The objectivity of mathematics

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Crispin Wright's *Truth and objectivity* [1992] is one of the most sustained treatments of the notion of objectivity in recent years. It is more comprehensive than any other account 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 several different notions or scales of objectivity, and a given stretch 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; a number of important issues and debates are engaged along the way.

There is another way to look at it. I confess to an intuition that mathematics is objective. At least to 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. From that perspective, the aim of this paper is to test Wright's criteria for objectivity, to see how well they fare on what I take to be a paradigm of objective discourse. I realize, of course, that my "intuition" is little more than a prejudice, subject to correction in light of philosophical theorizing. The intuition and Wright's criteria are thus to be 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 only a few limited areas where, from a certain perspective on a number of philosophical side issues, one can maintain that some highly foundational mathematical matters fail one of Wright's tests for objectivity. Those potential exceptions aside, someone attracted to a non-objective account of mathematics, whether intuitionist,[1] non-cognitivist, projectivist, or whatever, needs to find fault with Wright's criteria, or with the present application thereof.

According to Wright, the first hurdle for a non-objective account of any discourse is to establish 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, it 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", then the two formulations are equivalent (assuming that only truths are knowable).

Plausibly, in mathematics, the standard of knowledge is *proof*. So the principle of epistemic constraint is that all mathematical truths are provable. But of course, we cannot prove everything. Proofs have to start somewhere. And so we have to deal with axioms and premises as well as theorems.

If epistemic constraint fails for a given area of discourse—if there are true propositions in that area whose truth cannot become known—then it 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

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[1]The early intuitionists, L. E. J. Brouwer and Arend Heyting, took mathematical objects to be mind-dependent. Brouwer held a Kantian view that mathematics is tied to a pure intuition of time. Prima facie, this makes mathematics non-objective, in at least some sense of the term. Contemporary intuitionists, following Michael Dummett, 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.

> 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 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)

It is only when epistemic constraint holds that Wright's other criteria—width of cosmological role, the Euthyphro contrast, Cognitive Command—come into play. Here lies the potentially 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 here that mathematics is not epistemically constrained: there are, or at least can be, unknowable mathematical truths. But this is a delicate matter concerning what we mean 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 all mathematical truths are knowable. So I concede epistemic constraint, properly construed, for the sake of argument, in order to turn to Wright's other criteria for objectivity. As noted, 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. To be more careful, I explore what one must hold in order to deny any of these claims, suggesting that the costs are high, or that questions are begged, or that the conditions for application fail outright. I conclude that we cannot specify the needed idealizations without begging the question, or assuming the objectivity of mathematics.

1 Epistemic constraint. Are there, or can there be, unknowable (i.e., unprovable) mathematical truths? This may depend on what one means by "knowable" (or "provable"). Who is doing the knowing, and the proving? What processes and criteria are they to use?

There is a sense in which it is all but obvious that there are unknowable mathematical truths—that epistemic constraint fails badly. We know that there is no largest prime number. So let *p* and *q* be prime numbers, each of which is greater than $10^{100,000}$. If their product *pq* were written in standard arabic, decimal notation, it would have at least 200,000 digits. Consider the proposition, or sentence, *P* that *pq* has exactly two prime factors, each of which is at least $10^{100,000}$. 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 understand the proposition *P*. To be sure, there is a simple algorithm that will result in a verification of *P*. A bright 10 year old can grasp the algorithm, and can even get started executing it, given the first few digits of *pq*. But it is quite likely that 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 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. We *do* know that much. The proposition that there is no largest prime number is an easy consequence of even intuitionistic arithmetic, and the intuitionist agrees that for any number *n*, either *n* is prime or composite. So it is a theorem of intuitionistic arithmetic that there are prime numbers *p*, *q* which are greater than $10^{10,000}$. So there is a proposition like

P. 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, after all, and even the universe seems to be finite. There are prime numbers that cannot be represented even using all available material in the universe. And there are prime numbers that can be represented more or less feasibly, but which cannot be known to be prime, given the finite resources available.[2]

So our current opponent must reject even intuitionistic arithmetic. The only way he can deny the existence of the proposition, or sentence *P* is to deny that there are large numbers. This is to adopt a strict finitism, holding that there are only finitely many natural numbers. This would represent severe restrictions, and a crippling of mathematics as we know it, much more so than intuitionism. I have no interest in engaging that opponent here, with apologies to any readers inclined that way.

A more common response is to assert that the considerations brought here 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. Something similar holds for Erret Bishop's [1967] constructivism. Dummett's intuitionism is grounded in his semantic anti-realism. He also argues, on linguistic grounds, that all truths are knowable (see also Tennant [1987], [1997]).

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[2]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 that makes mathematical assertions true or false. Even on such views, there are true sentences that cannot become known by using resources available to human beings. One might avoid the whole problem by adopting a fictionalism. On that view, there are no unknowable truths, simply because there are no truths. Wright [1992, 9-12] discusses error theories concerning other parts of discourse in this context.

From the side of classical mathematics, Kurt Gödel held that every unambiguous mathematical proposition is either provable or refutable, on broadly rationalistic grounds (Wang [1974, 324-5], 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".

Sometimes this view is called "Gödelian optimism" (e.g., Tennant [1997, 166]). The opening of Hilbert's celebrated "Mathematical problems" lecture [1900] is an enthusiastic endorsement of optimism:

> 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.

The optimist and the intuitionist are well aware of the above considerations concerning feasible construction. When one of them declares that all mathematical truths are knowable, she does not mean knowable-in-our-lifetime, or knowable-using-available-resources. The modal suffix, "able", invokes idealizations of normal human abilities. For a start, these include, at least, the idealizations involved in the notion of computability. It is assumed, for example, that the knowing mathematician has unlimited time, materials, and attention span. 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, the truth or 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. On the same grounds, the proposition P broached above is knowable.

Notice that when the question of objectivity is raised for other areas of discourse, and a philosopher declares that all truths in a given area are knowable, he does not invoke idealizations, or at least not to the extent invoked here. 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 speaks of normal lighting conditions. But as the name indicates, we are speaking here of the conditions of judgement, not the state of the judge. And 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, there is 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.[3] In this case, 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.

However, on accounts like these, the idealizations can reasonably be *approximated* by (some) real human beings. On the views 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. Menzies [1998, 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

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[3]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 the notion of right and wrong.

> 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.

If, in a different case, the idealizations could not even be approached, or approximated, by real people, then I would say that on the view in question, epistemic constraint fails. There would be some truths, about how idealized human beings would judge, that we cannot know. I submit that this is the case with mathematics. The idealizations involved in even elementary arithmetic go far 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 addition. We can know only a small finite number of those. We cannot approximate, or approach direct knowledge of very many more.

In his defense of semantic anti-realism, Neil 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)

> . . . 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)

Wittgensteinian considerations of rule-following come to the fore at this point. Both the advocate of epistemic constraint and his opponent have to give an account of how the given rules themselves always determine the outcome. I propose to put such issues aside here, and just take for granted the notion of effective decidability, assuming that it is determinate. Even if we maintain, with Tennant, that epistemic constraint holds for individual applications of effectively decidable predicates, the idealizations involved in the intuitionistic and optimistic assertion of epistemic constraint go well beyond the usual idealizations of computability. Tarski's theorem, which is acceptable to both the intuitionist and the classical optimist, is that arithmetic truth is not arithmetically definable. 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. It is not just a matter of more of the same, 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 $\Phi(x)$ is a formula in the language of arithmetic, and that for each natural number n, if $\Phi(n)$ then the sentence coded by n is true. Let Ψ be a fixed-point for $\neg\Phi(x)$ so that $\Psi \equiv \neg\Phi(\ulcorner\Psi\urcorner)$ is provable. Assume $\Phi(\ulcorner\Psi\urcorner)$. Then, by hypothesis Ψ is true, and thus so is $\neg\Phi(\ulcorner\Psi\urcorner)$. This is a contradiction. So $\neg\Phi(\ulcorner\Psi\urcorner)$, and thus Ψ. So under the hypothesis that all of the Φ's are true, there is a true sentence that is not in the extension of Φ. So the Φ's do not exhaust the truths.

This, of course, is not an objection to either optimism or intuitionism. Gödel [1951, 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. The mathematician/physicist Roger Penrose[1996, §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 dispute with the optimist 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 truth is knowable. Of course, one can always envision or postulate the existence of some mind-like entities that somehow knows every truth of 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.

The intuitionists, of all stripes, join the optimist in holding that 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, for example, 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 §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.[4]

In any case, 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 a sense relevant to the issue of objectivity. Let me just register skepticism toward this project. At the same time, I hereby concede epistemic constraint for the sake of argument, so that we can invoke Wright's other criteria for objectivity. We will have more to say on the required idealizations as we go, especially in Section 4, on response-dependence.

2 Cosmological role. Wright [1992, 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]

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[4]I am indebted to Crispin Wright here. The main observation of Shapiro [2001] is that under Heyting semantics, excluded middle amounts to Gödelian optimism. Bishop [1967] calls excluded middle a principle of "limited omniscience". So the intuitionist who is not out to revise classical mathematics must side with Gödel and Hilbert (not that this is bad company).

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". Cosmological role is one of the hallmarks of objectivity.

The idea here is that a discourse is apt for a realist construal 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, 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.

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. Of course, the philosophical literature on explanation is extensive, and there is nothing close to consensus, but I presume, or at least hope, that we do not need a definitive resolution of those matters here. Intuitively, to explain something is to give a reason for it. According to *Webster's twentieth century unabridged dictionary*, to explain is to "make plain, clear, or intelligible; to clear of obscurity". It is a highly contextual matter. What makes for an explanation—what makes for clarity, intelligibility, and lack of obscurity—depends on interests, goals, and background assumptions.

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.[5] Wright [1992, 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.)

A friend once told me that he once noticed some strange behavior at the edges of an oscilloscope during a physics lab in college. When he asked about the phenomenon, the lab instructor wrote something on the board, probably a differential equation, and then said that the phenomenon occurs because the function so defined has a zero at a certain point. 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.

So mathematics does figure in explanations well beyond mathematics itself, and well beyond propositional attitudes concerning mathematical propositions. It passes the letter of wide cosmological role, with flying colors. The opponent of objectivity might complain that the criterion was not formulated properly. It is not a matter of whether mathematics in fact figures in explanations of non-mathematical (and non-propositional attitude) phenomena, but whether mathematics *must* figure in such explanations. In analogy with a deflationist about truth, our opponent might claim that mathematics is dispensable. Any relevant explanation that invokes mathematics can be reformulated so that it does not invoke mathematics.

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[5]For an illuminating account of explanation in mathematics, and mathematical explanations of physical phenomena, see Steiner [1978, 17-28], [1980].

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, with little to support it.

Even if a nominalist program succeeds one day, it may be that the mathematics-free versions of the explanations do 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 wordy, or unclear, or obscure for anyone to understand it. 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.

To this, our opponent of objectivity might retort that the proper criterion of cosmological role does not concern explanations for real flesh and blood human beings, but explanations-in-principle. 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 plausible this maneuver is. Why are the interests of our highly idealized counterparts relevant, as opposed to our own interests and goals? Nevertheless, if this is the shape of the dialectic, then we have returned to the idealizations invoked above, to get epistemic constraint on the table. We return to that matter in §4 below.

One might attempt to eliminate the 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 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" is second-order logic. And the result is only marginally more complex than one that invokes ordinary mathematics.[6]

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 phenomenon. It shows that (second-order) logic itself has wide cosmological role. Since mathematics is part of logic, mathematics also has wide cosmological role, as above. A philosopher who thinks that mathematics 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 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 places where an opponent of objectivity might have a little wriggle room, but not much. The underlying issues cannot be pursued here.

For a different tactic, the opponent of objectivity might argue that the above (re-)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 the width of cosmological role. The burden on this opponent is to articulate which parts of an explanation are explanatorily relevant, and defend the thesis. If this is the direction our opponent takes, then I propose to leave this topic until the burden is discharged.

3 Cognitive Command. Suppose that the purpose of a given area of discourse is to describe some mind-independent reality. It follows that if two speakers 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

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

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, 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. A non-cognitivist about ethics would say the same about moral disagreements.

Wright [1992, 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.

The relevance of Cognitive command to the matter of objectivity depends on the epistemology of the discourse in question. Suppose, for the sake of argument (at least), that ordinary science exhibits something in the neighborhood of Quine's underdetermination of theory by data. The scientist strives for reflective equilibrium. It may be that two scientists are each in reflective equilibrium, but have come to conflicting conclusions because each has 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. That is, the overall epistemic situation will not improve, and perhaps cannot improve.

On the combination of assumptions in play here, there may be nothing to fault either scientist. Each has followed the methodology correctly and flawlessly, and so each displays no cognitive shortcoming—or so it seems. They just came to different conclusions using the same, fallible methodology. So, on the foregoing, admittedly simplified assumptions, Cognitive Command fails for science, or so it seems. Blameless disagreement *is* possible. There are a number of different philosophical conclusions one might draw in this case, with perhaps no clear winner.

First, we might conclude that science is not objective. There simply is no fact of the matter, independent of the intellectual lives of scientists, whether *P* or ¬*P* is true. A philosopher inclined this way can maintain epistemic constraint. Whatever scientific truths there are, we can come to know them. Neither William nor Karen have knowledge here, since there is nothing to know. On the matter of *P* or ¬*P*, Cognitive Command fails. The disagreement is indeed (cognitively) blameless.

This flies in the face of a strong intuition that science is objective. There is a physical world, not of our making, that our two scientists, William and Karen, are trying to describe. At least one of them has gotten it wrong. Our second option is to maintain this intuition, and claim that epistemic constraint fails for science (if the scenario in question is possible). We just do not know, and in light of the standoff, cannot know which (if either) of our two scientists is right. So the truth in question is unknowable. This would sustain Wright's suggestion, noted above, that his three criteria (width of cosmological role, Cognitive Command, the Euthyphro contrast) apply only if the discourse is epistemically constrained. In this case, Cognitive Command fails, but that is irrelevant to the matter of objectivity, since the discourse is epistemically unconstrained.

The situation is not completely straightforward. There is a third option, which also enjoys some plausibility. One might think that epistemic constraint has not been ruled out by our scenario of blameless disagreement. Suppose, for example, that it is our second scientist, Karen, who is in fact correct: ¬*P* is true. One might argue that Karen does know ¬*P*. By hypothesis, ¬*P* is true. Moreover, Karen believes ¬*P*, and this belief was arrived at by flawlessly following the best scientific procedure available. What more does it take for (albeit fallible) knowledge? So on this third option, we maintain the intuition that science is objective, that epistemic constraint holds, and that blameless disagreement is nevertheless possible. On this, third option, Cognitive Command is not a completely reliable indicator of objectivity, contra one of the themes of Wright [1992].

One yet another hand, if Karen does know ¬*P*, then perhaps the other scientist in the scenario, William, does exhibit a cognitive shortcoming after all. After all, he believes an objective falsehood, *P*, and, by hypothesis, ¬*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. William was simply unlucky. Should this bad luck count as an cognitive shortcoming, as a blameworthy disagreement?[7]

We have broached some delicate issues in epistemology here, and issues concerning what counts as "cognitive". These matters go well beyond the scope of this paper, not to mention my own competence. I propose to move the focus to mathematics without resolving this issue.

It is surely possible for one mathematician to *conjecture* that a certain proposition *S* about 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

[7]On still another hand, note that in the scenario in question, Karen just got lucky. One might think that this disqualifies her from knowing ¬*P*. If she were confronted with William's theory, and realized that it is just as good as hers on all relevant epistemic grounds, she should retract any claim to know ¬*P*. This line of reasoning supports our second option, on which epistemic constraint fails for science. Shapiro and Taschek [1996] argue that, in general, epistemic constraint entails Cognitive Command.

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.

In mathematics, the epistemic standard for serious assertion is *proof*. So suppose that one mathematician, Pat, has produced what she takes to be a proof Π of a mathematical proposition *S*; and that another mathematician, Karl, continues to demur from *S*. The question at hand is whether we can be sure, a priori, that at least one of them exhibits a cognitive shortcoming (assuming that the disagreement is genuine)?

To be sure, this sort of disagreement happens all the time among professional mathematicians. 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 (or 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. To give epistemic constraint a chance at being correct, we must consider the epistemic states of highly idealized versions of mathematicians. So let us assume that the proof Π of *S* is fully formalized. All of the axioms and premises are explicit, and every step is an instance of a primitive rule of inference. The (purported) proof may be far too long for Pat or Karl to parse, but this is irrelevant. The appropriate protagonists to the debate, call them Pat* and Karl*, are idealized versions of their human counterparts. Pat* accepts the proof and Karl* demurs.

We can assume that they 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. So either they 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.

Prima facie, a disagreement over a premise is not really a disagreement. The two mathematicians are just talking past one another. Pat* is working on 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 like the parallel postulate, is not in real disagreement with a Euclidean. They work in different theories.

Of course, it was not always this way. Mathematicians and physicists saw the issue concerning geometry as concerning the structure of (physical) space. Alberto Coffa [1986, 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. Michael Resnik [1997] calls this a "Euclidean rescue".

Things may not be this neat if the disagreement concerns a more foundational matter, one that relates to the relationship between various mathematical structures, or the semantic relationship between premises and conclusion. For example, suppose that Pat*'s (and Pat's) proof invokes V=L, and that Karl* (and Karl) rejects that.

It may be that matters like this 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 parties 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 blameless.

Here we broach the options noted above, concerning the underdetermination of theory. Here, of course, there less pre-theoretic agreement that our subject matter is objective. Intuitions diverge more widely than in the case of science. Is there a fact of the matter concerning V=L, the disputed item?

Suppose that there is an objective fact of the matter concerning the disputed axiom, V=L. Then one of our idealized mathematicians, Pat* or Karl*, is mistaken. And yet, by hypothesis, both of them display no epistemic fault in the process. The best conclusion, one would think, 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. Then Cognitive Command fails, since the other idealized mathematician has no cognitive shortcoming (unless simply getting something wrong counts as a cognitive shortcoming). On the plausibility of all of the assumptions in play now, there may be some room for mathematics to fail one of Wright's tests. But note that we get this result only if we concede that there is an objective fact of the matter concerning the disputed item, V=L in this case. As in the scientific case, the fault would lie with the criterion of Cognitive Command, as a reliable test of objectivity.

Suppose now, that there is no objective fact of the matter concerning the disputed foundational item, V=L in this case. Then, once again, there is no real dispute between Pat* and Karl*. They just work with different premises. 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. In this case, we rule out blameless disagreement by denying that there is disagreement. The mathematicians are just talking past one another, on different subject. Pat* and Karl* agree that the "disputed" sentence *S* follows from V=L, and things are left like that. There is a change of subject.

It seems to me that the only way one can resist this conclusion is to maintain that Pat* (and Pat) and Karl* (and Karl) are not at cross purposes, since they work with the same concepts. There is no change of subject. Yet, the disputed item is genuinely indeterminate for those concepts. There is only one concept of "set". It is not ambiguous, nor does it bifurcate into ZFC+V=L and, say, ZFC plus some large cardinal assumptions (incompatible with V=L). The opponent of objectivity must maintain that the matter of V=L is indeterminate for that very concept.[8] I confess to having some trouble with this option, but this may be due to my structuralist leanings (or prejudices). I leave it to the reader to determine how plausible it is.

Recall that the other potential area for disagreement between Pat* and Karl* concerns the validity of one of the primitive rules of inference in the purported proof Π of the disputed proposition *S*. That is, they disagree over the logic. Say that Pat advocates classical logic, while Karl is an intuitionist.

I have written elsewhere on the objectivity of logic (Shapiro [2000, §5.4]). I'll briefly summarize the relevant parts here. Notice, first, that in the case under study, Pat* and Karl* accuse each other of ***inferential error***. And inferential error is one of the explicit items listed as a "cognitive shortcoming" in Wright's formulation of Cognitive Command. So each of them will hold that the other has a cognitive shortcoming, and each of these claims is fully justified from its own perspective. In other words, it is close to an analytic truth that, on the letter of Wright's criterion, a difference in logic entails a cognitive shortcoming in one of the parties.

Perhaps this little argument turns too closely on the exact wording of Wright's criterion of Cognitive Command. Presumably, it would not do to argue that ethics is objective by formulating a

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[8] I am indebted to Crispin Wright here.

version of Cognitive Command that includes "errors in value judgement" as a cognitive shortcoming.[9] In the case at hand, we would need a reason to count inferential differences as symptomatic of cognitive shortcoming on the part of one or the other party.

Later in [1992], Wright qualifies the constraint, noting certain exceptions to when a disagreement turns on a cognitive failing. A discourse exerts Cognitive Command just in case it

> . . . is a priori that differences of opinion formulated within the discourse, unless excusable as a result of vagueness in a disputed statement, or in the standards of acceptability, or variation in personal evidence thresholds, so to speak, will involve something which may properly be described as a cognitive shortcoming.

One might propose adding logic to this list of exceptions, so that differences of opinion "excusable as a result of different logics" do not entail a cognitive shortcoming.

The idea here is that the criterion of Cognitive Command can be applied only against the background of a shared logic. The inclusion of "inferential error" in the list of cognitive shortcomings would be restricted to errors within a logic common to the disputants.

The effect of this proposal does not support the non-objectivity of mathematics via logic. We do not have a failure of Cognitive Command here. Rather, the proposal removes logic from the arena in which Cognitive Command operates. Logic is declared to be part of the framework for determining whether Cognitive Command holds, rather than subject to the constraint itself. And given that we are idealizing so much anyway, we can assume that our disputants, Pat* and Karl* do not make inferential errors in their own logics. So we can safely leave the topic of logic, conceding that this might be another area in which some sort of blameless disagreement is possible, but adding that this does not bear on the objectivity of mathematics.

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[9] I am indebted to Jill Dieterle, Christopher Peacocke, and Neil Tennant.

Before we move on, note that the topic here is not a parochial matter concerning logic and mathematics alone. Given how pervasive logic and mathematics is, disagreements about logic are likely to result in disagreements elsewhere. In Wright's terms, logic and mathematics are not "disputationally pure" (pp. 155-156). If 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. For example, Pat* will agree that a version of the intermediate value theorem holds for space-time and Karl* will not. But, on the proposal under study here, this difference is excusable due to the difference in logic.

4 Response-dependence: the Euthyphro contrast. We turn, finally, to the question of whether mathematics is response-dependent or, better, judgement-dependent. By definition, in 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 want. Against this, Socrates argued that (at best) the gods have the ability to *detect* piety. The contrast between Socrates and Euthyphro would remain if the gods were replaced with actual human beings, or ideal agents acting under ideal conditions—even if the opinions of these agents were infallible. Socrates's view here is that piety is objective. As far as it goes, Euthyphro's perspective is consistent with piety being subjective or otherwise judgement-dependent, in which case the opinion or judgements of the gods is what *constitutes* piety. The Appendix to Wright [1992, Chapter 3] lays out constraints on a Euthyphro contrast for discourses concerning color, shape, morality, modality, etc. Our concern here, of course, is with mathematics.

John Divers and Alexander Miller [1999] argue that arithmetic, at least, is judgement-dependent.[10] 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 if any part of mathematics is judgement-dependent, then this very elementary fragment of arithmetic is. Taking the contrapositive, if this part of mathematics fails to be judgement-dependent, contra Divers and Miller, then the rest will come along.

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 show that (the relevant part of) arithmetic discourse has the relevant features of color discourse and first-person intention discourse, and they show 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 sufficient account of the idealizations needed to maintain epistemic constraint in arithmetic. In particular, 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:

For all *S*, *P*: *P* if and only if (if *CS* 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, 108-109)

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

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[10]This is the cornerstone of their response to a challenge from 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.

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 for response, *CS*, might alter the truth-value of the proposition in question. Suppose, for example, that there was a yellow object in a relatively dark room, one not optimal for viewing color. Due to its chemical composition, the object would turn blue if 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 dark 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. Once a basic or provisional equation is established, we can go on to articulate the further conditions for response-dependence, or judgement-dependence.

Divers and Miller [1999, 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 or 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.

A second reason to move to provisional equations is that 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 die, or become color blind, if he were to view an object under the proper lighting, due to some feature of his optic nerve. In the arithmetic case at hand, this consideration translates to the issue of how "human" our subjects remain, once we idealize in the needed way. The case to follow turns on just this matter. 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 §1 above).

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's view is that a discourse for which a basic equation is true can be construed as judgement-dependent, and thus as not objective, if a prioricity, substantiality, independence, and extremal conditions concerning the formulation of the conditions *C* on proper judgement obtain. I'll summarize three of these.[11]

First, 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). This is the *a prioricity constraint*.

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)

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[11]The fourth item is the independence constraint, which is an extension of the substantiality constraint. As far as I can tell, it has no bearing on the present argument.

This is the *substantiality* constraint.

Suppose 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.

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], 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 including the concepts of addition, multiplication, and quantification. 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; "Frege's favorite number", or "the least counterexample to the Goldbach conjecture, if there are any" do not count.

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 will accurately judge that 5 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, not to mention the numbers alluded to above that have over 100,000 digits? For all but a few numbers, even expert mathematicians must resort to calculation.

So far, it has not been built into the specification of the *C*-conditions that the subjects do not make mistakes 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 three or four 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, say. This is not enough. 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" (unless they get external help, from a calculator).

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 pencil and paper. 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—to the extent that they are reliable. 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 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, in §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.

Divers and Miller themselves raise a related, perhaps identical 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 wold 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 applies even if the number is given in canonical notation, provided 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 §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 is to specify the idealizations are that the judges have no limits on attention span, memory, and lifetime. If they use external materials, like paper and pencil, they never run out of them. To avoid issues concerning physical properties of paper, let us just give our ideal judges unlimited, stable memory and retrieval.

Will this 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 proofs. 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 four digit 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 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 judgement-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 (see §1 above):

> 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)

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 now true, on a priori grounds alone. I concede that the substantiality constraint is now met. To paraphrase Wright [1992, 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.

But now I submit that something in the neighborhood of the extremal condition is violated. In effect, we have turned our ideal mathematicians into abstract calculating devices, like Turing machines. That is, I our ideal mathematicians are themselves mathematical objects, and the basic equation is itself a piece of mathematics. In effect, the basic equation is of-a-piece with the 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, 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 "decidable". The "responses" of Turing machines match up perfectly with the facts. This is true by definition, or by theorem. 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 "judgement" at the end.

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