MEDITATIONS ON SECOND PHILOSOPHY:
ANTI-NOMINALIST REFLECTIONS ON MADDY'S SEMI-NOMINALISM

INTRODUCTION

Over the years, Penelope Maddy's philosophical writing has given as much attention to questions of mathematical ontology as to any other single subject. Constant throughout has been a concern with the bearing, or as the case may be, the lack of bearing, of this or that ontological view on the project of seeking new set-theoretic axioms to settle questions left open by the currently accepted ones. Constant also has been a commitment to naturalism, first in a broad, general sense, later in a narrower, more specific form as she developed what she calls *second philosophy*. Against this constant background there have been substantial changes, since she has always been ready to give up a thesis if, having given it a run for its money, she in the end finds it unsatisfactory.

All told she has given sympathetic attention to at least four ontological positions. If one compares the question of the existence of sets to that of the existence of a deity, the four doctrines might be roughly likened to *theism*, *deism*, *atheism*, and *agnosticism*. To the first three she gives names: *robust realism* and *thin realism* and *arealism*. The fourth position is that, though the first view is definitely wrong, there is nothing to choose between the second and the third, which have an equal bearing or lack of bearing on the new axioms project, and an equal claim to acceptability from her kind of naturalist standpoint. The first and second views are plainly varieties of *anti-nominalism* and the third a variety of *nominalism*, and so the fourth view, which Maddy leaves nameless, I will call *semi-nominalism*.

The first, robust or thick realist view, the one the semi-nominalist takes to be definitely wrong, was defended by Maddy in her Princeton dissertation and her first book (1990). It was developed under the influence of the well-known passage where Gödel (1947) writes of our having something like a perception of the objects of set theory. The commitment to naturalism at this stage amounted to an attempt to develop this thought in a way fully in accord with cognitive science and without any trace of parapsychology. Gödel's discussion famously suggests how intrinsic or extrinsic grounds might be found for adopting new axioms, especially large cardinal axioms. Maddy undertook her own examination (1988) of many post-Gödelian heuristic arguments in support of such axioms, but the ultimate outcome was her abandonment of her original ontological position, partly on account of what she saw as a mismatch between the character of these arguments and the kind of realist metaphysics she had been endorsing.

Her change of view was also closely connected with the direction the development of her naturalism took through her second book (1997), her comparative study (2007a), and her third book (2007b), as she articulated second philosophy. All four ontological views are discussed in her well-known paper on mathematical existence (Maddy 2005), and further elaborated in her fourth book (Maddy 2011). Between the paper and the book there is a change of emphasis in the formulation of the fourth view. The claim that it is not merely the case that thin or anemic realism on one side and arealism on the other are equally defensible, but further that the difference between them is merely verbal, comes much more to the fore in the later treatment, which for present purposes I will take to represent Maddy's final position. One might expect a philosopher who has arrived at such a position to conclude that, though much has been learned through pursuing the ontological question, it is time to deposit that debate in the archives and turn to other matters; and turn to other matters is indeed what Maddy has largely done in her later books and any number of her later papers.

ROUTLEYISM

I myself have also come to believe time should be called on the debate over nominalism, though my reasons are rather different from any considerations put forward by Maddy. Nominalism seems to me to have degenerated sadly since the period when Charles Chihara, Hartry Field, Geoffrey Hellman, and others came forward with ambitious programs of reconstrual or reconstruction of mathematics or mathematically-formulated science, with technical aspects of interest to the logician. These are what drew me as into the on-going debates, to address Chihara and Field in (Burgess 1983) and, with my colleague Gideon Rosen as coauthor, Hellman as well in (Burgess and Rosen 1997). Nominalism's present-day advocates, in contrast to those just mentioned, seem mostly to embrace cheaper and easier, quicker and dirtier routes to the conclusion that numbers, functions, sets, and such do not exist. Nowadays one may encounter a lazy instrumentalism of a kind earlier nominalists scorned, one that speaks as if mathematics were true while doing science, speaks as if mathematics were false while doing philosophy, and makes no real effort to explain or excuse the flip-flop.

And one often encounters claims to discern a distinction in meaning between a pair of synonymous expressions, or what many of us would take to be such, followed by exploitation of the supposed distinction to justify a kind of doublespeak that makes ontologically liberal-sounding noises out of one side of the mouth, and ontologically conservative-sounding noises out of the other. The most egregious example of this latter tendency, the cheapest and easiest, the quickest and dirtiest of all, dates from the last century and Richard Routley's *noneism*, which David Lewis (1990) remarked might with equal justice have been called *allism*. Speaking with a forked tongue, Routley says *yes, there are numbers and functions and sets*, or something of the sort, but also *no, there do not exist any numbers or functions or sets.*

It is a peculiarity of Routley's view that he makes no attempt to locate a sense of being or existence that would apply to the Cantor set but not the Russell set, or to the equilateral triangle but not the round square. This peculiarity, usually called *Meinongianism*, will not concern me here, but rather the general feature of seeing a distinction in meaning between "there exists" and some other expression that in ordinary usage is synonymous with it, or viewed by many of us as being so, which contrasting expression is claimed to mean something less, or weaker. If I call a view *Routleyist*, it is this last feature I will be attributing to it.

One form of Routleyism sees a distinction, not between the existence and the being of abstract mathematical objects, but between their actual and their possible existence. Now we *do* sometimes speak of possible existence in mathematics. We may say that it is possible that there is a largest twin prime and possible that there isn't. But the possibilities we speak of in such cases are merely *epistemic* possibilities, ways that *for all we know* things may actually be.

The grounds for the assertion are that it has been proved neither that there is no largest twin prime, nor that there is one. The assertion is not that there are two alternate mathematical realities, in one of which the sequence of twin primes comes to an end, and in the other of which it doesn't. The kind of more-than-epistemic possibilities involved in an alternate realities claim, possibilities understood as (the way things are plus all the) ways things actually aren't but potentially could have been, is called *dynamic* by linguists and *metaphysical* by philosophers. It is what is involved in Routleyist modal nominalism.

The same kind of modality may be said to have been involved in the projects of Chihara and Hellman, but for them the distinction between actual and possible existence was applied only to concrete objects, not abstract ones. The project was to replace standard assumptions about the actual existence of mathematical objects with suitable assumptions about the possible existence of physical objects. Such a project required considerable effort, while the Routleyist modal nominalist project is effortless. One simply replaces standard assumptions about the actual existence of mathematical objects with the corresponding assumptions about the possible existence of those very same mathematical objects. I myself do not find in our ordinary usage any serious, sustained invocation of any non-epistemic kind of modal distinction in application to purely mathematical facts about purely mathematical objects; and finding the distinction without use, I find it without meaning.

Lewis's theme in his discussion of Routley is that productive debate over noneism or allism is virtually impossible, and I would say the same about later Routleyist views, and about instrumentalist flip-flopping, and about the quasi-Routleyist claim to detect a difference in meaning between *existential consequence* and W. V. Quine's pretentiously inflated synonymous coinage *ontological commitment*. For discussion of claims of this kind see Rosen (2006). In conversation Rosen has gone so far as to characterize them as “muddying waters previously clear.” I now find the waters so muddy I do not want to step into them. But before pulling back from the water's edge, I want to put on record what are for me the most important points on which I continue to differ from Maddy. I hope I do not need to say that such differences occur against a background of massive agreement.

NATURALISM AND AREALISM

Maddy contrasts her form of naturalism with others, and perhaps to begin with I should say something about the contrast between the flavor of naturalism involved in Maddy's second philosophy and that involved in my joint work with Rosen, from which she quotes (Maddy 2011: 39n) the following snippet (Burgess and Rosen 1997:69):

> The naturalists' commitment is at most to the comparatively modest proposition that when science speaks with a firm and unified voice, the philosopher is either obliged to accept its conclusions or to offer what are recognizably scientific reasons for resisting them.

Maddy says, by contrast, of her second philosopher:

> My inquirer doesn't decide to place her faith in something called 'science'; she is simply one of those speaking with a firm voice, on the basis of the evidence.

There need be no mystery about what this "thing called 'science'" is: For present purposes we may take it to be that which is done university science departments, or rather, in university *natural* science departments. For we may set aside the so-called *social* sciences, which perhaps don't very often speak with a firm and unified voice, while we must set aside the so-called mathematical sciences, since counting them as among the sciences whose firm and unified voice the naturalist is not to gainsay would be considered question-begging by nominalists. The shift in Maddy's conception of naturalism from the mathematical naturalism of her second book to the second philosophy in her latest work, with the concurrent drift in the direction of nominalism, was in substantial part a matter of decreasing willingness to count mathematics as a member in good standing of the circle of sciences with the same rights as physics and the other members.

For me — I believe Rosen and I still after nearly two decades agree, but I won't presume to speak for him — a naturalist will, where there exists a stable scientific consensus, generally fall in with or acquiesce in it, unless in possession of some special information or expertise. In the discussion of nominalism, what needs emphasis is the passive or negative side of this acquiescence, non-dissent, the renunciation of any claim to have some higher philosophical standpoint from which to supply a critique of science. The active or positive side, assent, comes to fore in other contexts. For instance, in deciding whether to have their children vaccinated, naturalists of my kind would let themselves be guided by conventional science-based medicine rather than alternative who-knows-what-based medicine.

Bierce (1993, originally published 1911) defines "faith" as "belief without evidence in what is told by one who speaks without knowledge, of things without parallel." Is my naturalist's reliance and trust in conventional medicine "faith" in some such pejorative sense? Well, in renouncing the idea of an external philosophical critique of science one is also renouncing the idea of an external philosophical foundation for science. But this renunciation does not in itself amount to holding that there is *nothing* that can be said in favor of science and that we must simply make a leap of faith and accept it. There is a good deal that can be said in favor of science, but it is the sort of thing said by popularizers of science and debunkers of pseudoscience, not something distinctively philosophical.

By contrast, the passage quoted above is one of several that tend to make Maddy's second philosopher appear to be a kind of Wonder Woman, who never needs to rely on or trust in the expertise of others, and therefore never needs to decide which others to rely on and trust in, and so never needs to consider whether the possession of institutional scientific credentials should be taken into account in making such decisions about reliance and trust. Rather, the second philosopher has the energy and resources to investigate every issue for herself, and form her own opinion based on her own evaluation of the evidence, not anyone else's (except that at one point, faced with a question about chimpanzee nutrition, the second philosopher does seek outside advice; but why she on this occasion has faith in the opinion of a scientific botanist rather than a New Age herbalist we are not told). Maddy does, however, seem to expect the second philosopher's opinions will largely recapitulate those of actually existing natural science. The second philosopher presumably knows and feels qualified to judge the technical evidence, clinical and epidemiological, bearing on the safety and effectiveness of vaccines against childhood infectious diseases; whereas my kind of naturalist may well form an opinion based on pamphlets for the lay public from the usual professional medical sources, supplemented perhaps with journalistic accounts of recent controversies by debunkers of anti-vaxxer conspiracy theories.

Such differences in flavor of naturalism, however, are perhaps not the main source of differences on ontology. Maddy arrives at semi-nominalism by judging that, though the second philosopher is not required to reject mathematical existence claims, such rejection is fully compatible with her kind of naturalism. My kind of naturalism, with its acceptance of whatever stable consensus is to be found in physics, I believe to be, by contrast, incompatible with nominalism, owing to the heavy involvement of physics with mathematics. And the difference here between claims of incompatibility and claims of compatibility is less a matter of differences in flavors of naturalism, than of differences in conception of the role of mathematics in science, especially physics.

Indeed, I can largely confine my discussion of differences with Maddy to examination of one short passage where I find the crux of the issue between us to lie, a passage in which Maddy enunciates her arealist's understanding of the role of mathematics in empirical science. It consists of just two consecutive sentences from her discussion of arealism in the fourth chapter of her fourth book. Concerning what her second philosopher finds when she examines the practice of the application of mathematics in science, Maddy focuses on the role of mathematics as a source of models, and writes as follows:

(1) In all this, the scientist never asserts the existence of the abstract model; he merely holds that the world is like the model in some respects, not in others.

(2) For this, the model need only be well-described, just as one might illuminate a given social situation by comparing it to an imaginary … one, marking the similarities and dissimilarities.

(Maddy 2011: 90, minor typos corrected)

These are the views I especially want to examine. But first a matter of terminology. Gauss remarks somewhere that a lot of confusion would have been avoided if instead of saying *positive, negative, imaginary* mathematicians had said *forward, backward, sideways*; but despite his remark, *real* versus *imaginary* numbers remains the standard terminology. To avoid a clash with this technical mathematical usage I will, therefore, when discussing such claims as (1) and (2), write of the *fictitious* where Maddy writes of the *imaginary*, and use *genuine* rather than *real* as the contrasting term.

MATHEMATICAL EXISTENCE THEOREMS IN PHYSICS

The epistemological question nominalists so often press is that of how anyone could arrive at a justified belief that there are abstract mathematical objects. If physicists believe in the existence of abstract mathematical models, then there is a very easy answer to this question for those who accept scientific standards of justification and do not suppose these can be trumped by some higher philosophical standards. The answer is this: Just look at how, historically, physicists arrived where they are today, and *that* is how one can justifiably arrive at belief that are such abstract objects as mathematical models. Hence the significance of the question whether physicists do in fact believe in the existence of mathematical models, or whether on the contrary, as (1) suggests, they don't. Is (1) factually correct or not?

In a perceptive early review of Field's project by a distinguished philosopher of physics, various mathematical results important in physics that Field's approach fails to cover are enumerated, and these notably include "propositions that establish the existence of models with special features" (Malament 1982, page 528, item B). We seem to have here an assertion from one who can be presumed to know his business that physicists do discuss, and sometimes assert, the existence of models. But the remark just quoted is not, of course, addressed directly to (1), which first appeared in print more than two decades later. So though taking a hint from the review cited, I decided that rather than rely on the authority of the reviewer in addressing the status of (1), I should proceed more directly, and see if I could find quotations from physicists asserting the existence of models.

Now as Maddy brings out in a concise survey of the history of relations between physics and mathematics in the first chapter of the book under discussion, the paradigmatic cases of mathematical modeling of physical situations involve the framing of differential equations. The equations are sometimes spoken of as if they themselves were the models, but properly speaking it is *solutions* to the equations that are the models, where a single system of ordinary or partial differential equations may have many solutions with different special features, all of physical interest. The solutions to differential equations are, of course, functions, real- or complex-valued, of one or of several variables, and as such paradigmatic cases of abstract mathematical objects, in modern set-theoretic mathematics identified with certain sets. So our question becomes: do physicists assert the existence of solutions to differential equations?

It was solutions to the Klein-Gordon equation $\Box\psi = 0$ that were at issue in the review cited earlier, but I expected it would be easier to find quotations from physicists dealing with the field equations of general relativity. I expected it would be easy to find physicists asserting that Gödel (1949) proved that there exist, or that there are, solutions with closed time-like curves, or at least that Gödel found or discovered such solutions, which implies there are such solutions, since one cannot find or discover what isn't there. But while I had no trouble accumulating examples of assertions of this kind, in every case I located the writer was one whom a captious nominalist might accuse of being a mere philosopher or mere logician or mere mathematician, and not a genuine physicist; or if a genuine physicist, not a genuine physicist engaged in genuine physics, but rather in some kind of popularizing, or worse, philosophizing.

Setting the specific Gödel example aside, however, I did quickly locate the sort of thing I was looking for. Princeton professor R. H. Dicke (1916-1997) was without cavil a physicist: an experimentalist and inventor of experimental apparatus as well as a theorist, and an astrophysicist and cosmologist as well as a gravitational and an atomic physicist. He was a physicist of high standing to boot, winner of the National Medal of Science among other awards. The *Astrophysical Journal* (ApJ) is unequivocally a technical scientific periodical publishing the results of technical scientific research, not the musings of professional scientists pursuing a philosophical hobby after work or after retirement. And I quickly found Dicke, in an ApJ paper on the cosmic background radiation, writing as follows: "From the above discussion it is evident that there are solutions to the field equations for which…" where then follows a technical enunciation of certain special features that need not detain us (Dicke 1968: 12). Googling will turn up any number of further examples. And so, I submit, if Maddy's arealist hasn't found scientists asserting the existence of abstract mathematical models, she hasn't read widely enough.

It may be suggested at this point that though a physicist may *assert* that solution exist, he doesn't really *believe* it. After all, Maddy has remarked that "the attitudes of scientists toward their best theories are complex and nuanced: some posits are regarded as useful fictions, some aspects are suspected to be artifacts of the modeling, and explicit idealizations are freely employed" (Maddy 2005: 363). And this is very true if it is taken to mean that when a mathematical model of a physical situation is proposed, the physicist seldom if ever supposes that every item in the mathematical model corresponds to some item in the physical situation, or that those that do correspond do so exactly. But it is one thing to say that nothing physical corresponds exactly to such-and-such items in the mathematical model, and quite another to say that the mathematical model itself does not exist, and that whatever mathematical existence theorems say it does are falsehoods, and that whatever mathematicians believe such theorems are deluded. Do physicists ever, in the course of doing physics, say anything like the latter? If so, let the arealist produce a quotation to counter the one I have produced from Dicke.

It may be suggested instead that though physicists *do* assert and believe that functions constituting solutions to certain differential equations exist, they may, not having taken to heart the lessons of Frege (especially 1904), suppose the functions to be mathematical formulas, and as such not set-theoretic but linguistic entities. This does not make very much difference to the issue of nominalism if linguistic entities are just another species of abstract object beyond numbers, functions, and sets. But it may be further suggested that physicists suppose linguistic expressions to be, not abstract types, but concrete tokens, physical items made of chalk or the like. Solutions to the field equations would be bits of calcium carbonate.

To be sure, physicists say many things that contradict such an understanding: for instance, that two researchers on opposite sides of the world have produced the *same* solution to certain equations, when they certainly have not produced the same chalk marks on the same blackboard. Do physicists simply hold contradictory beliefs here? One cannot refute the suggestion that they do by eliciting explicit statements from physicists through questioning, since whatever they say when questioned may be dismissed on the grounds that when addressing philosophers' questions they are not doing physics but philosophy. There may be no finally refuting such a suggestion at all, but I do not believe for a moment that this is what Maddy had in mind with the formulation (1), or that she would try to defend her formulation with such desperate suggestions. As for myself, I would modify or amplify my characterization of naturalism by making it clear that the naturalist is not limited to parroting things said by scientists, but may also draw out consequences that obviously follow if their statements are accepted, and presuppositions that must be accepted if the statements are to make coherent sense.

FICTIONS IN SOCIAL SCIENCE

All this, however, may be considered but a preliminary skirmish. For even if scientists do as a matter of actual fact assert and not deny the existence of abstract mathematical models, contrary to (1), still if (2) is correct they *need* not do so, and that may well be enough for arealists' nominalistic purposes, at least as some of them view matters. Here a difference in flavor of naturalism may be important, since for my kind of naturalist what matters is how the scientific community actually does things, not how it potentially could have done them. The claim is that our way of doing things is all right, not that it is uniquely right, that doing things any other way would not have been all right.

Maddy and I both reject so-called indispensability arguments, but for different reasons. For Maddy it is, to put it very briefly, because indispensability arguments portray mathematics as more like or of a piece with physics than it is. For me, it is because indispensability arguments concede too much to nominalism. For me, the form of skepticism known as nominalism is to be repudiated if it turns out, as I have claimed in the preceding section it does, that the assumption of the existence of mathematical models is customary in our way of doing science; it need not be shown to be indispensably necessary.

Be all that as it may, (2) with its comparison between social and mathematical situations merits separate and close consideration. Literary writers present us with many examples not merely of fictitious social situations but even of whole fictitious societies, Swift's Lilliput and Brobdingnag being perhaps the best-known. Let me begin with the role of these fantasy lands in social thought before returning to mathematics and its role in physics. Three points stand out.

First, many issues about our society can indeed be illuminated by comparison with another society whether that other society is a genuine or a fictitious one. This may include virtually all theoretical or descriptive questions. For instance, our tendency to hold people responsible for things beyond their control in some cases but not in others, a tendency that is the topic of a voluminous philosophical literature on so-called *moral luck*, can be thrown into relief by comparison with Samuel Butler's *Erewhon* (2002, originally published 1872), an account of a fictitious society where people are blamed and even imprisoned for falling ill, with no pleas for leniency allowed on grounds of having been brought up in an impoverished and insalubrious environment, whereas those found committing what among us would be considered serious felonies, including the host of the narrator in the novel, who perpetrates a major embezzlement, are not on that account thought badly of, though they do have to undergo treatment at the hands of so-called *straighteners* that may be almost as painful as some of the therapies of Victorian medicine.

But second, many comparisons between our society and another are illuminating only if the other society is a genuine and not a fictitious one. This may include virtually all practical or prescriptive questions. It is one thing to argue that it would be feasible to improve some of our practices regarding some matter or other on the grounds that they order the matter better in France, and quite another to argue that it would be feasible to improve those practices on the grounds that they order the matter better in Utopia. France genuinely exists and it might be feasible to imitate here what is done there. The same cannot be said of Utopia. When discussing the prospects for prison reform in the United States, comparison with France or other developed countries, which have drastically lower rates of incarceration, is appropriate. Comparison with Erewhon, where one can be jailed only for disease, is not.

And third, the question whether a given description is of a fictitious or a genuine society has always a clear sense. Consider, for instance, Margaret Mead's famous account of adolescence in the South Seas (Mead 1928), which became the subject of an academic controversy in the 1980s, a few years after Mead's death. Was the society depicted in her classic book that of the genuine Samoa of a hundred years or so ago, or was it, as Derek Freeman (1983) alleged, that of a fictitious Samoa described to a gullible twenty-something anthropology doctoral candidate by mischievous teen informants who were hoaxing her? The answer to the question is relevant to whether Mead's Samoa shows how we might hope to make the teenage years in our own society less a time of storm and stress than it generally is. My impression is that her profession has generally sided with Mead on this contentious question, but we who are not up on the iterature may not be in a position to give a confident opinion. For all that, we understand perfectly well what is at issue.

FICTIONS IN MATHEMATICS

When we turn from social to mathematical models, we must note that literature provides far fewer examples of fictitious mathematical objects or structures than of fictitious human societies, though it does provide some. One may for instance cite, in contrast to the genuine *surreal* numbers of the genuine mathematician John Conway (see Knuth 1974), the fictitious *supernatural* numbers of fictional character Noam Himmel (see Goldstein 1983). Or if the interest is not in fictitious species of numbers but fictitious members of genuine species, one may contrast the least counterexample to Merten's conjecture, a conjecture genuinely proved to have counterexamples by Odlyzko and te Riele, with the least counterexample to Fermat's theorem, a theorem genuinely proved to have no counterexamples by Andrew Wiles.

The Fermat example figures in a blog post of a well-known mathematical gadfly (Zeilberger 2012) that apparently took in some readers who did not notice that the date of the post was April first.

If examples of fictional mathematics are few, straightforward examples of the application of modeling by fictitious mathematical objects or structures are fewer. The supernaturals will never be useful as models since all the novel where they appear tells us is that a character's reputation as a genius was established by his introducing them, not what they are like. The April fools' day blog post makes a serious point about a *social* practice, contemporary editorial policies at mathematical journals, by invocation of a fictitious *social* situation, the embarrassing computational discovery of an error in what had been accepted as a major and prize-worthy conceptual proof. Other fictions about mathematics, from Swift's description of Laputa to the well-known play *Proof* (Auburn 2001) are also illuminating about various social and psychological issues; but that is not the kind of modeling we are now looking for.

So let us keep looking. There are, as is only too well known, many cases where lapses in rigor by physicists working on a mathematical model have the result that the physicists end up describing something mathematical that not only does not but cannot exist. And I suppose any such case can be described as a case of the physicist using a fictitious model. The most famous example, always cited in this connection, is Dirac's use of a supposed function δ that takes the value 0 everywhere but at 0, yet has an integral of 1 over any interval containing 0. Mathematics recognizes no such "δ-function," the features ascribed to it being inconsistent with each other. But Dirac's rigor was fairly promptly patched up by Laurent Schwartz. If this example is accepted, then eighteenth century mathematical physics provides no end of further examples. One may well wonder, however, whether in these cases the models can be properly called "well described" until the rigor has been cleaned up.

Prospects may be better for the future application of another kind of fictitious number. In very weak formal systems of arithmetic, where the principle of mathematical induction is not unrestrictedly assumed, it may not be provable that 10^x exists for every x, even though the conditional is provable that tells us that 10^{y+1} exists whenever 10^{y} exists. By a hundred applications of the conditional one can prove the existence of 10^{100} or *googol*, but it would take googol applications to prove the existence of 10^{googol}, or *googolplex*, and a proof of this size would not be feasible to write down, googol probably exceeding the number of elementary particles in the visible universe. If therefore we add to our weak arithmetic the axiom that googolplex does *not* exist, we obtain a system that is inconsistent but for which it is not feasible to exhibit an explicit derivation of a contradiction: there can be no genuine species of number satisfying the theory, though it is not feasible to prove this fact directly. There have been hints in the writings of mathematicians and logicians and philosophers from Alexander Yessenin-Volpin on (see the last paragraphs of Nelson 1986) that this sort of fictitious species of number may have applications.

The reader may at this point be impatiently muttering that the sort of cases I have been discussing are surely not what (2) is pointing towards, and this is quite correct: (2) seems to point to something cheaper and easier, quicker and dirtier, than exploitation of what are fictitious mathematical models with our present understanding of *fictitious*. What is being contemplated seems to be discarding such understanding as we currently have, an understanding on which the supernaturals are fictitious but the surreals are not, and declaring *all* numbers to be fictions, and all functions and sets along with them.

The distinction between the surreals and the supernaturals would presumably even then still have to be marked in some way, but it would have to be a different way from labeling one genuine and the other fictitious. As it happens there is precedent for distinguishing among items all deemed fictitious the genuine fictitious from the fictitious fictitious. What are in effect several cases of this kind are discussed in (Hayaki 2009). One striking instance, brought to my attention by Saul Kripke, is that of the many nouns in Biblical literature that have been understood to be the names of fictitious gods, which become the names of demons in later tradition down to Milton. Here there is a distinction between genuine fictitious gods, such as Chemosh or Dagon, whom archeology reveals to have been once the object of cults now long defunct, and cases like those of Belial in the Hebrew Bible or

Mammon in the New Testament, both high-ranking fallen angels in *Paradise Lost*, where the understanding of the noun involved as a proper name is generally regarded by scholars as a confusion. The kind of nominalism being contemplated would demote the supernaturals to the status of Belial, in order to have room to demote the surreals to the status of Chemosh, and more generally to declare all mathematical models used in physics, such as solutions to the field equations of general relativity, to be as fictitious as Dagon, if not quite as fictitious as Mammon.

But here we see a very sharp difference between the sociological and the mathematical cases contemplated in (2). If someone claims Mead's Samoa is fictitious, we know how its being so would contrast with its being genuine; and if someone claims Mead's Samoa is genuine, we know how its being so would contrast with its being fictitious. But is there an equally meaningful distinction of the kind the arealist professes to see, which is to say, a distinction between the assumption that a model genuinely exists, which is supposed to be superfluous, and the assumption that it is a *well-described* fiction, which is supposed to suffice for purposes of applications?

As the reader will doubtless have guessed, I find no more meaning in this supposed distinction than in that between Routley's supposedly ontologically loaded existential quantifier and his supposedly ontologically innocent particular quantifier, or between the actual and the metaphysically possible existence of pure numbers, functions, and sets, or between existential consequence and ontological commitment. The arealist, who seemed to me to be misstating the facts in (1), seems to me to be making a distinction without a difference in (2).

Such, then, are my differences with the arealist, whom I judge to be little if at all better than a Routleyist. Turning from the nominalist character to her semi-nominalist creator, my differences with Maddy herself might be thought to be exactly half as large. For Maddy may be described as in a sense doubting the meaningfulness of the arealist's and the thin realist's assertions and counter-assertions, and I may be described as in a sense sharing her view of the former, though not of the latter.

CONVENTION

But is this not, perhaps, overstating our differences? Am I not, in the very act of accusing the arealist of Routleyism, agreeing with the spirit, even if not the letter, of Maddy's claim that there is no more than a verbal difference between the nominalist's and the anti-nominalist's ways of describing our mathematical and scientific theorizing? Let me try to formulate the maximum concession I find myself able to make, so as to bring out what residual disagreement there may be.

Let us start a long way back from the issue of ultimate concern, right back at the first reification of numbers in the tradition leading to contemporary cosmopolitan mathematics. This world-historic occurrence may plausibly be dated to the transition from the use of numerals as adjectives, as in *six days*, to their use as nouns, as in *six is a perfect number*, which appears to have occurred sometime during the period between Pythagoras and Plato. It was attended, we are told by historians of logic, with a good deal of grammatical confusion (see Kneale and Kneale 1962: 392), and as we know it set off some metaphysical fireworks, speculations about the Monad and the Dyad and so forth. Strands of what we would accept as number theory and strands of what we would reject as numerology were almost inextricably intertwined. Centuries later there was still a lot of this sort of thing going on, as when Augustine ponders whether six is a perfect number because God created the world in six days, or whether God created the world in six days because six is a perfect number.

If things had never progressed beyond this stage, I think we can all agree there would have been no reason to consider mathematics a science. A naturalistic philosopher of any flavor would have no reason to make truth claims for the subject. Were we to try to produce an idealized representation of theorizing at this stage using first-order logical formalization, such science as there was would be codified in a theory with variables ranging only over concrete, physical objects. Such mathematics as there was would be codified in a theory with variables ranging only over abstract, mathematical objects. No scientific applications of mathematics would link the two, and a naturalist philosopher might regard the one as true, or probably largely approximately true, and the other as mythology.

If the claim that assumptions about the existence of solutions to differential equations are components of many areas of modern physical theory is accepted, as I have argued it should be, then science and mathematics cannot be separated in this way. At least one branch of mathematics, the theory of ordinary and partial differential equations, must be held to be true if a large part of physics is to be held to be true. But what about the branch of mathematics, number theory, we were just considering? As it happens, number theory, even after the mists of number mysticism dissipated, remained without many important scientific applications down through the period between the world wars. Though applications to cryptography lurked just over the horizon, the celebrated number theorist and pacifist G. H. Hardy was able to write at the end of interwar period (1940) that he took satisfaction in the fact that *his* kind of mathematics had no military applications. If number theory were still like that, would a naturalist philosopher today have any reason to regard it as a body of truths?

I think the answer may be affirmative, owing the applicability of existence theorems for ODEs and PDEs, together with the way in which modern mathematics is organized. Modern mathematics is, for reasons carefully discussed by Maddy in a late paper (2017), organized today in a framework of axiomatic set theory, where the axioms suffice both for the rigorous reconstruction of older useful theorems, and for the continuing deduction of new ones, as well as for the deduction of less applicable results, number-theoretic and other. If utility is taken to justify directly ascription of truth to theorems on the existence of solutions to differential equations, it must be regarded as justifying indirectly ascription of truth to the axioms from which spring an on-flowing stream of such theorems. And if truth is ascribed to the axioms, it must be ascribed to their logical consequences, to anything rigorously deducible from them, and hence to all theorems, even useless ones.

Be that as it may, a formal representation of our actually existent science would have to take account of the fact that it speaks both of physical and of mathematical objects. But it remains the case that the kind of questions it makes sense to ask about numbers and functions and so on are very different from those it makes sense to ask about physical objects.

To reuse a favorite example of mine, no cosmologist would consider the suggestion that it is numbers rather than neutrinos that are the bearers of the notorious "missing mass" a plausible or even an intelligible hypothesis. (Numbers' lack of mass is indeed one of the features that make them count as abstract, and make nominalists suspicious of them.) For this reason, were we seeking to model our mathematico-scientific theorizing using first-order formalization, it would be best to use a formalization in a *two-sorted* first-order theory, with one style of variable, say x, for concrete, physical objects, and a different sort, say ξ, for abstract, mathematical objects, rather than with a single style ranging over both. (This is indeed what is done in my joint work with Rosen.) For each predicate it would be specified which sort of variable is to go in each of its places. For instance, the two-place predicate _ *is the mass in grams of* _ would take a ξ in the first blank and an x in the second blank, because it makes sense to say a number is the mass (in specified units) of a body, but not that anything is the mass of a number, or that a body is the mass of anything.

But what of the quantifiers that go with two sorts of variables? These are governed by the same logical introduction and elimination rules. Should they also be represented by the same symbol? Or should we use two different symbols, perhaps $\exists$ to go with x and € to go with ξ? Well, because our custom in unformalized theorizing is to use the same word "exists" in both cases, use of the same symbol $\exists$ in the formalization would make the formal modeling of informal argumentation one point more faithful. By contrast, if we systematically used "exist" in one case, and another word, say "extrudes", in the other, and spoke, for instance, not of existence theorems but of extrusion theorems in the theory of differential equations, then the other notational option, distinguishing an existential quantifier $\exists$ from an extrusive quantifier €, would be one point more faithful instead.

My maximum concession to the semi-Routleyist would be to grant that if we had it all to do over again, our purposes might have been equally well served by the opposite to our actual usage, by our contrasting "exist" and "extrude" rather than making do with the single word "exists". One may say, if one wishes, that there is no *intrinsic* reason to prefer one option to the other. It is in this "could equally well have done otherwise" sense, the sense developed in

Lewis's dissertation and endorsed by his thesis director Quine (see Lewis 1969 and the foreword by Quine thereto), that our actual usage may be called *conventional*. Our actually existing natural science contains many conventional features present in our theorizing as the result of something like historical accident. (And that is why the second philosopher, if she really undertook to re-examine personally every scientific question *ab initio*, could *not* be expected to recapitulate exactly the course actually existing natural science has taken.)

But having conceded this much, I would insist that as a matter of actual fact we do *not* have it all to do over again: we are deep into on-going projects. And the fact that the "exists" rather than the "extrudes" usage has become our actual established usage is a powerful *extrinsic* reason to uphold it. Possession is nine-tenths of the law. Why make a linguistically disruptive switch if none of our purposes would be better served by doing so? The long and the short of it is that to call something conventional is not to criticize it. It was as the result of a conventional decision or historical accident that drivers in most of the world keep to the right. But to say so is not to say that we should switch over and drive on the left instead, or refrain from driving at all. There are reasons why less driving would be a good thing, but the conventionality of the choice of which side of the road to drive on is not among them.

More importantly, suppose we did use one quantifier for the concrete and another for the abstract, as we may use *someone* for persons, but *somewhere* for places. Even then our usage would not be Routleyist, since it would be lacking the central Routleyist feature of claiming that the expression used for the concrete is in some sense *stronger* than the the one used for the abstract. It is not, according to Routleyism, just that chairs and tables *exist* while number and functions *are*, as we may say battlefields *exist* but wars *occur*; rather the Routleyist claim is that concrete items not only *are* but also *exist*, while abstract items only *are*; or on a variant version, seats not only *possibly exist* but *actually exist*, whereas sets only *possibly exist*. If it is claimed that some items both *are* and *exist*, while others only *are*, how would things have looked different if the latter also existed, as the former do? This is a question to which I, as an anti-Routleyist, find no intelligible answer in Routleyist writings.

But the main difference between Maddy and myself I now take to lie in contrary attitudes towards conventionalism and conventional features of our theories. I simply don't see the recognition of a feature as conventional as warranting skepticism or agnosticism, as was perhaps already clear from my statement earlier of my reasons for rejecting indispensability arguments. The issue of conventionality at work here is further illustrated and illuminated by some other instances of appeal to abstract objects in scientific theorizing that I would now like to take up briefly before closing.

I have been writing up to this point as if the questions of the ontological status of numbers, of functions, and of sets were all of a piece, and though I do not think it affects the cogency of my discussion above, I must now acknowledge that Maddy has come to make distinctions here. In some of her later work (see Maddy 2014) she has turned from her special preoccupation with set theory to consider number theory as a distinct subject, and arrived at conclusions a point or two less skeptical. What she has not, to my knowledge, addressed at any length, is the question of the existence of *non*mathematical abstracta.

I am thinking especially of what I would call *equivalence types* of physical objects or events, though a variety of other terms have also been used in connection with them. Their equivalence type is what items that are equivalent in some respect thereby have in common, as figures that are geometrically similar have in common their *shape* and patches that are chromatically matching have in common their *color*. Talk of shapes and colors and the like of course long antedates the development of any sort of advanced science, and precedes the introduction of such phrases as *geometrically similar* or *chromatically matching*, for which the ordinary expressions are indeed simply *of the same shape* and *of the same color*. Such equivalence types will be with Maddy's second philosopher from the very beginning of her inquiries. But there have also been some significant cases of the historically late recognition of certain species of equivalence types in science.

For instance, the late nineteenth and early twentieth century saw the development in linguistics of the concept of *phoneme*, equivalence types of physical events of a certain kind, short segments of utterances, with respect to an equivalence relation whose behavioral characterization is somewhat differently explained by different authorities, and need not concern us here. The predicates it makes sense to apply to equivalence types correspond to those predicates of the items they are the types of that are invariant or preserved under substitution of equivalents for equivalents. In the case of phonemes, given two utterance segments exemplifying the same phoneme, the one will be sibilant if and only if the other is, and so a notion of sibilance can be applied to phonemes. By contrast, one of the two utterance events may take place in Helsinki and the other in Honolulu, and so the phoneme cannot be assigned a spatial location. Given a background of modern set theory, equivalence types can be identified with equivalence classes, or sets of equivalents, and so raise no additional ontological issues; but this identification is not open to nominalists.

There is no question but that talk of equivalence types could be systematically avoided, at the cost of introducing a certain prolixity into our discourse, in favor of talk of the things that are equivalence types of. Equivalence types *could* be dispensed with, but for me at least, that circumstance, or that plus the abstractness of types, is no reason why they *should* be dispensed with. And in actually existing science they are *not* dispensed with. For whatever may be the case with physicists and solutions to differential equations, linguists do unquestionably assert the existence of phonemes. They tell us, for instance, that among modern European languages Greek has few more than twenty, Lithuanian few less than sixty. For the nominalist, by contrast, both have zero, since for the nominalist there are no such things as phonemes. Nor are there genotypes or a whole wide range of other equivalence types recognized in science, including linguistic expression types as contrasted with tokens.

The case of equivalence types is one that especially invites consideration from the perspective of the particular variety of conventionalism associated with the name of Rudolf Carnap (see Carnap 1950). The theory of phonemes involves the introduction of a new way of speaking, what Carnap would call a *linguistic framework*. It should be admitted at once that there may very well be good reasons for resisting the innovation. One may object that the new notion is obscure because the relevant equivalence relation has not been sufficiently clearly characterized; and there is in fact a degree of such unclarity in the case of phonemes, which is one reason I was only able to cite approximate numbers for different languages just now. Or one may object that talking of phonemes will encourage unjustified assumptions about the existence of some neural correlate underlying the behavioral facts. This is something that may not have happened in the case of phonemes, but something like it did happen with unfortunate results in the case of the reification of a mathematical factor, the *g* that arises in factor analysis of IQ test results, as a *general intelligence* supposed to be a physical characteristic of, or even a fluid in, the brain (see Gould 1981).

According to the Carnapian, however, one reason for resistance to phonemes that is *not* legitimate would be doubt about whether there really are such things as phonemes. For on a Carnapian account it is the cluster of basic assumptions of the theory that constitute the linguistic conventions that guide the use and so constitute the meaning of the word *phoneme*. If one does not accept those assumptions the word remains meaningless and the question whether phonemes exist cannot even be enunciated; while if one does accept those assumptions, they trivially imply an immediate affirmative answer to the existence question. In this sense the theoretical question whether phonemes really exist is in principle meaningless: Asking in the material mode whether phonemes really exist may in practice serve as a way of putting what, expressed in the formal mode, would be the meaningful practical question whether phoneme theory should be accepted; but it is a misleading way.

Such is the Carnapian view, which I share. And what makes the status of phonemes something of which I think Maddy owes us an account is the fact that one constant through her various changes of ontological view has been a rejection or ignoring of Carnapianism. I am curious as to how, then, her second philosopher would regard phonemes and phones in phonology, genotypes and phenotypes in genetics, and so on, and I hope Maddy may sooner or later find time and occasion to address such questions. I anticipate that I will not agree with what she has to say, but I am confident it will be interesting.

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