortcoming in any difference of opinion ... We remain so unentitled precisely because that would be a commitment to a *locatability* claim ... The immediate lesson is that it is an error (albeit a natural one) to characterize failures of cognitive command ... in terms of the possibility of blameless differences of opinion ... Failures of cognitive command ... must be viewed as situations where *we have no warrant for* a certain claim, not ones where – for all we know – its negation might be true. We *do* know [that blameless disagreement is not possible]. But that is not sufficient for cognitive command.(Wright 2001, p. 86)

On this refined version of Wright's view, the proper slogan for cognitive command is not that blameless disagreement is impossible, but rather that wherever there is disagreement, there is locatable blame.

Wright comes to a similar conclusion in the 1992 book, on a different matter:

... it cannot be ... that [a cognitive] shortcoming may be indefinitely unidentifiable. For if that were so – if a situation were possible where we could definitely say that one of a pair of disputing theorists was guilty of cognitive shortcoming, but there was no way of saying who – then there would be no identifying the *winner* (if any) in the dispute either; and that would be as much as to allow that a true theory might be unrecognizable after all. When truth is regarded as essentially epistemically constrained, Cognitive Command requires the *identifiability* of cognitive shortcoming whenever it occurs. (Wright 1992, p. 163)

On the constructive reading, cognitive command does not hold in the situation with our scientists Karen and William. They cannot assign a cognitive shortcoming to either of them, since they both followed the best available procedure flawlessly. They could not *identify* a shortcoming unless they could somehow identify which of them is right, and by hypothesis, this is just what they cannot do.

Once again, the issues here invoke delicate issues in epistemology, as well as issues concerning what counts as "cognitive". These matters go well beyond the scope of this paper, not to mention my own competence. Let me briefly summarize the options on the table, when it comes to the underdetermination of theory. First, one might claim that the discourse in question is not objective, simply because cognitive command fails. Second, one might claim that the discourse in question is objective, but that epistemic constraint fails. The true proposition in question is unknowable. Or else one might maintain that the discourse in question is objective and that cognitive command fails anyway – in which case cognitive command does not track objectivity. We have a counterexample to Wright's thesis.[14] I take the fourth option – where the incorrect party automatically has a cognitive shortcoming – as ruled out.

We turn, finally, to mathematics. It is surely possible for one mathematician to *conjecture* that a certain proposition S about, say, the real numbers is true, and for another to conjecture that S is false, and for neither of them to display any cognitive shortcoming.

This happens all the time, but it is not the sort of disagreement relevant to cognitive command. I presume that people can have conflicting conjectures blamelessly in just about any area of discourse. All we need is for the participants to be in a less than perfect epistemic state.

Wright notes that when it comes to simple calculations, cognitive command obviously holds: " … it seems impossible to understand how a disagreement about the status or result of an elementary calculation might be sustained without some cognitive shortcoming featuring in its explanation" (p. 148). Suppose, for example, that two people differ on the product of two four digit numbers. Clearly, at least one of them made a mistake, and a careful check or a calculator will identify the guilty party or parties. Examples like this will occupy us in the next section.

As noted, the epistemic standard for serious assertion in professional mathematics is *proof*. So suppose that one mathematician, Pat, produces what she takes to be a proof of a mathematical proposition S; and that another mathematician, Karl, continues to demur from S, even after being presented with Pat's purported proof. Of course, this is not to say that Karl believes $\neg S$. His not accepting Pat's purported proof is enough for disagreement. The disagreement is over whether the purported proof is good. The question at hand is whether we can be sure, a priori, that at least one of these mathematicians exhibits a cognitive shortcoming – assuming that the disagreement is genuine.

To be sure, this sort of disagreement happens all the time in the real world of mathematics. For example, two referees may disagree whether the argument in a submitted article does in fact prove its conclusion, with the competence of neither referee (nor the author) in doubt. A number of prominent mathematicians expressed doubts concerning the validity of Wiles's first proof of Fermat's last theorem. Such doubts were vindicated, and the proof was repaired, but it may be that some mathematicians still remain unconvinced. Given the complexity of the proof, it is hard to doubt the competence of someone just because she harbors doubts about this case.

None of this is relevant to the matter at hand. As noted above, several times, we are not concerned here with the epistemic states of actual, flesh and blood, mathematicians. Actual mathematics, as practiced, is fully objective since it is epistemically unconstrained. Cognitive command is irrelevant. To give epistemic constraint a chance at being correct, and thus bring cognitive command into play, we consider the states of highly idealized mathematicians. Call them Pat* and Karl*.

In actual mathematics, it is not always clear whether a given purported proof has a unique formalization. It is not even clear whether the idealizations required for epistemic constraint will determine a unique formalization for purported proofs. As above, we assume that Pat* and Karl* have unlimited, stable memory and attention span, and they do not run out of materials. It does not follow, from that alone, that they will produce only completely rigorous, fully formalized proofs, or even that they will agree on the proper formalization of what they (or their human counterparts) do produce. Here, I just assume that speaking a formal language, whose logical form is explicit, is part of the idealization in question. I do not know how to argue for (or against) this, and will leave open the possibility that the relationship between actual mathematical discourse and formalized deductions may not be fully objective.[15] Here is another area where an opponent of objectivity has some room to maneuver.

So let us assume that Pat*'s (purported) proof Π of *S* is fully formalized, in the sense that all of the axioms and premises are explicit, and every step is an instance of a primitive rule of inference. The purported proof Π may be far too long for Pat or Karl to read in their lifetimes, but this is irrelevant. Pat* accepts the proof and believes its conclusion, while Karl* demurs and does not accept the conclusion.

We can assume that Pat* and Karl* agree on what sentence appears on any given line of Π. A disagreement there would involve a cognitive shortcoming on the part of one of them: the perceptual mechanism for recognizing which sentence is written in a given place would be faulty. So either Pat* and Karl* disagree over one of the premises of Π or they disagree over the validity of one of the primitive rules of inference. Let us take up each of these cases in turn, starting with the primitive rules of inference. This amounts to a disagreement over logic. Say that Pat and Pat* advocate classical logic, while Karl and Karl* are intuitionists.

This raises the question of the objectivity of logic, which could (and should) take up another lengthy paper. I will be brief here.[16] Notice, first, that *inferential error* is one of the items that Wright lists as a "cognitive shortcoming" in the formulation of cognitive command. In the case under study, Pat* and Karl* each find the other guilty of inferential error: Karl* accuses Pat* of accepting the validity of the invalid principle of excluded middle, and Pat* returns the favor, claiming that Karl* mistakenly demurs from a valid principle. In Wright's text, however, "inferential error" is listed as a result of "inattention or distraction", an example of "unsuitable conditions". Clearly, Pat* and Karl* do not accuse each other of inattention or distraction. Those are the very things we idealize out in moving to Pat's and Karl's asterisk-counterparts. So it is not clear, yet, whether the mutual accusation of "inferential error" is thereby an accusation of cognitive shortcoming. We cannot save cognitive command this easily.

Even if adopting classical logic or adopting intuitionistic logic somehow counts as a cognitive shortcoming, we would still have the problem, noted just above with our scientists William and Karen, of *locating* which of the parties has the shortcoming. If we cannot say which one of them has the faulty logic, then we still cannot say that cognitive command holds in this case. Is logic-choice an objective, cognitive matter? Over the centuries, logicians have disputed the correctness of various inferences, with what looks like rational argumentation.

Arguably, the methodology of logic-choice – how one goes about, or ought to go about, settling on which inferences to accept – is holistic, although this is yet another controversial matter that cannot be fully adjudicated here. Michael Resnik proposes an adaption of the program of "wide reflective equilibrium" formulated by Nelson Goodman and used by John Rawls for an account of justice:

> One starts with one's own intuitions concerning logical correctness (or logical necessity). These usually take the form of a set of test cases: arguments that one accepts or rejects, statements that one takes to be logically necessary, inconsistent, or equivalent ... One then tries to build a logical theory whose pronouncements accord with one's initial considered judgements. It is unlikely that initial attempts will produce an exact fit between the theory and the 'data' ... Sometimes ... one will yield one's logical intuitions to powerful or elegant systematic considerations ... [I]n deciding what must give, not only should one consider the merits of the logical theory *per se* ... but one should also consider how the theory and one's intuitions cohere with one's other beliefs and commitments, including philosophical ones. When the theory rejects no example one is determined to preserve and countenances none one is determined to reject, then the theory and its terminal set of considered judgements are in ... *wide reflective equilibrium*. (Resnik 1997, p. 159)

If something like this is the correct methodology for logic, its objectivity is brought into doubt, at least potentially. Is the logician's determination to preserve and countenance a given inference a cognitive matter, subject to rational appraisal, or is it more of a non-cognitive attitude, something like gilding and staining, as Resnik suggests? If the relevant determination is subject to rational appraisal, what is to be the logic for that? This is a complex matter, which would take us too far afield. Let us assume that the correct procedure of logic choice involves achieving some sort of reflective equilibrium, leaving the details out. We wonder whether the results of this process can claim some sort of objectivity.

It may be that our two mathematicians, Pat* and Karl*, are both in reflective equilibrium, each by the lights of his or her own logic. The two of them would thus be in the same situation as our scientists William and Karen, and we have the same triad of theoretical options. First, we can hold that cognitive command fails, and for that reason, logic is not objective. It would follow that mathematics also fails to be objective, at least to the extent that mathematics turns on logic. Second, one can maintain that for logic, epistemic constraint fails: it is unknowable which logic is correct, and thus which of Pat* and Karl* is correct. In particular, it is unknowable whether the law of excluded middle is valid, as strange as that sounds. This second option would leave logic objective, and would make cognitive command irrelevant to the issue. The same would go for mathematics, to the extent that it turns on logic. The third option is to maintain that logic is objective, in some sense, but that cognitive command fails for it.

I must admit that there is something troubling about the whole issue concerning the objectivity of logic. Just about any serious dispute in any area of discourse is going to involve logic. All disputants are themselves reasoners, and come to their respective conclusions in part by drawing inferences. Given how pervasive logic is, disagreements about logic are certain to result in disagreements elsewhere. In Wright's terms, logic is not "disputationally pure" (1992, pp. 155–156). Indeed, logic is about the most disputationally impure discourse that there is. Suppose that Pat* and Karl* agree that space–time is continuous. Then their disagreement about the logic of mathematics will result in a disagreement about space-time. Pat* will hold that a version of the intermediate value theorem holds for space–time and Karl* will not. For example, Pat* will hold that every continuous space–time curve that goes from the exterior of a given sphere to the interior has a point of intersection with the surface of the sphere. Karl* will demur from this. If logic-choice is not (completely) an objective matter, then neither is the structure of space–time. If the disagreement over logic is cognitively blameless, then so is the disagreement over the structure of space–time. In general, if cognitive command fails for logic, then how can it hold anywhere?

An argument is valid only if it is truth preserving. If the discourse in question is itself objective, then valid arguments preserve objective truth. I presume that it is an objective matter whether objective truth is preserved by a given argument. Intuitively, then, logic is at least partly objective. But, of course, the preservation of truth (objective or otherwise) is not sufficient for logical validity. Depending on the correct account of logical consequence, a valid argument preserves truth of necessity – it is not possible for the premises to be true and the conclusion false – or else a valid argument preserves truth in virtue of its form, or in virtue of the meaning of the logical particles, or in virtue of there being a particular sort of deduction of the conclusion from the premises, or .... The objectivity of logic thus seems to turn on the objectivity of the relevant sort of modality, or form, or meaning, or deduction, or whatever. I am afraid that, once again, we have strayed beyond the scope of the present article. It may or may not be that all legitimate discourses are connected to each other. The web of belief may or may not have seams. Still, *logic* is connected to just about everything else. The objectivity of logic is tied to that of other discourses, and vice versa. Sticking to the theme of this paper, if logic fails to be objective, in some sense and to some extent, then mathematics fails to objective in the same sense, to the same extent.

So let us now assume that the logic is not in question, and turn to the remaining possibility that our antagonists Pat* and Karl* disagree over a premise or axiom in the purported proof Π. Here the going is easier, for a bit. In mathematics, a difference over a premise is, prima facie, not really a disagreement. The two mathematicians are just talking past one another. Pat* is working in a certain structure (or type of structure), characterized in part by the premises of the derivation Π. Karl* prefers to work in a different structure. A mathematician who demurs from the Pythagorean theorem, because he does not assume the parallel postulate, is not in real disagreement with a Euclidean. They work in different theories, with different subject matters.

Recall that in setting up the scenario involving the underdetermination of theory in science, I stipulated: "Suppose that ... William says P and ... Karen says $\neg P$ – and assume that each attaches the same meaning to P." The claim here is that in mathematics, the assumption at the end is false. The Euclidean and the non-Euclidean geometers do not attach the same meaning (or extension) to terms like "straight" and "perpendicular", since they work with different structures. Terms get their meaning (or reference) from the structure in which they are embedded.[17]

Of course, mathematicians did not always think this way. Supposedly, they once saw the issue concerning geometry as concerning the structure of (physical) space or intuitions concerning perception. Alberto Coffa (1986, p. 8) describes the historical transition:

> During the second half of the nineteenth century, through a process still awaiting explanation, the community of geometers reached the conclusion that all geometries were here to stay … [T]his had all the appearance of being the first time that a community of scientists had agreed to accept in a not-merely-provisory way all the members of a set of mutually inconsistent theories about a certain domain … It was now up to philosophers … to make epistemological sense of the mathematicians' attitude toward geometry … The challenge was a difficult test for philosophers, a test which (sad to say) they all failed …

I think we understand the situation now. If Pat* and Karl* differ only over premises, then they do not disagree at all. They simply work in different structures. This explains why mathematical theories are not discarded as false when they become unusable in science. Resnik (1997, p. 131) calls the phenomenon "Euclidean rescue".

Things may not be this neat if the disagreement concerns a more foundational matter, one that relates to the semantic relationships between various mathematical structures, or the very relationship between premises and conclusion. In effect, we broach matters much like those involving the logic. Suppose that the "disputed" item is in real analysis, but that Pat*'s (and Pat's) proof invokes the set-theoretic principle V=L, and that Karl* (and Karl) rejects that, since he advocates a large-cardinal hypothesis incompatible with V=L.

One can still maintain that there is no real disagreement here. Pat* works within real analysis in the context of Zermelo-Fraenkel set theory plus V=L, while Karl* works in real analysis embedded in a different, but not competing background set theory that includes large cardinal axioms. As with our geometers, they just work in different theories. The idea is that characterizing the background set theory (or model theory, if the axioms are second-order) is part of delimiting the structure in question.

One might think, however, that the phenomenon of Euclidean rescue should not be extended to meta-mathematical, foundational matters, where the logical, or semantic relationships between theories are themselves under study. Logical consequence sometimes goes via model theory, which is an application of set theory. This is a deep issue in the philosophy of mathematics, and I'll rest content here to explore the options, along what should now be familiar lines.

To be sure, we do have intuitions about foundational matters, but they are not particularly stable from mathematician to mathematician, nor over time. It is plausible that foundational matters are decided on holistic grounds. Which foundational theory, overall, does best on a number of different criteria? If this is how it works, then Pat* and Karl* are in the same dialectical situation as the scientists William and Karen, above. Let's assume that both of them are in the mathematical version of reflective equilibrium, concerning foundational theories, and so we have no reason to fault either one on epistemic grounds. The disagreement is thus blameless.

So, for the third time, we encounter the options concerning the underdetermination of theory. Here, of course, there is less pre-theoretic agreement that our subject matter is objective. Is there a fact of the matter concerning V=L, the disputed item?

Suppose first, that there is no objective fact of the matter concerning the disputed foundational item, V=L in this case. Then there is no real dispute between Pat* and Karl*. They just work with different premises, and we have a Euclidean rescue. The difference between them would be exactly the same as that between someone who works in Euclidean geometry and someone who works in a non-Euclidean geometry. Once again, we rule out blameless disagreement by denying that there is disagreement. Pat* and Karl* agree that the "disputed" sentence S follows from $V=L$, and that it does not follow from the large cardinal hypothesis. Things are left at that.

To hold, against this, that Pat* (and Pat) is not at cross purposes with Karl* (and Karl), one might argue (or just insist) that the two mathematicians work with the same *concepts*. There is just one notion of "set" or "model", and there is no change of subject between them. If one still maintains that the there is no objective fact of the matter concerning the disputed item, $V=L$, then one should hold that there is genuine indeterminacy *for those concepts*.[18] The notion of "set" is not ambiguous, nor does it bifurcate into $ZFC+V=L$ and, say, ZFC plus a large cardinal assumption incompatible with $V=L$. If the logical consequence relation of second-order languages is somehow indeterminate, then it might very well be that there is no fact of the matter concerning $V = L$ on the unique notion of set as captured by second-order ZFC. I confess to having some trouble with this option, but this may be due to my structuralist and realist leanings (or prejudices). I leave it to the reader to determine how plausible it is.

Suppose now that there is an objective fact of the matter concerning the disputed axiom, $V=L$ (assuming, of course, that it makes sense to say this – if not, skip ahead). Then one of our idealized mathematicians, Pat* or Karl*, is mistaken. And yet, by hypothesis, neither of them display an (identifiable) cognitive fault in the process. One conclusion is that under the foregoing assumptions, epistemic constraint fails. The truth of the disputed proposition, or its negation, is an unknowable fact. As noted above, on Wright's view, this makes cognitive command irrelevant.

As with the scientific case, involving William and Karen, one can argue the other side as well. The truth in question *is* knowable, and indeed known by the idealized mathematician who got it right. It depends on delicate and contentious matters in epistemology, as applied to the mathematical cases. But if such a case can be made, then, given our (supposed) inability to identify a cognitive shortcoming, we must conclude that cognitive command fails, and so it does not track objectivity in this case. In this limited area, Wright's criterion is at odds with the underlying intuitive notion of objectivity. As in the other cases, we'd like an account of why this is so, and a motivated refinement of the notion of cognitive command to handle the exceptions.

4. RESPONSE-DEPENDENCE: THE EUTHYPHRO CONTRAST

Here, I submit, we can be more definitive. By definition, for any discourse that satisfies epistemic constraint, "truth" and "best opinion" coincide in extension. In the early stages of Plato's *Euthyphro*, Socrates did not contest the claim that an act is pious if and only if it is pleasing to the gods. Instead, he asked which of these is the chicken and which the egg. Euthyphro contended that there is no more to piety than what the gods desire. Against this, Socrates argued that (at best) the gods have the ability to *detect* piety. A similar contrast between Socrates and Euthyphro could remain if the gods were replaced with actual human beings, or ideal agents acting under ideal conditions – even if (or especially if) the opinions of these agents were infallible. Socrates's view here is that piety is objective. Euthyphro's perspective is consistent with piety being subjective or otherwise judgement-dependent, in which case the responses of the gods is what *constitutes* piety.

The Appendix to Chapter 3 of Wright (1992) lays out constraints on a Euthyphro contrast for discourses concerning color, shape, morality, modality, etc. It is especially clear that the intelligibility of the underlying question depends on epistemic constraint. If truth does not coincide in extension with best opinion, then there can be no question of which is the chicken and which the egg. We would not have a Euthyphro question to ask.

John Divers and Alexander Miller (1999) argue that mathematics is response-dependent, or perhaps better, judgement-dependent.[19] Actually, their case applies only to decidable arithmetic statements, such as instances of primitive recursive predicates. I am content to focus on those, since I take it as agreed that decidable statements give the defense of response-dependence its best shot. One would think that in order for a mathematical predicate to be judgement-dependent, it should be effectively decidable and thus, with Church's thesis, recursive. Someone who wants to argue that all of mathematics, or even all of arithmetic, is judgement-dependent has a much tougher row to hoe.

Wright argues that discourse about color and first-person ascriptions of intention are response-dependent, and that discourses about shape and morality are not. Divers and Miller argue that (the relevant part of) arithmetic discourse has the relevant features of color discourse and first-person intention discourse, and they argue that arithmetic discourse does not have the disqualifying features of shape discourse and morality discourse. I submit, however, that Divers and Miller do not take account of the idealizations needed to maintain epistemic constraint in arithmetic. Without the idealizations, the case for judgement-dependence does not get off the ground. I show here that articulating the idealizations indicates how and why arithmetic, and mathematics generally, is not judgement-dependent. In short, even if we can maintain that epistemic constraint holds, we must put mathematics on the Socratic side of the divide.

To set the stage for the articulated criterion, Wright first proposes that we focus on discourses for which the following *basic equation* is true:

$$\text{For all } S, P: P \text{ if and only if (if } CS \text{ then RS)},$$

where S is any agent, "P" ranges over some wide class of judgements ... "RS" expresses S's having of some germane response (judging that P ...) and "CS" expresses the satisfaction of certain conditions of optimality on that particular response. (Wright 1992, pp. 108–109)

Essentially, this is a semi-formal statement of epistemic constraint.

A few pages later, Wright notes that the basic equations are not what we want in general. In bizarre circumstances, the satisfaction of the proper conditions, *CS*, might alter the truth-value of the proposition in question, due to some interference. Suppose, for example, that there is a weird red object in a dim room, not optimal for viewing color. Suppose that, due to the object's chemical composition, it would turn blue if more light were shined on it. In other words, getting the object into proper viewing conditions changes its color. The basic equation would have it that the object is blue, even in the dim room. This is clearly wrong. So Wright opts for what he calls a *provisional equation*:

> If *CS*, then (it would be the case that *P* if and only if *S* would judge that *P*). (p. 119)

The idea, I take it, is that we are only concerned with propositions whose truth or falsehood can be judged under the appropriate ideal conditions. I presume that for the other propositions, epistemic constraint need not hold (although we cannot square that with the failure of objectivity, as above).

Divers and Miller (1999, pp. 307–308, note 5) point out that in the case of arithmetic, we can deal with the original, non-provisional basic equation, since there can be no interference between the obtaining of the proper conditions for judgment and the truth-value of the judged proposition: "We can know a priori in the mathematical case that there will be no causal interference of the sort discussed, since mathematical objects, platonistically construed, are neither causally active nor causally acted upon." Even if one is not a platonist about mathematical objects, it is still plausible that the truth value of a proposition of arithmetic cannot be affected by whether or not some conditions of judgement are met. Getting ourselves into position to judge whether a number is prime cannot change whether that number is prime.

There is, however, a second reason, beyond interference, to move to provisional equations. What of the judge herself? In some cases it may not be possible for a subject to be in the appropriate conditions, optimal for judging. In unfortunate circumstances, bringing about the optimal conditions may make it impossible for someone to judge. Suppose, for example, that there were a subject who would become color blind if he were to view a particular object under the proper lighting, due to some strange feature of his optic nerve. Even more gruesome, suppose there were a peculiar shade called "killer-yellow" which kills anyone who looks at it in normal lighting (see Lewis 1997, p. 145, who attributes the thought experiment to Saul Kripke). The basic equation would declare that an object so colored has no color at all, since it cannot be appropriately judged to have a color. Thus the move to provisional equations.

In the case of arithmetic, this consideration brings in the idealizations. Almost all natural numbers are "killer-numbers" in the sense that anyone who tries to determine whether they are prime will die of old age in the process. If we did invoke provisional equations, we would have to restrict the range of judgement-dependence in arithmetic to a small, finite collection of natural numbers. This would not be a very interesting thesis (up to the dismissal of strict finitism in Section 1 above). And it would not account for very much mathematical knowledge. The problem translates to the issue of how "human" our subjects remain, once we idealize in the needed way.

So let us focus on the non-provisional basic equation discussed by Divers and Miller:

$$\forall x(x \text{ is prime} \equiv (\text{if } C \text{ then } S \text{ judges that } x \text{ is prime})),$$

where C is a list of conditions on the subject S. Wright argues that a discourse for which a basic equation is true can be construed as judgement-dependent, and thus as not objective, if four constraints on the formulation of the conditions C on proper judgement are met. These are labeled the "a prioricity", "substantiality", "independence", and "extremal" constraints.

The first constraint is that the basic equation must be knowable a priori: "The truth, if it is true, that the extensions of colour concepts are constrained by idealised human response – best opinion – ought to be accessible purely by analytic reflection on those concepts, and hence available as knowledge a priori" (pp. 116–117). Second, note that one can make the basic equation true, and knowable a priori, too easily. Concerning discourse about shapes, Wright writes:

Suppose we characterize "standard conditions" as ones supplying everything necessary (whatever-it-takes) to enable a standard observer to apprehend shapes correctly … Then the basic equation for "square" … is, trivially, dignified as a necessary truth. There is therefore no hope of capturing the distinction we want … unless we stipulate that the *C*-conditions imposed on the subject be specified *substantially*: they must be specified in sufficient detail to incorporate a constructive account of the epistemology of the judgements in question, so that not merely does a subject's satisfaction of them ensure that the conditions under which she is operating have "whatever-it-takes" to bring it about that her opinion is true, but a concrete conception is conveyed of what it actually does take. (p. 112)

This is the substantiality constraint. The independence constraint is an extension of the substantiality constraint, and has no bearing on the present argument.

Suppose, finally, that one formulates *C*-conditions so that the a priority and substantiality (and independence) constraints hold. The extremal condition is that there be no way of accounting for the strong match between best opinion and truth, other than the thesis that best opinion constitutes truth. In other words, the burden of proof is on the advocate of judgement-dependence. She must show that there is no other way to explain the basic equation.

Divers and Miller propose a list of *C*-conditions for arithmetical discourse. First, the judging subject should be sincere. In saying that a given number n is prime, for example, the subject should be expressing her belief that n is prime. Second, there are background psychological conditions: "the speaker is sufficiently attentive to the object(s) in question [i.e., the number(s)], the speaker is otherwise cognitively lucid, and the speaker is free from doubt about the satisfaction of any of these conditions" (p. 287). The speaker must be "conceptually competent", in the sense that she understands the sentence in question: "in making the report or judgement … the speaker must be competent with whatever concepts are directly and conventionally implicated in the use of the sentence … and competent with whatever concepts have to be mastered in order to achieve competence with the directly implicated concepts" (*ibid.*). So in the case at hand, the subjects must understand the concepts of "natural number" and "prime", along with whatever is involved in that, presumably the concepts of addition, multiplication, quantification, and the like. Divers and Miller suggest that the subjects should not just be minimally competent with these notions, but experts. I presume that professional mathematicians qualify. Finally, the number, or numbers, must be given in canonical notation. Stroke notation or standard decimal numerals will do. Locutions like "Frege's favorite number" and "the least counterexample to the Goldbach conjecture, if there is one" are not legitimate singular terms in this context.

Divers and Miller settle on the following basic equation:

> $\forall x$(x is prime $\equiv$ [$\forall s$](s meets the conditions on reporting, on background psychological considerations and on conceptual competence, and x is presented to s in a canonical mode of presentation $\rightarrow$ s will judge that x is prime)) (p. 292).

They then go on to argue that this meets Wright's conditions. In particular, they show that these basic equations compare favorably with those for color and intention reporting, and unfavorably with those for shape and morality.

It is unfortunate that the main instance of the basic equation that Divers and Miller invoke is the primeness of the number 5. It is plausible that anyone, human or otherwise, who grasps the concepts of natural number and primeness, and knows which number 5 is, will accurately judge that it is prime. If someone judges that 5 is not prime, then, quite literally, he does not know what he is talking about. But what of even slightly larger numbers like 73, or 277, or 10,200,007, not to mention the numbers alluded to above that have over 100,000 digits?

For all but a few numbers, even expert mathematicians make mistakes, and such errors do not undermine their competence with the concepts. So far, it has not been built into the specification of the *C*-conditions that the subjects do not err in multiplying and dividing numbers. It is only an a posteriori fact about (some of) us that we can do such calculations reliably, without making a mistake, if the numbers are small enough. And it is simply false that humans can do, say, 300 different calculations in a row without making a mistake. I know that I am not reliable with even one calculation, if it involves numbers with two or three decimal places.

The advocate of judgement-dependence may claim that it is part of understanding the concepts of natural number, primeness, and related notions, that one can calculate correctly. I do not see this. As noted above, Divers and Miller say that the proper judges are experts – professional mathematicians, for example. This is not enough to ensure that no computational errors are made. It is quite possible for a mathematician to fully understand the notion of natural number and the notion of primeness, and still be unreliable in arithmetic calculation, when it comes to three or four digit numbers.

I know such people. They are world class mathematicians, and so understand their concepts if anybody does, and yet they are poor "judges" concerning the extension of "prime number".

In reply to this observation, Divers and Miller might say that when calculating, these mathematicians are insufficiently attentive. But this is hard to maintain. The mathematicians are as attentive as they are when proving theorems, in their professional work. They just mess up fairly often when it comes to simple calculations.

I might add that, with rare exceptions, no one is reliable with calculation with three and four digit numbers unless they use external aids. But even pencil and paper reliability depends on a posteriori features of our physical universe. If we lived in a world in which numeral tokens did not last more than a few seconds, and changed shape randomly, we would not get the right results very often. And beyond four digit numbers, we use calculators and computers, or supercomputers for the larger numbers. The reliability of those items is surely an a posteriori matter. With sufficiently large numbers, even computers are not reliable. At some point, the probability that a random malfunction will occur is greater than, say, .5.

I submit, then, that even for expert human mathematicians, Divers and Miller's version of the basic equation is false. It holds only for smallish numbers, and except for the very smallest of numbers, the truth of the basic equations is an a posteriori matter. The vast majority of the instances of the equations are not guaranteed a priori, by conceptual analysis of the *C*-conditions and the concepts in question. In short, Wright's a prioricity condition fails.

This much should be anticipated. The Euthyphro contrast is directly tied to epistemic constraint, and as we saw, starting in Section 1 above, one can maintain that mathematics is epistemically constrained only by idealizing the knowers. If we are to have a chance at holding that mathematics is judgement-dependent, we must specify the *C*-conditions in such a way that no human being even comes close to satisfying them.

In a different context, Divers and Miller themselves raise a related matter. In motivating the requirement that the number be given in canonical notation, they raise the possibility that "an ideal (i.e., maximally conceptually equipped) judge would be in a position to make a truth-value-matching judgement but no actual judge, pro tem, has the conceptual equipment that qualifies her as ideal" (p. 290). They do not note that this same problem applies even if the number is given in canonical notation, if it is large enough.

One might well wonder if we have lost touch with the notion of objectivity, once we concede that arithmetic is not human-judgement-dependent. As noted in the treatment of epistemic constraint in Section 1 above, other cases of purportedly judgement-dependent concepts invoke judges that are in fact human, or at least approximated by humans.

Still, let us push on. Can we specify *C*-conditions that meet Wright's criteria on *ideal* judges? How would this go? As above, the usual way to begin is to specify that the judges have no limits on attention span, memory, and lifetime. To avoid issues concerning physical properties of external objects like pencil, paper, and computers, let us just give our ideal judges unlimited, stable memory and retrieval.

This, alone, will not do. Let us start with the aforementioned human (world-class) mathematicians who are rather bad at calculation. They already display enormous powers of concentration, considering the depth and complexity of their published work. Let us assume that they have *unlimited* powers of concentration. They do not get tired, and have perfect memory and recall. Does it follow *a priori* that they never make calculation errors? It seems to me that it remains a conceptual possibility that these ideal mathematicians still make mistakes when they calculate with large numbers. Often, when they try to calculate, they mess up. After all, unreliability in calculation does not compromise their conceptual competence in the actual world, where they are made of flesh and blood. Why should unreliability in calculation be ruled out in the idealized cases – so long as we specify the idealization in the standard ways?

Of course, it will not do to specify the *C*-conditions as ones in which the subjects do not make errors in calculation. This would violate the substantiality constraint. It would be to specify the conditions on judgement as those under which our subjects get it right, whatever it takes.

The route for the defender of response-dependence is clear enough. First specify the algorithms for addition, multiplication, division, etc., and assume that our judges understand how to perform the individual steps of each algorithm. Real humans manage that much. Then we assume that they can string together arbitrarily long sequences of such primitive steps flawlessly. In effect, we follow the aforementioned proposal from Tennant (1997, Chapter 5) in the context of epistemic constraint for decidable mathematical predicates (Section 1 above). To repeat:

> We are equipped, right now, to perform tasks such as applying Eratosthenes' sieve. The sheer size of the number whose primality is in question is neither here nor there when it comes to our ability to conceive the kind of fact that it is (or would be) for some gargantuan number N ... to be prime. (p. 145)
>
> ... the actual limits to effective human thought ... that we are thinking of ourselves as transcending here are not limits to the kind of thinking we may do, but only limits on how much of that kind of thinking one could do. The thinking is all of one uniform kind. (p. 147)

If the idealizations are specified in this manner, then it does follow that the "subjects" will answer questions about primeness correctly. The basic equation is true, and the substantiality constraint is now met. To paraphrase Wright (1992, p. 112), we have conveyed "a concrete conception ... of what it actually does take" for our idealized mathematicians to make correct judgements about which numbers are prime. Moreover, the basic equation is now knowable a priori, on conceptual grounds alone. So the a prioricity constraint is met.

But this is the end of the line. I submit that the extremal condition is violated. With all this idealizing, we have turned our subjects into abstract calculating devices, like Turing machines. That is, our ideal mathematicians are themselves mathematical objects, and the basic equation is itself a piece of mathematics. In effect, the basic equation is just a routine theorem that a Turing machine with such and such a program calculates the characteristic function of "prime number". So of course the equation is knowable a priori, assuming that mathematics is. This also explains why our "subjects" get it right all of the time. The problem is that their accuracy has nothing to do with the fact that they are human-like and have "responses" or "beliefs", and make "judgements". The accuracy is guaranteed by the mechanisms or the algorithms for computation themselves, and not by the responses or judgements to the results of the algorithms.

Clearly, this very elementary part of arithmetic is what may be called "Turing-machine-dependent". All that this means is that properties like primeness are effectively decidable, in the mathematical sense of "effectively decidable". The "responses" of Turing machines match up perfectly with the facts. This is true by definition, or by theorem (depending on how one axiomatizes). I don't think it matters much if there is a fact of the matter as to whether the Turing machine "responses" or the facts about primeness are the chicken or the egg. A natural number n is prime if it has exactly two divisors, itself and 1. This is true if and only if certain calculations come out a certain way – say by following the sieve of Eratosthenes. This last is a mathematical fact, proved in the usual manner. I do not see how we have said anything against the objectivity of mathematics if we define another mathematical object – an ideal subject – who correctly does the relevant calculation and then "judges" or "comes to believe" the result of the calculation. The calculation is doing the determination, if anything is, not the "response" or "judgement" at the end.

On this axis of objectivity, then, we can be definitive; there are no loose ends or caveats to note. In order to get a Euthyphro contrast started, we have to invoke the idealizations on the knowers or judges needed to sustain a semblance of epistemic constraint. Otherwise, Wright's basic equations are simply false. The details of the idealization then demand that the basic equation be given what Wright calls a "detectivist" or Socratic reading. The elementary propositions of arithmetic are thus not response-dependent or judgement-dependent in the sense relevant to objectivity.

The journey here was long, but I hope it was worthwhile. To summarize, if we are dealing with actual mathematics, as practiced by human beings, or beings who approximate humans, epistemic constraint fails: there are unknowable truths. This occurs for classical and intuitionistic mathematics alike, and even for moderate versions of strict finitism. Thus, actual mathematics is objective. To give a non-objective account a chance, and to invoke Wright's various criteria, we have to idealize on the mathematician. Once this is done, I contend, mathematics passes all of Wright's tests for objectivity, with a few possible exceptions for foundational matters. Delicate and controversial issues concerning general epistemology are invoked along the way.

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NOTES

[1] I am indebted to Mark van Atten here. Some contemporary intuitionists, following Michael Dummett (e.g., (1973)), base their conclusions on considerations of meaning and the learnability of language. At least one of them, Neil Tennant [1997], takes mathematics to be objective.

[2] Since the issues with the potential divergence are delicate and complex, the "exceptions" to the general thesis that mathematics passes Wright's tests take up a significant amount of our space.

[3] It will not help our opponent to adopt a philosophy, like that of Geoffrey Hellman (1989) or Charles Chihara (1990), that denies the existence of numbers, but provides an interpretation according to which mathematical assertions are non-vacuously true or false. On such views, it is at least possible for there to be sentences corresponding to P, and so there can be true sentences that cannot become known by using resources available to human beings. One might avoid the whole problem by adopting a variety of mathematical fictionalism, an error theory about mathematics, perhaps following Field (1980, 1989). On that view, there are no unknowable truths, simply because there are no (non-vacuous) truths. On such views, I presume, there is no issue of objectivity to discuss. On the other hand, for Field, there are serious questions concerning whether a given mathematical sentence is *conservative* over nominalistic physics. If we query the objectivity of such questions, the same issues will arise. Wright (1992, pp. 9–12) discusses error theories in the context of objectivity.

[4] In all likelihood, there are unknowable truths that can be feasibly stated. Consider the proposition Q that the $10^{200,203}$th prime greater than $10^{200,721}$ leaves a remainder of 5 when divided by 32. It is a theorem of intuitionistic arithmetic that Q is either true or false. If Q is true, then, surely, it is an unknowable truth. Otherwise, $\neg Q$ is an unknowable truth. Thanks to a referee here.

[5] If $\boldsymbol{\Psi}$ is a sentence in the language of arithmetic, then let $\ulcorner\boldsymbol{\Psi}\urcorner$ be its Gödel number (in some, fixed, arithmetization). The version of Tarski's theorem I have in mind is that for every formula $\boldsymbol{\Phi}(x)$ in the language of arithmetic, with one free variable, there is an arithmetic sentence χ such that $\neg(\boldsymbol{\Phi}(\ulcorner\chi\urcorner) \equiv \chi)$. So $\boldsymbol{\Phi}$ is not a definition of arithmetic truth. The meta-theorem in question does not invoke a philosophically contentious notion of "truth".

[6] I am indebted to Crispin Wright here. The main observation of Shapiro (2001) is that under Heyting semantics, excluded middle amounts to Gödelian optimism. So the intuitionist who is not out to revise classical mathematics must side with Gödel and Hilbert. This, of course, is not bad company, but there is a serious burden to discharge.

[7] It does not matter for present purposes how plausible this view is as an account of morality. If the notion of a disinterested, unbiased judge is coherent, then we have a response-dependent account of *something*, even if it is not a moral notion of right and wrong.

[8] This, of course, is controversial. One might think that the moral corruption of a given person helps explain why she has no friends, or why she is prison. Wright argues that in such cases, the proper (or best) explanation does not invoke moral facts, but turns instead on "our being in attitudinal states which take such states of affairs as object". Thanks to an anonymous referee for forcing this issue.

[9] A referee finds it curious that in the cited examples, mathematical language is used in stating the phenomena to be explained. Wright's example invokes the notions of a "rectangular" floor and of "remainder", and my own refers to drops, which are (more or less) spheres. It seems to me that this is all but inevitable. If mathematics is to figure in an explanation of a phenomenon or event, there must be some connection between the subject matter of mathematics (whatever that may be) and the phenomenon or event in question. So the terminology of mathematics must engage in some way with the terminology in which the phenomenon or event is described. To switch to material mode, this observation underlies my own view is that there is no sharp separation between the mathematical and the physical (see, for example, Shapiro 1997, Chapter 8, especially Section 3).

[10] I am indebted to Agustín Rayo for this suggestion.

[11] For illuminating accounts of explanation in mathematics, and mathematical explanations of physical phenomena, see Steiner (1978, pp. 17–28, 1980) and the papers in Mancosu, et al. (2005). Steiner's account is criticized in Mancosu and Hafner (2005). Most of the latter concerns explanation within mathematics, whereas present concern is with the use of mathematics in explanation of non-mathematical phenomena.

[12] This is not to say that every disagreement about comedy is cognitively blameless: "That is not funny: he is having a seizure!"

[13] I am indebted to an anonymous referee for pushing some of the epistemological issues here.

[14] Chapter 4 of Wright (1992) deals with the Quine-Duhem phenomenon that observation is theory-laden. Wright comes to tentative conclusions similar to those reached here: it is hard to maintain both that epistemic constraint holds in science and that cognitive command is a criterion of objectivity.

[15] Thanks to a referee for pointing me to this issue.

[16] Shapiro (2000, Section 5.4) applies Wright's criteria for objectivity to logic. Present conclusions are a bit different from those. Once again, I am indebted to a referee for pressing this issue.

[17] We might say the same about the dispute over logic. It is commonly argued that the intuitionist and the classical mathematician do not attach the same meaning to the logical terminology. Thus, they do not really disagree. The intuitionist may accuse the classical mathematician of incoherence, but that is a different matter.

[18] I am indebted to Crispin Wright here.

[19] This is the cornerstone of their response to a challenge, due to Field (1989), to show how platonism is compatible with the reliability of mathematicians' beliefs. If mathematics were judgement-dependent in the appropriate manner, the challenge would be met.

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