Non-Standard Models in a Broader Perspective

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Abstract. Non-standard models were introduced by Skolem, first for set theory, then for Peano arithmetic. In the former, Skolem found support for an anti-realist view of absolutely uncountable sets. But in the latter he saw evidence for the impossibility of capturing the intended interpretation by purely deductive methods. In the history of mathematics the concept of a non-standard model is new. An analysis of some major innovations—the discovery of irrationals, the use of negative and complex numbers, the modern concept of function, and non-Euclidean geometry—reveals them as essentially different from the introduction of non-standard models. Yet, non-Euclidean geometry, which is discussed at some length, is relevant to the present concern; for it raises the issue of *intended interpretation*. The standard model of natural numbers is the best candidate for an intended interpretation that cannot be captured by a deductive system. Next, I suggest, is the concept of a well-ordered set, and then, perhaps, the concept of a constructible set. One may have doubts about a realistic conception of the standard natural numbers, but such doubts cannot gain support from non-standard models. Attempts to utilize non-standard models for an anti-realist position in mathematics, which appeal to meaning-as-use, or to arguments of the kind proposed by Putnam, fail through irrelevance, or lead to incoherence. Robinson's skepticism, on the other hand, is a coherent position, though one that gives up on providing a detailed philosophical account. The last section enumerates various uses of non-standard models.

1. The Appearance of Non-Standard Models

A non-standard model is one that constitutes an interpretation of a formal system that is *admittedly different* from the intended one. The import of 'admittedly different' will become clear in sections 2 and 3. To prevent misunderstandings, let me emphasize that by 'interpretation' I mean a structural interpretation, where

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isomorphic models count as the same interpretation. (Any attempt to find what the mathematical objects really are amounts to a wild goose chase.)

Non-standard models have been introduced by Skolem, in a series of papers from 1922 to 1934, in two cases: set theory and arithmetic. The earlier papers concern set theory. In [1922] he observes that if there is a structure satisfying the axioms of set theory, then, because of the Löwenheim-Skolem theorem, there is also such a countable structure. This came to be known as Skolem's paradox: a theory that asserts the existence of uncountable sets is itself satisfiable in a countable model (if it has models at all). There is of course no paradox. As Skolem notes, the model satisfies the claim that some member, X, is uncountable just when there is no member in the model that, inside the model, constitutes a one-to-one mapping of X into the model's natural numbers. This is compatible with the fact that the set of all members of the model is countable. The mappings that establish countability "from the outside" need not belong to the model. Skolem himself was somewhat leery of uncountable totalities and he found that Skolem's paradox sits well with the view that everything is countable; uncountability is a property that an entity might have inside some countable structure, but that is all there is to "uncountability". As reported by Wang, in [Skolem 1970], Skolem makes in [Skolem 1929] the following observation:

> One recognizes here again, as with the earlier review of the Löwenheim Theorem, that there is no possibility of introducing something absolutely uncountable except by means of pure dogma.

By 'absolutely' Skolems means the non-relative concept. A set is absolutely uncountable, when it has this property not inside a model, but in the "real universe", which is studied by non-formalized mathematics (i.e., it is an infinite set that is not equinumerous with the set of natural numbers). The *non*-absolute concept is, by contrast, something that is model-dependent. It is a property a set can satisfy within a model, assuming that there are models that satisfy the sentence 'there are uncountable sets'.

Of course, the paradox does not imply that absolutely uncountable sets do not exist; it is compatible with what Skolem calls "dogma". But it can give comfort to someone who is skeptical, because it shows how one who rejects absolutely uncountable sets can nonetheless apply the concept coherently when relativized to some countable model. One can thus work in set theory and speak of uncountable sets, but view all such talk as a description of what goes on in countable models. It is likely that Skolem, who spoke of the "relativity of set theoretic notions", was inclined to such a view. He was willing, for example, to accept the axiom of choice as a formal consistent supposition, but rejected it as a principle that goes beyond this. In a lecture from 1932 (reported by Fenstad in [Skolem 1970] p. 14) we find:

> If one works within a completely formalized mathematics, based on a finite number of precisely stated axioms, there is nothing to discuss but questions of consistency and the ease of manipulation. But in ordinary mathematical practice, e.g., in the usual studies on continua, which are never given by a set of specified rules, the axiom of choice is, in my opinion, definitely undesirable—a kind of scientific fraud.

It should be noted that 'consistency' was for Skolem not a syntactic notion but a semantic one: the existence of structures satisfying the axioms. Skolem thus distinguishes between "completely formalized mathematics" and "ordinary mathematical practice". The first amounts to a study of structures satisfying the axioms; the second is presumably a study of what we might call today 'the intended interpretation'. The shift to a completely formalized mathematics can serve to defuse foundational disagreement about what the intended interpretation should be. **A** can doubt the truth, plausibility, or factual meaningfulness of an axiom adopted by **B**, but, as long as it is consistent, **A** can make sense of what **B** is doing by regarding it as an investigation into the common properties of the structures that satisfy the axioms. This is possible as long as the completely formalized theory is consistent; if it is not, then those who presuppose it are not investigating anything. The consistency problem becomes crucial.[1] Formalized mathematics may thus serve as a mediator of sorts between different foundational views. But for this very reason it does not fully capture the view that underlies ordinary mathematical practice—in as much as the practice implies a a particular structure that constitutes *the* subject matter of the inquiry, "what it is all about". If set theory is about some domain that includes uncountable sets, then any countable structure that satisfies the formalized theory must count as an unintended model. From the point of view of those who subscribe to the intended interpretation, the existence of such non-standard models counts as a failure of the formal system to capture the semantics fully.

This indeed is the way Skolem views non-standard models of arithmetic. The very title of his 1934 paper, in which he constructs an elementary extension of the standard model of arithmetic, says as much: "About the impossibility of characterizing the number sequence by means of a finite or an infinite countable number of statements involving only numeric variables". A 1933 forerunner of this paper bears a similar title. (Note that the existence of a non-standard model of arithmetic is not a consequence of the Löwenheim-Skolem theorem. Skolem's original construction of it anticipates the formation of an ultrapower.)

Skolem thus drew different lessons from the existence of unintended models in the case of set theory and in the case of arithmetic. In the first case, the existence of countable (hence, "non-standard") models helps him to maintain his doubts about absolutely uncountable sets. In the second, non-standard models show an essential shortcoming of a formalized approach: the failure to fully determine the intended model. The reason for the difference is obvious: In as much as the intended model is problematic, the existence of non-standard models support one's doubts. But when the intended model is accepted as a basic precondition of our mathematical investigations, the existence of non-standard models points to the inability of the formalization to characterize the intended model. The difference thus stems from the gulf that separates the standard model of natural numbers from higher order arithmetic. In [Skolem 1934], the very statement of Theorem V, which asserts the existence of a non-standard model, takes for granted the standard model:

> There exists a system N^* of things, for which two operations $+$ and $\cdot$ and two relations $=$ and $<$ are defined, such that N^*

[1]The proof of consistency, which had been a central goal in Hilbert's program, turned out to require a strictly stronger theory than the theory whose credentials were in question. But this is another matter that does not affect the points at issue.

> is not isomorphic to the system N of natural numbers, but nevertheless all sentences of P which are true of N are true of N^*.

This is not to say that foundational misgivings may not apply to the standard model of arithmetic. But such misgivings, which may lead to the adoption of a weaker deductive system, do not derive from the existence of non-standard models.

The term 'non-standard' is nowadays used first and foremost for non-standard models of arithmetic, or for models containing the natural numbers as an essential component whose numbers are non-standard. Its use for models of set theory is less common; to the extent that it is used there, it is applied not to countable models but to models whose ordinals are not well-ordered. Terminological conventions can be quite arbitrary and it is dangerous to use them as a basis for speculation. Yet it seems to me that this terminological usage is happy and significant; for 'non-standard' is applied in the most clear-cut cases, where we have an intended interpretation and the model differs from it. The case of arithmetic is obvious; it appears that, after it, the notion of a well-ordering (i.e., the distinction between well-orderings and non-well-orderings) is a very natural candidate for an absolute mathematical notion. Some reasons for this view are given in section 3. I have also encountered 'non-standard' as a description of models of second-order logic in which the second-order variables do not range over the full power set. If one subscribes to an absolute notion of power set, then one can apply there the standard/non-standard distinction. But many have found this notion problematic. Appreciating the difficulties that go with the power set, I do not think that this would be a happy use of the standard/non-standard distinction.

The concept of a non-standard model depends essentially on that of a fully formalized language that can have, as interpretations, non-isomorphic structures. Prior to the development of these languages, at the end of the nineteenth century and the beginning of the twentieth, there was no place for such a concept. Yet, given the conceptual innovations and fundamental changes that took place in the long history of mathematics, one may wonder whether something like non-standard models appeared in disguise. In what follows I shall consider certain examples.

2. Comparison with Some Crucial Innovations in the History of Mathematics

The remarkable thing about mathematics, which sets it apart from any other discipline, is its continuity and robustness. The point can be easily made by observing that it would be still possible nowadays to use parts of Euclid's *Elements*—a 2,300 years old text—as a high school textbook in geometry, as indeed it was used until the end of the nineteenth century in England. We might omit some items, and we would do better to add some story about the axioms. But starting from the propositions and the constructions we can go on and teach Euclidean geometry, just as it is laid out in the ancient text. By doing so we will be teaching a solid piece of mathematics, at an acceptable level of rigor. Can we imagine a similar use of Aristotle's *Physics* as a textbook in physics, or of Ptolemy's *Almagest* in astronomy? Mathematical continuity notwithstanding, the history of mathematics is rich in change that is not mere accumulation. Some turning points are in the nature of radical shifts, others come through gradual development whose significance can be appreciated only after the fact. I shall consider briefly the following events.

- The discovery of irrationals,
- The incorporation of negative and complex numbers in the number system,
- The extension of the concept *function* in the nineteenth century,
- The discovery of non-Euclidean geometry.

My goal is mathematical-philosophical rather than historical. For this purpose a rough, overall correct sketch will do. I have relied on standard well-known works.[2] The descriptions are, of course, rational reconstructions, with full benefits of present-day hindsight.

What is known as the discovery of irrational numbers took place probably in the Pythagorean school, more than 100 years before Euclid. Strictly speaking, this was the discovery that there are incommensurable segments. Greek mathematicians took for granted the concept of a whole number (which corresponds to that of a non-zero natural number, or—on some philosophical views that I shall ignore here—a natural number > 1), but did not have a number system in the present day sense, which encompasses integers and rationals. The basic concept that underlies their framework is that of *magnitude* (positive quantity), of which there are various kinds; length is one kind, area is another. Magnitudes of the same kind are ordered and can be added; a magnitude can be subtracted from a bigger one. Based on the addition of magnitudes, one naturally gets a product operation of magnitudes by whole numbers:

$$n \cdot x =_{Df} x + x + \ldots + x \quad (n \text{ times})$$

Magnitudes of the same kind, x and y, are commensurable if there exists z and whole numbers m and n, such that

$$x = m \cdot z, \quad y = n \cdot z.$$

In that case the *ratio*, $x : y$, can be defined as $m : n$. Various properties were presupposed, explicitly or implicitly, which nowadays would appear as axioms. Trying to present their setup in contemporary terms, we may recast it as a many-sorted system that includes some fragment of Peano arithmetic. Prior to the discovery of incommensurable segments, it had been taken for granted that any two magnitudes of the same kind were commensurable. There is little doubt that this was a deeply ingrained presupposition. It was refuted by the example of an isosceles right triangle; Pythagoras' theorem was used to show that if the ratio of the hypotenuse to one of the sides is $m : n$, then the square of this fraction must be 2; as can be easily shown, this is impossible. The historical evidence is scant, but from later stories it appears that the discovery generated a crisis;[3] from the Greeks' perspective, it revealed something strange in the very nature of the world. The crisis was eventually resolved by providing a definition of equality between ratios, without assuming that these ratios are rational. Formally, we can describe it as a definition of a 4-place relation, $\mathrm{EqRat}(x, y, x', y')$, which reads:

The ratio of x to y is the same as the ratio of x' to y',

[2]For the first item I drew on Euclid's text and on [Heath], for the second—on [Boyer], for the third—on [Boyer] and [Kline], and for the fourth on [Kline], [Bonola] and [Greenberg]; the letters of Gauss are quoted in [Greenberg].

[3]cf. [Heath], pp. 154–157 and p. 326 in the Dover edition.

where the magnitudes in each of the pairs, x, y and x', y', are of the same kind. If all the magnitudes are lengths of rectilinear segments, a geometric definition, based on similar triangles, can be given. But this cannot serve for lengths of different types of curves, or for general areas, or volumes. The solution, given by Eudoxus, amounts to the following definition of $\mathrm{EqRat}(x, y, x', y')$:

$$\text{For all whole numbers, } m, n\text{:} \quad m \cdot x > n \cdot y \;\Leftrightarrow\; m \cdot x' > n \cdot y'$$

The definition had all the desired properties and it provided the so-called method of exhaustion for proving a host of results concerning lengths, areas and volumes.[4] Until the nineteenth century it served as a paradigm of rigorous mathematical reasoning.

Can we describe this development as a change in the intended model? By no means. The Greek were realists about geometry. There was only one story to be told, and there was no question what the intended model was. The existence of incommensurable magnitudes was an objective fact. It meant that their previous story contained a factual error.

Now to the second example. The appearance of negative numbers predates that of complex numbers by more than 1,500 years. I have lumped them together, because they constitute the same type of innovation. Both appeared as auxiliary devices in algebraic-computational contexts, both have been regarded with suspicion since they did not represent recognized quantities, and both were eventually legitimized through easy modelling that construes them in terms of positive numbers.

Negative numbers appear in Chinese texts in the third century BC, as intermediary items in the process of solving algebraic problems, but not as final answers. Previous to that, they appear in ancient Chinese abaci, which had red counters for positive numbers and black for negative ones. In the seventh century AD, both positive and negative numbers appear as solutions of equations in the works of Brahamagupta; these works, which were geared to equation solving, were not noted for their rigor. After that, negative numbers disappear for a long time, being absent from the works of the Arab mathematicians and reappearing in the renaissance, as computational props for solving algebraic equations. At that time also imaginary numbers appear, playing a similar auxiliary role. The joint algebraic-geometric approach provides a way of interpreting negative numbers in terms of the negative direction on the line. Nonetheless they were viewed with suspicion. In his *Geometrié*, from 1637, Descartes distinguishes between "true" (positive) and "false" (negative) roots. A similar suspicion greets imaginary and, more generally, complex numbers. Girard, in a treatise from 1629, recognizes both negative and complex numbers as roots of polynomials, which enables him to guess the relation between the polynomial's roots and its coefficients. But, prior to the nineteenth century, only polynomials with real coefficients are considered. Eventually, negative and

[4]Obviously, this requires that we assume the axiom of Archimedes; Dedekind's and other definitions of real numbers presuppose this as well. The axiom was stated by Archimedes as a claim that any magnitude is capable, upon being added "continuously" to itself, of exceeding any magnitude of the same kind (he mentions that it had been in use before him). That claim had been taken for granted as a truth about magnitudes in general. Non-Archimedean geometries appeared only in the last decade of the nineteen century, within the general foundational work in geometry, though one may see in Leibniz's conception of infinitesimals a precursor of the idea.

complex numbers become first-class citizens through the realization that the desired structure can be easily defined in terms of its non-negative real part. Thus, the addition of negative numbers is accomplished by adding a reversed copy of the positive ones, back to back with the non-negative numbers, and by defining, over the enlarged domain, addition and multiplication in the obvious way. And complex numbers are identified in the usual way with pairs of reals.

The story is a striking illustration of the gap separating the epistemic aspect of mathematics and what, for lack of a better term, I shall call its "structural ontology". The reductions just mentioned show that if we view realistically a certain structure (say, the sequence of natural numbers), we are justified in taking a realistic view of another structure (say, the integers), because we can trivially define the second structure in terms of the first (say we use the collection of pairs $(0, m)$ and $(1, n)$, where m varies over the natural numbers and n varies over the non-zero natural numbers, to represent all integers: $(0, m)$ represents m, $(1, n)$ represents $-n$, and the arithmetical operations are defined in the obvious way). Included also under "trivial reductions" are cases where equality is represented as a congruence relation, as in the representation of rational numbers as pairs of integers. The various types of structural reductions are not my concern here (for a general, philosophically-motivated technical treatment cf. [Gaifman 1974]); it is clear that the above reductions *are* sufficient for showing that there is no more to the enriched structure than there is to the original one. Statements about the former are thereby easily translated into equivalent statements (which must have the same truth-values) about the latter.[5] Epistemically, though, there can be a tremendous difference. Real analysis and complex analysis are different subjects, involving different methods and different heuristics, the representation of the latter in terms of the former notwithstanding. Note that the "discovery of zero", i.e., the extension of the positive-number system to one that includes also zero, is another example of this kind. It, and the admitting of negative and complex numbers, are momentous steps, which, however, do not constitute switches to a different system, but unfoldings of the existing ones. The advance consists in realizing the system's inherent possibilities.

The third example consists in the extension of the concept *function*, which resulted in admitting as functions strange entities that went against the prevailing picture: a nowhere continuous function, the function of Weierstrass (everywhere continuous but nowhere differentiable) and Peano's curve (a function that maps $[0, 1]$ continuously onto a square). Superficially it might appear as if such extensions consisted in changing the intended model by admitting some "non-standard" elements. But this would be quite a wrong description. The fact is that the intended interpretation of 'function' was loose to start with. Functions were conceived on the paradigm of dependencies arising in physical processes, or as correlations determined by rules of the kinds current at the time. The development of mathematics

[5]This type of translation, which leaves the logic intact, should be distinguished from the syntactic type that preserve provability relations, exemplified by the translations between classical and intuitionistic logic. A more general approach to "ontological reductions", which is based on translations only, without appeal to modelling, is outlined in [Gaifman 1976]. In a nutshell, the translation, beside being recursive, should commute with the connectives; this blocks the intuitionistic/classical case. The topic is beyond this paper's concerns.

provided definitions of abstract correlations, which had anti-paradigmatic properties. Since it became clear that there was no natural boundary for definitions that can produce functions, mathematicians in the end adopted the extensional concept, on which a function is any set of ordered pairs satisfying the functional condition. Previous types of functions could be recovered as particular subspecies of the comprehensive notion: algebraic functions, rational functions, continuous functions, piecewise continuous functions, and so on.

The fourth example, that of non-Euclidean geometry, is again, I shall argue, not a case of a non-standard model, though more than other candidates it might appear so. It is also the most instructive case concerning our topic. While the history is known in general, there is some prevailing vagueness concerning the details. Here is a brief clarification.

Parallel lines are defined by Euclid as lines in the same plane that do not intersect, or, to put it nearer the original phrasing, do not meet one another when produced indefinitely in both directions. It is proven, without using the parallel postulate, that if two straight lines form with a transversal (a third line that intersects both) equal interior alternate angles—or, equivalently, if the sum of the interior angles on the same side is equal to the sum of two right angles (i.e., π)—then they are parallel. This implies that, given any line and any point outside it, there is at least one line parallel to it passing through the point (for one can construct a line that forms an equal alternate interior angle). The fifth postulate, which became known as the parallel postulate, is stated in the first book of the *Elements* and claims this: For any two lines and any transversal, if the sum of two interior angles on one side is less than the sum of two right angles, then the lines meet on that side. This postulates implies that, given a line and a point outside it, there is a unique parallel line to it through that point. Vice versa, the last statement (also known as Playfair's postulate, after Playfair's 1795 formulation) implies the parallel postulate. Thus the two are equivalent, given the rest of the system; and the negation of the parallel postulate amounts to the claim that there is more than one parallel. Here and henceforth, I include under "the rest of the system", also the many implicit assumptions that underlie Euclid's geometry. To make this precise one should appeal to axiomatization that renders it all explicit, such as Hilbert's. But this would not change the picture just sketched, in which the parallel postulate plays a singular role. When I speak of statements equivalent to it, I mean that the rest of the system is kept unchanged.

Before the nineteenth century the subject matter of geometry was taken to be actual space—as we conceive it by our geometric intuition. Disagreements about the space's nature—whether it merely reflects relations between existing physical bodies (as Leibniz thought), or whether it is an independent absolute medium (as Newton thought)—did not affect the agreement about its mathematical properties. The parallel postulate was regarded as a true statement about space. The dissatisfaction with it stemmed from its complicated and somewhat contrived nature, when compared with other postulates; it was considered true, but not sufficiently self-evident to justify its status as a postulate.

For example, geometric intuition suggested that, on the side where, according to the postulate, the two lines should meet, the lines approach each other. But what if they approach asymptotically without intersecting? Such asymptotic behaviour was known to the Greek from the hyperbola and other curves. Proclus, in the fifth century AD, observes that in the case of straight lines such a behavior is implausible, yet it needs refutation by proof. He ends by deriving the postulate from the assumption that if two lines have a common perpendicular, the distance between them (measured by perpendicular segements from one to the other) is bounded. The assumption may appear intuitively more appealing then the parallel postulate; in fact, it is equivalent to it.

Quite a few proofs are of this type. The proofs cannot be considered errors, if the aim of the authors was to derive the postulate from a simpler, more obvious claim. Thus, Wallis (around 1660) derived the postulate from the assumption that for any given triangle there is a similar triangle with a specified side; and Clairaut (in 1741) suggested that the postulate be replaced by the simpler more obvious truth: there exists a rectangle (a quadrilateral with four right angles). Others, who tried to get a contradiction from the negation of the postulate, and who guarded meticulously against using implicit assumptions, found themselves developing non-Euclidean geometry. Saccheri reduced the postulate to the hypothesis that in a quadrilateral $ABCD$, with two right angles at A and at B and with $AC \cong BD$ (picture AB as the base and AC and BD as the two sides), the remaining two angles are right. (Such a quadrilateral is known as a Saccheri square. It is easily seen that the angles at B and C are equal.) By 1733 he has refuted the hypothesis that the angles are obtuse, and it remained to refute the hypothesis that they are acute. Having tried for years in vain, he wrote in frustration:

> The hypothesis of the acute angle is absolutely false, because
> it is repugnant to the nature of the straight line.

He supported that claim by further analysis and further arguments that appealed to geometric intuition. A considerable amount of non-Euclidean geometry was also developed by Lambert (around 1766, and published in 1786, after his death). He found that the negation of the parallel postulate implies (again, retaining the rest of the system) that the side of an equilateral triangle is completely determined by its angle, and that the angle must lie in $(0, \pi/3)$. The side of an equilateral triangle that is associated with a given angle can therefore serve as an absolute unit of length.

At the beginning of the nineteenth century some mathematicians were convinced that the parallel postulate could not be derived, and some speculated about the possibilities of geometries that are based on its negation. These speculations went hand in hand with the discoveries of theorems implied by the negated postulate, which revealed a possible new geometric structure. The grasp of this structure was the crucial factor underlying the convictions of Gauss, Bolyai and Lobachevsky that the non-Euclidean geometry, which is obtained by replacing the parallel postulate by its negation, is a consistent theory describing a species of a geometric space—all of this long before a consistency proof was even thought of. Philosophers sometimes complain that the *perceiving* of a pattern, or the *grasping* of a structure, are mysterious notions, and that no philosophical account has been given of the human faculty by which this is achieved. The discussion in section 3 will touch on this problem, but I shall not pursue it. In the case of non-Euclidean geometry, we can gain clarification from the posthumously published letters of Gauss.[6] A crucial

[6]Gauss did not publish his results on non-Euclidean geometry, partly for fear of a hostile reception, given the then prevailing Kantian dogma that Euclidean geometry is an *a priori truth*. In

step for him was to realize how the metric should work in a non-Euclidean space, and how everything then fits together. To cap it all, the geometry turned out to depend on the value of single parameter, in such a way that the non-Euclidean space converges to the Euclidean one when the parameter tends to infinity. In a letter from 1824 to Taurinus, he notes that, without the parallel postulate, one can prove that the sum of the angles in a triangle does not exceed π, but the case of a sum smaller than π is quite different. He then continues:

> The assumption that the sum of the three angles is less than π leads to a curious geometry, quite different from ours but thoroughly consistent, which I have developed to my entire satisfaction, so that I can solve every problem in it with the exception of a determination of a constant, which cannot be designated *a priori*. The greater one takes this constant, the nearer one comes to Euclidean geometry, and when it is chosen indefinitely large the two coincide.[7] The theorems of this geometry appear to be paradoxical and, to the uninitiated, absurd; but calm, steady reflection reveals that they contain nothing at all impossible. For example, the three angles of a triangle become as small as one wishes, if only the sides are taken large enough; yet the area of the triangle can never exceed a definite limit, regardless how great the sides are taken, nor indeed can it ever reach it.
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> All my efforts to discover a contradiction, an inconsistency, in this non-Euclidean geometry have been without success, and the one thing in which it is opposed to our conception is that, if it were true, there must exist in space a linear magnitude, determined for itself (but unknown to us). But it seems to me that we know, despite the say-nothing word-wisdom of the metaphysicians, too little or too nearly nothing at all, about the true nature of space, to consider as *absolutely impossible* that which appears to us unnatural. If this non-Euclidean geometry were true, and if it were possible to compare that constant with such magnitudes as we encounter in our measurements on the earth and in the heavens, it could then be determined *a posteriori*. Consequently, in jest I have sometimes expressed the wish that the Euclidean geometry were not true, since then we would have *a priori* an absolute standard of measure.

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one of his letters he refers to the Boetians [philistines] as those who will probably misunderstand his results. His work became known after his death in 1855. Lobachevsky's first Russian publication in 1829 was indeed greeted with ridicule in St. Petersburg. A German version appeared later in 1840. Bolyai's work was published in Hungarian, in 1832, as an appendix in his father's mathematical text book, though he probably had worked out most of the system by 1825.

[7] This constant appears in a letter from 1833, as k, in the expression for the circumference of a circle with radius r: $\pi k(e^{r/k} - e^{-r/k})$. The expression can be rewritten as: $2\pi r(r/k)^{-1}\sinh(r/k)$. When $r \to \infty$, the value approaches $2\pi r$. Gauss remarks that, in order to make non-Euclidean geometry accord with known experience, the constant should be chosen incomparably larger than known measurements. k also appears in the neat expression for the area of a triangle Δ: $k^2\,\mathrm{def}(\Delta)$, where $\mathrm{def}(\Delta)$ is Δ's *deficit*, the difference between 2π and the sum of its angles.

The basis for Gauss's conviction about his non-Euclidean geometry consists of (i) his detailed grasp of the geometric structure, (ii) the failure of his efforts to derive a contradiction from the non-Euclidean axioms. One's grasp might of course be misleading, especially in the case of a radically new structure. Gauss is therefore cautious to test his conception by repeated attempts to derive a contradiction. (ii) is required, but (ii) is not sufficient in itself. Formal consistency does not generate meaning.

Let me sketch briefly, for the sake of completeness, the rest of the story. After Gauss's death in 1855, non-Euclidean geometry became a focus of intensive studies. Riemann's groundbreaking works in the second half of the nineteenth century helped to unleash a host of geometrical spaces. Within this host, the system that had been the subject of the historical drama—the one obtained from the Euclidean system by replacing the parallel postulate by its negation—came to be known as hyperbolic geometry.[8] In another type of geometry, known as elliptic, there are no parallel lines altogether. This geometry involves, however, changes in other Euclidean axioms, beside the parallel postulate. It should be also noted that projective and affine geometries, which are in a sense non-Euclidean, were known prior to the emergence of non-Euclidean geometry. But these geometries can be regarded as representing basic non-metric aspects of the Euclidean system. The special role of hyperbolic geometry, which makes it the center of the drama, stems from the fact that it is a metric geometry that revolves solely on the status of the parallel postulate. In 1868 Beltrami showed that any finite portion of an hyperbolic plane can be realized as a surface known as the pseudosphere (obtained by revolving a certain curve, the tractrix, around its asymptote). Thus he constructed inside the Euclidean space an "inner model" for any finite portion of the hyperbolic plane. Strictly speaking, a relative consistency proof requires a model for the whole hyperbolic plane; but it is not difficult to get such a model from Beltrami's construction.[9] Later, Klein and then Poincaré constructed inner models in which the entire hyperbolic plane is interpreted as the interior of a circle.[10]

Bolyai used the name *absolute* geometry for the system obtained from the Euclidean one by removing the parallel postulate. The more current name is *neutral* geometry (introduced by Prenowitz and Jordan in 1965). The theorems of neutral geometry are those that are common both to Euclidean and hyperbolic geometry. To be precise, the vocabulary of the geometric theory should be specified and the axioms of neutral geometry should be stated. A formal specification—according to present day standards of logic, and based on one of the known axiomatizations of geometry—can be given, though it would be anachronistic for the period under

[8] The term, which is due to Klein, derives from his proposed classification of different geometries in terms of invariants of various transformation groups.

[9] Represent the hyperbolic plane as a union of an increasing chain of finite portions; model each on a pseudosphere and construct embeddings between the pseudospheres that are induced by the inclusions between the corresponding portions of the hyperbolic plane. The directed family of pseudospheres provides a model that can serve to prove relative consistency. I do not know if this had been observed at the time.

[10] In Klein's model chords play the role of hyperbolic straight lines; in Poincaré's model they are circular arcs, perpendicular to the given circle (including the diameters of that circle). In both, hyperbolic congruence between segments is defined in a somewhat complicated way. In Poincaré's model, but not in Klein's, the angular measure is Euclidean.

consideration. The details do not affect the points I am making here.[11] The discovery of non-Euclidean geometry means that, contrary to previous expectations, neutral geometry has non-Euclidean models. In this obvious sense, the models were unintended. But we would be wrong to classify them as non-standard. As far as *Euclidean* geometry is concerned, they are not models at all, since they violate one of the axioms. But they have been recognized as models of a different kind of geometry, with a legitimate claim on the concept of a geometric space. It thus turned out that neutral geometry does not determine a univocal concept. By contrast, a non-standard model of arithmetic has no analogous claim on the concept of natural number; it is not as if we have become aware that 'the sequence of natural numbers' has another legitimate interpretation—one which falsifies a standard arithmetical truth. The point will be further elaborated in section 3.

Having said all this, we should note that there is no formal characterization of what comes under 'geometric space'. This is a question about conceptual organization, and it involves, besides the basic vocabulary, the methods and heuristics practiced in mathematical research. That question is not my present concern. It should be also remarked that, as is natural in such matters, opinions vary widely when it comes to the importance of this or that approach, or this or that research area. Gauss, Bolyai and Lobachevsky put non-Euclidean geometry on a par with the Euclidean one, speculating about the possibility of its being physically true. Later mathematicians did not take the possibility of a non-Euclidean physical space seriously. Many considered geometric space to be Euclidean *par excellence* and the non-Euclidean varieties as deriving their meaning from Euclidean based models. There was, in the later half of the nineteenth century, a spectrum of opinions on these matters.[12] With general relativity, the pendulum has of course swung back, as far as physics is concerned, though not in favor of the variant considered by Gauss, Bolyai and Lobachevsky. All of this does not change the picture outlined above. Even if, mathematically speaking, we accord pride of place to Euclidean

[11] The vocabulary that seems to fit the historical usage should have both numeric and geometric terms. Accordingly, let us consider structures of the form $(\mathcal{M}_n, \mathcal{M}_g, \mathcal{R})$, such that (i) $\mathcal{M}_n$ is a numeric model whose universe consists of the real numbers, with various sets and relations, including the subset of natural numbers (which interprets predicate 'x is a natural number'), and various real functions, (ii) $\mathcal{M}_g$ is a geometric structure that includes points, lines, planes, the incidence relation and others, e.g., the being-in-between for points on a line, and (iii) $\mathcal{R}$ consists of relations that connect the two, e.g., the relation that associates with any two line segments (given as 4 points) the real number that is their ratio. The formal language is the corresponding first-order, many-sorted one. The model should satisfy the axioms of neutral geometry, which we can get by adopting some known system, such as Hilbert's, to the type of structure we have. The axiom of Archimedes is stated in a straightforward way, using quantification over natural numbers. What is known as Hilbert's axiom of linear completeness can be rephrased as a second-order statement stating that every bounded set of real numbers has a least upper bound. To express it in our first-order theory, we use, as is well-known from other cases, an axiom scheme. This allows for the possibility of a non-standard numeric component: The natural numbers can be non-standard, and the real field may contain also "gaps", that is, cuts that do not determine a real number (the completeness scheme only guarantees that cuts that are definable from parameters determine real numbers). But it would not affect the syntactic aspect of the theory, because a non-standard numeric component will determine a corresponding non-standard geometric one; that is: given $(\mathcal{M}'_n, \mathcal{M}'_g, \mathcal{R}')$ satisfying the axioms, every elementary submodel $\mathcal{M}_n \prec \mathcal{M}'_n$, determines an elementary submodel $(\mathcal{M}_n, \mathcal{M}_g, \mathcal{R}) \prec (\mathcal{M}'_n, \mathcal{M}'_g, \mathcal{R}')$, which is unique up to isomorphism over $\mathcal{M}_n$.

[12] cf. [Kline] pp. 921-923.

space, we should still acknowledge the status of non-Euclidean systems as different species within the overall geometrical framework.

Let me sum up the four historical cases and how they differ from non-standard models. First, the discovery of incommensurables is a discovery that a certain presupposition about spatial magnitudes was false. There is only one model, the standard one; we were simply mistaken about one of its basic features. Second, the enlargement of the positive number system by incorporating negative and complex numbers amounts to utilizing the possibilities inhering already in the positive numbers; there is no change of the standard model, but an unfolding of it. Third, the extension of the function concept to that of an arbitrary mapping (given as a set of pairs) is an explication of a previously loose concept, which is needed because new examples do not conform to previous expectations. There are no two models, but one developing conception. Fourth, the discovery of non-Euclidean geometry is the discovery that the concept of geometric space is ambiguous and admits an additional specification besides the received one; the difference is expressed as the denial of an accepted postulate. Here indeed there are several models and non-Euclidean geometry is, as Gauss noted, strange. If 'non-standard' is another word for 'strange' then it is "non-standard". But, as Gauss, Bolyai and Lobachevsky made clear, and as subsequent developments have borne out, non-Euclidean geometry is a legitimate conception of geometric space. If physical space is to be the arbiter of truth (as Gauss suggested) then neither the Euclidean nor the hyperbolic geometry is the winner. But whatever the verdict of physics, the different geometries constitute different specification of the general mathematical concept of geometric space.

By contrast, a non-standard model *does* share with the standard one the relevant axioms (e.g., the first-order axioms of Peano). It might or might not agree with it on other sentences, but what marks it as non-standard is a structural feature; and, crucially, it does *not* constitute an alternative specification of the concept in question (that of the number sequence, or that of a well-ordering). It is, by definition, something that shares many properties with the standard model, but has no claim on the original concept. One may have doubts about the standard model, which stem from doubts about actual infinities. One may then opt for change of logic, or, as Abraham Robinson did, treat both standard and non-standard models as useful fictions; this has no bearing on the point at issue, namely, that the existence of a non-standard model cannot undermine, in any coherent way, the status of the standard one as the sole intended model for the concept of natural number. That much I hope will become clear in the next section.

A non-standard model can however have its own use and its own interest. It is even conceivable, though extremely unlikely, that physics may give rise to some such model. For if there are physical infinitesimal magnitudes, we may get a physical non-standard model for the theory of real numbers; and it may include as a subset a non-standard model of arithmetic. Even then, such a model will not constitute a possible specification of the concept of natural number.

3. Some Standard and Non-Standard Models in a Philosophical Perspective

Unintended interpretations have loomed large in the philosophy of language in the second half of the twentieth century. Quine used them famously (or infamously) in his behavioristic approach to language. Goodman's celebrated example of 'Grue' belongs here as well. In the nineties they attracted considerable attention, following Kripke's use of them in his highly controversial interpretation of Wittgenstein. Underlying the employment of these unintended interpretations is, roughly, the idea that language acquires its meaning through its use in overt interactions between people or with the world. Therefore, in principle, one should be able to manifest, through public usage, differences between different interpretations. What cannot be thus manifested should be dismissed as something occult. This theme in the philosophy of language is beyond the scope of this paper. I shall only address a particular offshoot of it, which relates directly to the philosophy of mathematics. On this view, if we cannot point to public usage that distinguishes between the standard and the non-standard interpretation of 'the sequence of natural numbers', then the reference of this term is undetermined. Let $\mathcal{N}$ be the standard model, and let $\mathcal{N}'$ be a non-standard elementary extension of it. What is there, it is asked, that determines that one refers to $\mathcal{N}$ rather than $\mathcal{N}'$? Nothing in our deductive practices and in our use of mathematics in science and everyday life seems to decide this.[13] It is important to be clear on the logic of this move. The questioner, call him Q., bases the question on the construction of a non-standard model. Having shown that such a model, which is different from the standard one, exists, Q. claims that the reference is undetermined, since nothing in our public behavior determines it. The trouble with this question is that Q. presupposes the distinction between standard and non-standard models to start with. For Q. appeals to a construction of a non-standard model, which yields, as we can convince ourselves, a different model. If it were impossible to refer differentially to the two types of models, Q.'s question could not be asked.[14] The point can be also put as follows: Q. seems to assume the superior stance of someone who can switch the interpretation from standard to non-standard, while we, who use routinely arithmetical concepts, do not notice. But in fact, Q. plays in the same court, appealing to the same conceptual apparatus. To the question "What is it that determines that the intended interpretation is the standard model?", the simple answer is: "The intended interpretation is, by definition, what you yourself called 'the standard model'."[15] This is not an appeal to some mysterious common understanding ("We both know what we mean by 'natural numbers' "), but an exploitation of the fact that the questioner uses the very term, and presupposes the very meaning, which he tries to undermine. Also the question cannot be construed as a *reductio* argument, where one assumes the opponent's point of view in order to derive a difficulty within it. For, by presupposing the

[13]This type of argument is applied by Putnam [1981] to elementary examples from natural language, using very simple unintended interpretations in the spirit of Quine.

[14]Kripke, who raises in [1982] a "skeptical doubt" that is similar to Putnam's, is nonetheless sensitive to this point. Kripke considers a non-standard interpretation of 'plus', called by him 'quus', which coincides with standard addition in all the finite number of cases in which we have performed the operation, but which differs from it in some untested case. Kripke admits that in order to raise the question the skeptic must presuppose a shared understanding of 'plus' and 'quus' [1982, p. 12]. Therefore he phrases his question as being not about our present use of 'plus' but about our past uses of the term. From this he tries to raise a skeptical doubt about our present use as well. I do not think he succeeds, but this is a different matter. At least he tries to bypass the obvious incoherence that is involved in the straightforward move.

[15]The same type of answer can be given to Putnam's questioner in [1981], because, in order to define the unintended interpretation, Putnam helps himself to the standard interpretation of everyday language.

conceptual apparatus that is needed to construct a non-standard model (the basis of the question), Q. provides us with a way of answering it. The question may have, though, a hidden motive: a request for some sort of explanation of how we come to know mathematical entities. If the explanation is supposed to provide some sort of causal link between the brain and the mathematical structures, then the request should be rejected as a muddled question stemming from a muddled philosophical picture. But if it is a request for an account of mathematical knowledge, then it amounts to a fundamental question that the philosophy of mathematics should tackle. I do not propose to embark on it here.

Intended interpretations are closely related to realistic conceptions of mathematical theories. By subscribing to the standard model of natural numbers, we are committing ourselves to the objective truth or falsity of number-theoretic statements, where these are usually taken as statements of first-order arithmetic. The standard model is supposed to provide truth-values for these statements. Since deductive systems can only yield r.e. (recursively enumerable) sets of theorems, they can only partially capture truth in the standard model.[16] Thus we get a substantial notion of truth: truth that goes beyond what we can prove (from any given r.e. set of axioms, using any r.e. collection of inference rules). Even the truth of Π_1 sentences cannot be fully captured. Realism and intended interpretations are thus intimately related; often they are treated as the same problem. Yet the intended models of a given mathematical language may contain non-isomorphic structures (e.g., the theory of all well-ordered sets, with ordinal addition and multiplication). Truth in the theory then means truth in all the intended models, and, depending on the case, it may or may not outrun deductive capacity.

One can be skeptic with regard to the standard model of arithmetic, because, say one has doubts about actual infinities; but, as argued above, one cannot support this skepticism by appeal to non-standard models. This applies also in the case of set theory. For one who subscribes to some standard model of ZFC (Cantor's universe, or whatever), the existence of different models of the same theory, does not *per se* pose a problem. One can however pose a different question: *Which, if any, of some given models, is the standard one?* This question does not presuppose the notion of a standard model; it only asks us to locate the intended model within a given family. This question brings out the difference between arithmetic and set theory. In the case of the natural numbers, the standard model is characterized by a *minimality* condition: it is the smallest model, included as an initial segment in any other model. If a given model is non-standard, then this will be revealed by a proper initial segment that is closed under the successor function. Formally, the characterization is expressed by the inductive scheme:

$$\text{(I)}\quad P(0) \wedge \forall x[N(x) \rightarrow (P(x) \rightarrow P(x+1))] \;\rightarrow\; \forall x[N(x) \rightarrow P(x)]$$

where '$N(x)$' stands for 'x is a natural number', and where '$P(\ )$' stands for any predicate. Any wff of the language we are using can be substituted for '$P(\ )$'. The concept of the sequence of natural numbers is, however, *not* language dependent. The absoluteness of the concept can be secured, if we help ourselves to the full (standard) power set of some given infinite set; for then we can treat 'P' as a variable ranging over that power set. But this is highly unsatisfactory, for it bases

[16]One can, of course, consider deductive systems whose axioms are not r.e.; but these will be "deductive systems" only in name.

the concept of natural numbers on the much more problematic shaky concept of the full power set. It is, to use a metaphor of Edward Nelson [1986], like establishing the credibility of a person through the evidence of a much less credible character witness.

The inductive scheme should be therefore interpreted as an open ended meta-commitment:

(II) Any non-vague predicate, in whatever language, can be substituted for 'P' in (I).[17]

(We assume here either that the substitution involves no category mistakes, or that category mistakes are treated as false by definition, so that the antecedent in (I) becomes false, and the whole conditional—true.) As Van McGee expresses it, if God himself creates a new predicate, then this predicate can be substituted for 'P'.

I think that, after the concept of the number sequence, the next candidate for having an intended interpretation is the concept of a *well ordering*. Here we are not concerned with a particular structure, but with a condition on ordered sets. The intended interpretation is thus given by the class of all "truly" well-ordered sets. I shall content myself with some observations in support of that view. For any given ordered set, the condition of its being well-ordered is a natural generalization of (I):

(I*) $\forall x[\mathrm{Ord}(x) \rightarrow [(\forall y < x\, P(y)) \rightarrow P(x)]] \ \rightarrow\ \forall x[\mathrm{Ord}(x) \rightarrow P(x)]$

where $\mathrm{Ord}(x)$ says that x is a member of the ordered set in question. As in (I), 'P' is substitutable by any (non-vague) predicate in whatever language; the richer the range of 'P' the stronger the condition. By increasing this range we weed out non-standard well-orderings. Let ORD be the ordered class of all ordinals. Since every well-ordered set is isomorphic to a unique initial segment of ORD, this well-ordered class gives us, essentially, the standard interpretation of the well-ordering concept. ORD is a proper class, yet we have a truth-definition for it, i.e., for the structure $(\mathrm{ORD}, <)$, because the structure is a union of a closed unbounded class of initial segments, whose inclusions in each other are elementary embeddings. The same holds if we enrich the structure by adding ordinal addition, multiplication and exponentiation, as well as various other functions and relations. The problem of truth that plagues the concept of "Cantor's universe of sets", does not arise for these types of structures.

If we accept the concept of a well-ordered set as clear-cut and unambiguous, whose interpretation is given by ORD, we may go on and accept any inductive construction based on it, provided that each step is non-problematic. Now the

[17] The qualification 'non-vague' is added to bar predicates such as 'small number', which give rise to an arithmetical version of the Sorites paradox: 0 is small, and, for all n, if n is small so is $n+1$; but there are numbers that are not small. Such predicates are problematic in any case, for the Sorites paradox arises in strictly finite situations (a distance of 10 feet is a walking distance, and, for every n, if n feet are a walking distance so are $n+1$ feet; yet $50,000$ feet are not a walking distance). Hence, until we have a good account of such predicates, we should exclude them. I have proposed an account, on which the evaluation of such a predicate depends also on a contextual parameter (this is presented in "Vagueness, Tolerance and Contextual Logic", which is on my website, http://www.columbia.edu/∼hg17); when the contextual parameter is made explicit one can remove the qualification. Vagueness has however been suggested by Essenin Volpin as a way of setting up a strictly finitistic arithmetic. That strictly finitist view, which amounts to a radical denial of actual infinity, rejects of course the standard model altogether.

universe of constructible sets, L, is obtained in this way. Thus, our third candidate for a standard model is L, the intended interpretation of the concept *constructible set*. The argument for L is considerably weaker than the above argument for the structure of ORD, under $<$ with various additional functions and relations; for we cannot get for L a truth-definition (if we could we could have proven the consistency of set theory within itself). The argument rests solely on the plausibility of a transfinite construction, running through all ordinals, where each stage is non-problematic. Yet the suggestion is appealing and it is reinforced by the observation that there are no known independence results for L, of the kind that have proliferated in set theory in the last forty years. L seems in this respect more like the natural numbers.

Consider, for contrast, the concept *power set of* ω. While the natural numbers are characterized by a minimality condition, the totality of all sets of numbers involves a maximality requirement:

(III) Any (non-vague) predicate P, in whatever language, determines a set, whose members are all the numbers satisfying P.

Guided by (III), we are led to consider a sequence of interpreted languages, obtained by adding repeatedly new definable sets to the universe of the model. There is however no guarantee that such a process will eventually give us every subset of ω. Some constructions end by producing a countable number of subsets. The most comprehensive construction uses ORD in order to carry an induction that runs through all ordinals: at each stage we add all subsets of the structure's universe that are definable in the structure, and at limit ordinals we take unions. This yields the constructible universe, L. The subsets of ω obtained in this way are the constructible ones. Still many set theoreticians believe that "most" subsets of ω are not in L. Apparently, (III) does not secure the concept of *all* subsets of ω, in the way that (I) secures the concept of the number sequence. Yet the concept of *all* subsets of ω (or of any given set) has an extremely strong hold on our mathematical imagination. It takes a corpus of sophisticated forcing techniques for generating a host of well-founded models of set theory, which satisfy different theories, to give rise to speculations that the "totality of all subsets" of ω is not a univocal concept. Can new set-theoretical axioms, which carry high plausibility, point to a univocal concept? This is an open central question, the subject of a great amount of current research.

Let me now discuss Putnam's so-called model-theoretic argument [1980], which uses non-standard models of set theory in order to make an anti-realist point in the philosophy of mathematics.[18] The argument uses Skolem's set-theoretic paradox, in an attempt to undermine a realistic view with regard to set theory. In a nutshell, Putnam claims that the mathematical belief, shared by Gödel and many other set theoreticians, that $V{\neq}L$ has no bearing on any complete description of the physical world, for the following reason. There are no more than a countable number of physical magnitudes that physics (conducted by humans) will ever deal with; all the data of experimental science can be represented by a sequence of real numbers—the experimental values of these magnitudes. They can be coded into a single real number, from which they can be recovered. Putnam then claims that, given any

[18]For all I know, this may be one of the cases (there are several) where Putnam later modified or abandoned his views.

real number, s, there is a countable ω-model (i.e., one with only standard natural numbers) of ZF+V=L containing it. Putnam's proof contains a mathematical error; one needs either to take the claim as an additional assumption, or to derive it from some other additional assumption about L. And we have to assume that the realist, who believes that $V \neq L$, also believes in the truth of the additional assumption. Let us grant this.[19] To be sure, if s is not in L, the model is not well-founded, but this makes no difference; we can carry out all our physical measurements, while assuming that $V = L$. The model, in Putnam's terminology, satisfies all the operational constraints, i.e., all experimental results. The argument addresses a realist who has the belief that $V \neq L$, as well as certain other, not implausible set-theoretic beliefs (cf. footnote **??**). In addition to the operational constraints there are theoretical constraints, consisting of the axioms that underlie our "total science". Set-theoretic axioms are part of that system. Some principles of set theory, we are told, are grounded in rationality requirements, but not statements such as $V = L$ or AC (the axiom of choice). These, Putnam argues, can be adopted or rejected by decisions whose justification boils down to the pragmatic criterion of fruitfulness. We are invited to imagine a society whose mathematicians reject AC, because it has some counterintuitive implication (the Banach-Tarski paradox). We could *not* say, he argues [1980, p. 470] , that "an equally successful culture which based *its* mathematics on principles *incompatible* with choice (e.g., the so-called axiom of determinacy) was *irrational* [all emphases in the original]."

This type of reasoning muddies the water, being irrelevant to the issue. Surely, a realist does not deny the existence of structures—possible interpretations of set-theoretic terms— in which AC is false. And a realist can fully appreciate the fruitfulness of investigating these structures. As a matter of fact, structures in which AC is false have been intensively studied by set-theoreticians who believe that AC is true—true under the interpretation that stems from the set-theoretic conceptual apparatus. Putnam would argue that this "stemming from" is obscure. Indeed, there is a problem whether our set-theoretic apparatus (and practice) is sufficiently unequivocal so as to single out the interpretations in which AC is true. *That* problem, however, is not addressed by him at all; it is rather dismissed, using arguments about the indeterminacy of reference that are based on trivially unintended interpretations, of the kind discussed at the beginning of this section. Instead, he treats the problem as one that should be decided by appeal to general pragmatic criteria and some blurry ideal of rationality.[20] Note that whatever weight his argument

[19] Putnam claims that the statement: 'For every real number x there is an ω-model of ZF + V=L containing x' is true in L. He does this by applying the Löwenheim Skolem theorem to L, an illicit move, since L is not a set. In fact, one can consistently maintain that the statement above is *not* true in L. The above claim should be therefore taken as, or derived from, an additional assumption. It can be derived if we have a definition of satisfaction for L, and this can be obtained from the existence of a suitable large, for example, measurable cardinal. We can then continue, as Putnam does, by applying Schoenfield's absoluteness theorem and deduce the truth of the statement in V. The whole argument is addressed to a realist whose beliefs imply that $V \neq L$ and that the above statement is true in L. Belief in the existence of a measurable cardinal would suffice for both.

[20] Putnam invokes here [1980, p. 473], and in other writings, a theory that is "epistemically ideal for humans", what scientists should rationally accept at the limit of all inquiry. This is supposed to give us ersatz realism, within a humanly-oriented approach. In fact, it is a posit that is supposed to do some philosophical work in an unclear way. One wonders what mathematical number-theoretic beliefs are to be included in this epistemically ideal theory? Do they form a

has for set theory, it also has for number theory; and here the situation is clearer. Non-standard models of arithmetic have many applications, they score high on the fruitfulness scale and they have been studied by and large by logicians who subscribe to the standard model (the notable exception is Robinson, whose views will be discussed shortly). It is possible, in principle, that non-standard models that satisfy a statement widely believed to be false, e.g., 'ZF is inconsistent', will have fruitful applications. There is no incongruity in studying the consequences of a consistent arithmetical statement believed to be false. We study, in that case, models in which, for example, there is a proof of a contradiction from the axioms of ZF; this proof is of course non-standard. Can we then dismiss, by appealing to fruitfulness and rationality, the question of the truth or falsity of 'ZF is inconsistent'? or claim that it does not matter? There is, to be sure, a difference between arithmetic and set theory; but this difference, which is not discussed by Putnam, is not the kind of thing that the model-theoretic argument can clarify.

The model used in that argument is an ω-model, i.e., it has only standard natural numbers. But why should this matter? A much simpler argument from compactness, which does not appeal to Schoenfield's absoluteness theorem (cf. footnote **??**) establishes the existence of a model satisfying $V = L$, which includes all the real numbers that physical measurements will ever yield. These real numbers are included in the sense that their standard parts (i.e., the standard parts of their binary expansions) coincide with what the actual measurements yield. They might have non-standard "tails"; but, on Putnam's view, why should this matter? since we could never discover them.

It appears that Putnam preferred, in [1980], to challenge realism about sets rather than realism about natural numbers. But his line of reasoning cannot make the necessary distinctions between the former and the latter. For this would require that one engage internally in a conceptual analysis of the theories in question. Instead, Putnam uses an argument that in the end boils down to offhand instrumentalism.[21] It is instructive to compare Putnam's easy instrumentalism with what is sometimes described as Hilbert's instrumental position. Hilbert was skeptical about actual infinities and tried to legitimize their use by regarding them instrumentally, as tools for getting true general finitistic results. This was a substantial position in the foundation of mathematics, deriving from internal conceptual analysis and engaging the problem at its core. Although he did not give a precise definition of finitistic statements, or finitistic reasoning, it is not difficult to see what he was aiming at and why. PRA (primitive recursive arithmetic), for example, can serve

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recursively enumerable set? And if so, what about those Σ_1 statements, like the existence of solutions to certain diophantine equations, which are neither provable nor refutable in the ideal theory? Is the question of their truth or falsity meaningless? The dilemma might lead naturally to an intuitionistic view, which enables one to reject the very question. Towards the end of the paper Putnam toys, indeed, with intuitionism; but his observations there are scant and his suggestion of "liberalized intuitionism" is no more than a gesture. In any case, once we are in this ballpark, the model-theoretic argument is altogether irrelevant.

[21]Besides the establishing of consistency, the relevance of model-theoretic arguments of the type proposed by Putnam is doubtful, even on an instrumental approach. Pitowski [1980] suggests a treatment of Bell's paradox in quantum mechanics, which presupposes the continuum hypothesis. (Actually something weaker is needed: the union of less than $2^{\aleph_0}$ sets of measure 0 is of measure 0). The suggestion, whether we find it convincing or not, indicates that, in principle, highly abstract set-theoretic statements can be relevant to physics. Note also that large cardinal axioms can have implications for the structure of the real numbers.

as a plausible candidate for delineating what finitism amounts to. Hilbert's instrumentalism committed him moreover to showing how the use of actual infinities can be eliminated. This was a carefully thought out position. The very failure of the program shows how substantial and well defined it was.

I conclude the section with Abraham Robinson's philosophical view on models of arithmetic, standard and non-standard. In [1973] Robinson characterizes his position as (i) the rejection of actual infinity and (ii) the acceptance of formal systems that purport to describe infinite structures. Robinson describes his view as formalist. Like Hilbert's formalism, it regards formal systems that purport to describe infinite structures as useful tools for getting good finitary results. Any hope of eliminating non-finitistic reasoning is of course gone, by now, after the incompleteness theorems. So Robinson can offer only pragmatic justification, of a rather general kind, for our habits of thinking in terms of actual infinity—something he regards as useful fiction. The position is elaborated in the earlier [1964]. Robinson is aware that the formal system itself—the totality of sentences and proofs—is essentially of the same kind as an arithmetical structure that involves an actual infinity. He reconciles this with his ruling out actual infinities, by distinguishing between formalism as a *method*, or *tool*, and formalism as a *subject of study*. When the formal system becomes a subject of study, it is seen as an actually infinite structure. Then it is not a tool; the tool, at that stage, is a meta-formalism: the one we use in studying our previous tool. We can then adopt, for pragmatic reasons of convenience, efficacy, etc., the "useful fiction" attitude towards our subject of study.

From this overall point of view, Robinson regards standard and non-standard models on a par; both are useful fictions. But while he denies the reality of what mathematical thinking seems largely to be about, he does not want to pay the price of imposing limits on this thinking. Such a position amounts to giving up on the possibilities of a more satisfactory philosophical account.

4. The Many Good Uses of Non-Standard Models

I have argued that non-standard models, or unintended interpretations, cannot serve as a source of philosophical skepticism. To use them in this way is to misuse them. On the other hand, non-standard models have been studied for good mathematical reasons and they have an impressive spectrum of applications. Here is a spur-of-the-moment list of some items. I have made no attempt at full coverage. The topic merits a broader more thorough analysis.

(1) Non-standard models are interesting mathematical structures, studied as such. Their interest is to a large extent logic oriented, stemming from the fact that the model shares many first-order properties with the standard one, but is very different structurally.

(2) They have been broadly used to establish properties of deductive systems. They can provide insight into fragments of Peano arithmetic—for example, an easy proof that bounded induction is not sufficient for defining exponentiation: take a non-standard model of Peano arithmetic; it is easy to show that an initial segment of it closed under addition and multiplication satisfies bounded induction, hence the claim follows by taking such an initial segment that is not closed under exponentiation—as well as hard results, such as the first proof (by Paris) of the Paris-Harrington theorem, or various results concerning Quine's NF system.

(3) They have been used for finding results, or new illuminating proofs, in other branches of mathematics (Gromov's work in group theory, Keisler's work in probability).

(4) Non-standard analysis, Robinson's project, provides a rigorous way of legitimizing a popular conception of infinitesimal magnitudes, which goes back to Leibniz. It has had many applications, from a new way of teaching calculus, to applications in some branches of decision theory where non-Archimedean utilities are considered.

(5) They can serve as a heuristic guide for behavior "in the infinite". As noted above they can be useful in studying fragments of Peano's arithmetic. Such weaker systems can be motivated by finitary views. Nelson [1986] proposes a brand of finitism, which aims to avoid the exponential function. The desired deductive system is clarified by the use of a non-standard model. Of course, the model, being an infinite structure, is regarded in that philosophical context only as an auxiliary tool.

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