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ON ANTIPLATONISM AND ITS DOGMAS

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§1 Introduction

Recent discussion in the philosophy of mathematics, especially in the Anglo-American world, occurs within the framework of some empiricist dogmas accepted as self evident truths by philosophers presumably propounding very different views. Thus, not only Quine and Putnam, Benacerraf and Kitcher, but even philosophers like the so-called Platonist Penelope Maddy, and the so-called nominalist, Hartry Field, and many others accept in one way or another the common core of 'evident truths' that only serve the purpose of reassuring them of the 'obviousness' of their common prejudice, namely: the rejection of the existence of mathematical entities as conceived by the Platonist. Their nowadays common coin argumentations, which we are going to consider in this paper, are devised in such a way that they all presuppose that there are no abstract mathematical entities.

One of the cornerstones of this common framework is the belief that the only, or at least the best, argument on behalf of Platonism in mathematics -thus, on behalf of the belief in the existence of mathematical entities, like numbers or sets, foreign to causal interaction and immune to the vicissitudes of the physico-real world- is the so-called indispensability argument wielded by Quine and Putnam, according to which the successful application of mathematics in physical science guarantees the truth of mathematical statements and (in Quine's case but not necessarily in Putnam's) the existence of mathematical entities. This argument was put forward by Quine in 'On what there is'[1] and more

[1] 'On what there is', in W. O. Quine, *From a Logical Point of View*, 1953, pp. 1-19.

emphatically by Putnam in 'What is Mathematical Truth?'[2], and is more or less accepted by most philosophers in the same tradition.

Another cornerstone of the common framework is Paul Benacerraf's argumentation in 'What Numbers could not be'[3], which supposedly establishes, firstly, that numbers are not sets, and, secondly, that numbers are not objects. Strictly, we have here two different arguments with two different purposes. We shall call them 'Benacerraf's first and second ontological arguments' (in that order). A third and extremely important component of the common framework is Benacerraf's epistemological argument against Platonism in mathematics, put forward in his paper 'Mathematical Truth'[4]. Briefly, that argument tells us that since we do not have epistemological access of a causal nature to any abstract mathematical entity, it is at least unnecessary to invoke those entities to explain our mathematical knowledge. A fourth -probably, less generally accepted- component of the common framework is Putnam's argument against realism in mathematics wielded in his paper 'Models and Reality'[5]. We will call that argument 'Putnam's skolemization argument', since it intends to extract epistemological consequences from the so-called 'Skolem Paradox'. In what follows we shall see that all four components of the common framework have a common 'foundation' in the above mentioned prejudice against the existence of abstract mathematical entities and our possible access to them.

§2 Putnam's skolemization argument

Let us consider first the most important features of Putnam's argumentation in 'Models and Reality'. At the beginning of the paper Putnam tells us that there are three different stands with respect to the problem of reference and truth, especially, reference and truth in mathematics, namely: (1) the Platonist, or extremely realist stand, which he repeatedly stigmatizes as postulating the existence of 'non-natural' or 'mysterious'

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[2] 'What is Mathematical Truth?', in H. Putnam, *Philosophical Papers*, Vol. 1, 1975, pp. 60-78.

[3] 'What Numbers could not be', 1965, reprinted in P. Benacerraf and H. Putnam, eds., *Philosophy of Mathematics*, second edition, 1983, pp. 272-294.

[4] 'Mathematical Truth', 1973, reprinted in P. Benacerraf and H. Putnam, eds., *Philosophy of Mathematics*, second edition, 1983, pp. 408-420.

[5] 'Models and Reality', 1980, reprinted in P. Benacerraf and H. Putnam, eds., *Philosophy of Mathematics*, second edition, pp. 421-444.

mental powers that give us access in an 'irreductible' and 'unexplained' way to mathematical entities and truths; (2) the verificationist, which substitutes the notion of verification for the classical notion of truth - Putnam says 'verification or proof', although it is a consequence of Gödel's first incompleteness theorem that the notion of proof is, in general, no adequate surrogate for the notion of truth in classical mathematics-; and (3) the moderate realist stand, which he characterizes as aiming to preserve the centrality of the classical notions of truth and reference without presupposing 'non-natural' mental powers. Although Putnam does not say it explicitly, when he speaks about moderate realism, he seems to have in mind the conception that he had propounded in 'What is Mathematical Truth?', according to which[6] to be a realist means to maintain that (1) the statements of the theory under consideration (in this case, the whole of mathematics) are true or are false, and that (2) it is something in the physico-real world that makes them true. Thus, such kind of realism does not need to commit itself to the existence of abstract mathematical entities, but only to the objectivity of mathematics. Indeed, Putnam's realism in 'What is Mathematical Truth?' is related to the thesis that the indispensability argument is the only (or at least, the best) argument on behalf of realism in mathematics, since he maintains[7] that 'the criterion of truth in mathematics, as in physics, is the practical success of our ideas'. This sort of pragmatism concerning the notion of truth induces Putnam to follow in that paper in the footsteps of Quine and consider mathematical knowledge as corrigible, thus, as neither absolute nor immune to revision by experience. (Although it is not directly relevant for our present purposes, it should be mentioned that Quine's holism -which should not be confused with the much sounder Duhem thesis- is another dogma of Anglo-American analytic philosophy, and, ironically, one that was introduced in response to the two dogmas of pre-Quinean empiricism discussed by Quine in his duly famous paper.[8] Contrary to the Quinean dogma, revisions in the physical and biological sciences never revise the mathematics -in the sense of considering the theorems of the mathematical apparatus used by the scientist as false- nor revise other scientific theories that are considered

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[6] See pp. 67-68.

[7] See pp. 60-61.

[8] See W. O. Quine, 'Two Dogmas of Empiricism', in *From a Logical Point of View*, pp. 20-46.

completely isolated from the one under consideration. Such a revision would be considered unsound scientific practice.[9]) Continuing with Putnam, he is going to argue that the so-called Skolem Paradox represents a very strong case against such a moderate realism, but leaves untouched both Platonism and verificationism. Let us now present the paradox together with Putnam's assessment of it, which is an uncritical adoption of Skolem's rendering.

Let us consider a system of axioms for set theory, e.g., the system ZF of Zermelo-Fraenkel set theory. One of the theorems of ZF asserts that there are uncontably many sets. The Skolem Paradox occurs when we attempt to formalize ZF in a language -like those of first order- for which the Löwenheim-Skolem theorem is valid. Such a theorem says that any theory expressed in a (finite or countable) first order language -or in any language for which the theorem is valid- which has a model, also has a countable model. Thus, if we formalize ZF in a first order language -as is usually done since Skolem-, ZF will have a model with a countable universe, although one of its theorems establishes that there exist uncountably many sets[10].

Skolem's solution, adopted by Putnam and by most contemporary logicians and philosophers of mathematics, is essentially the following. The existence (or non-existence) of a set is not an absolute feature of sets, but depends on the language under consideration. Hence, the existence (or non-existence) of a set of ordered pairs, as required to establish a bijection between two given sets, is relative to the language under consideration. When we say that a model for a formalization of ZF in a first order language is uncountable, we are not considering all the possible bijections between the set of natural numbers and the universe of the model, but only those that exist inside the model. But the possibility is not excluded that outside the model there exists a set of ordered pairs as required to establish the desired correspondence. Therefore, a model can be uncountable relative to the language under consideration, but countable as seen from outside the model.

9 For a comparison of Quine's holism with Duhem's thesis, see Donald Gillies *Philosophy of Science in the Twentieth Century*, London 1993.

10 For an authoritative exposition of Skolem's paradox –although with the same usual interpretation–, see H. D. Ebbinghaus, J. Flum and W. Thomas, *Mathematical Logic*, pp. 108 and specially 112-113.

Putnam argues[11] that the Skolem Paradox establishes that no interesting theory (in the sense of 'first order theory') can, by itself, determine its universe of objects up to isomorphism. Moreover, he adds[12] that Skolem's argument can be extended to show that if the theoretical constraints do not determine the universe of discourse, then possible additional restrictions of an operational nature cannot determine it either. Putnam even maintains[13] that such an argument shows that a formalization of all science, or of all our beliefs, could not eradicate undesirable countable interpretations.

We are not concerned here with an assessment of the negative impact of Skolem's paradox on moderate realism, but with the two conceptions of mathematical truth and reference that, according to Putnam, remain unaffected by Skolem's paradox, namely, Platonism and verificationism. Of special concern are the grounds that Putnam has for adopting verificationism and rejecting Platonism. The reason given by Putnam[14] to reject the Platonist stand is that it invokes 'non-natural' cognitive faculties, and this he considers 'epistemologically otiose and devoid of conviction as science'. It is interesting to observe that this same reason, namely, the rejection of cognitive faculties that he calls 'non-natural' -'natural' for him would probably be only sense perception with some sort of causal link between the knower and the objects of knowledge-, makes him avoid a much less artificial rendering of the so-called Skolem Paradox, namely, that first order languages -and, in general, all those languages in which the Löwenheim-Skolem theorem is valid- are inadequate to formalize set theory, in an analogous, but not exactly identical way to that in which first order languages are inadequate to formalize arithmetic. In second order languages you can formalize both set theory and arithmetic much more adequately, since for such languages neither the Löwenheim-Skolem theorem nor any of its Tarskian variants are valid, and, hence, the Skolem Paradox cannot be construed for them. (Moreover, in the case of arithmetic, second order arithmetic is categorical, i.e., all its models are isomorphic, whereas first order arithmetic, thanks to the compactness theorem, has non-standard countable models, i.e., countable models not isomorphic to the stan-

11 Models and Reality, pp. 442-443.

12 Ibid., p. 423.

13 Ibid., p. 423.

14 Ibid., p. 430.

dard model, as was shown precisely by Skolem.) Indeed, we should not forget that the Löwenheim-Skolem theorem and its Tarskian variants are limitative results, which establish the non-categoricity of theories. Of course, second order languages have other probably undesirable properties like non-compactness and semantic incompleteness. However, decidability, e.g., seems to be a desirable property, and propositional languages are decidable, whereas (full) first order languages are not. But that has not hindered logicians to prefer first order languages to propositional ones because of their expressive power. This same ground could be given to prefer second order languages to first order ones. However, no matter how this rivalry between first and second order languages (and possibly others) is finally decided, the fact is that Skolem's paradox has a parochial nature, and this fact weakens considerably Putnam's argumentation in 'Models and Reality'. Thus, even if his argumentation against moderate realism on the basis of the so-called Skolem Paradox were correct, his preference for verificationism over Platonism, and his rejection of a much more natural rendering of the paradox than the one he adopts from Skolem are based only on the prejudice against the existence of abstract mathematical entities and our epistemological access to them.

§3 The Quine-Putnam indispensability argument

Let us consider now the belief that the only (or, at least, the best) argument on behalf of realism in mathematics is the success in the application of mathematics to physical science. According to this view, as Putnam tells us in 'What is Mathematical Truth?'[15], realism in the philosophy of mathematics is based both in mathematical experience and in physical experience, and 'the rendering under which mathematics is true has to be compatible with the application of mathematics outside of mathematics'. [16] Moreover, Putnam even maintains[17] that, in view of the integration of mathematics with physics, it is not possible to be a realist with respect to physics but a nominalist with respect to mathematics.

First of all, it sounds somewhat queer that mathematics, the most exact of all sciences, with the possible exception of logic -if it is sound to

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15 'What is Mathematical Truth?', p 73.

16 Ibid., p. 74.

17 Ibid., p. 74.

distinguish between them-, has to justify the truth of its theorems and the existence of the entities about which she presumably speaks, by referring to the success of its applications to physical science. As observed by James R. Brown in his paper 'Π in the Sky'[18], the truth of elementary arithmetical statements as, e.g., 'There is an immediate successor of 3 in the natural number series', or '2 is less than 3' is much more evident than the truth of any statement in the physical sciences. Something analogous occurs with elementary statements about sets, e.g., 'If the set M contains x and y as its sole members, and the set C contains y, v and w as its sole members, then the intersection of M and C contains y as its sole member, and the union of M and C contains x, y, v and w as its only members'. Moreover, such statements are not only much more evident than the laws of physics, but also seem to be true in all possible worlds. Indeed, an existential mathematical statement as, e.g., 'There is a prime number greater than 100', whose truth seems to convey the existence of the objects spoken about, seems to be true in every possible world and, thus, such objects seem to exist in every possible world. On the other hand, statements in the physical sciences seem to be only contingently true, since it is not really difficult to imagine possible worlds governed by different physical laws.

It should be mentioned here that the Quine-Putnam tradition seems to have an inadequate understanding of the role of mathematics in physical science. As has been correctly argued by James R. Brown in the above mentioned paper[19], Quine's claim that not only physical science, but also mathematics, is tested by experience via the so-called observational sentences, seems not to be warranted by the history of science. In that history, Brown argues[20], when there is an unexpected mathematical result, scientists have never concluded that it is the mathematics used (or part of it) that has been falsified and requires modification. They have concluded correctly that it is the physical theory (or part of it) that has been falsified. Brown claims against Quine[21] that the role of mathematics in physical science does not consist in constituting additional hypotheses, but in offering models (in the sense in which the word 'model' is

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18 'Π in the Sky', in A.D. Irvine, ed., *Physicalism in Mathematics*, 1990, pp. 95-120. See p. 98.

19 Ibid. , pp. 101-102.

20 Ibid., p. 102.

21 Ibid., p. 102.

used in the natural sciences). In the face of adverse empirical results the model is substituted by a more adequate one, without claiming that the mathematical theory has been falsified. This theory has simply been shown to be inadequate as a model for those features of the physical world that the scientist wanted to explain. Thus, e.g., it is not the case that Euclidean geometry has been falsified by general relativity. It has simply been shown that Euclidean geometry is not an adequate model for the description of the spatio-temporal structure of our physical universe. (We are not claiming here -and Brown most surely was not- that mathematics is unrevisable. The antinomies of naive set theory clearly yielded a revision in mathematics. But revisions in mathematics are internally motivated, not the result of any empirical testings. Of course, there are also shifts of interest in mathematics, and these can be partly motivated by external sources. But shift of interest is not revision. A mathematical theory can loose its interest, e.g., if from the theoretical standpoint there is not much still to be discovered, or its results are surpassed by a more general theory. It could also be the case that the physical theories to which it is applied are falsified or lose interest, in which case there is an additional, but not decisive reason, for the abandonment of research in the mathematical theory.)

Let us return to the Quine-Putnam argument, and compare it with the following similar fictional situation. Someone wants to argue that words have meaning and that statements express thoughts. However, instead of arguing directly, he claims that if it were not the case that words have meaning and that statements express thoughts, it would not be possible to explain how it is that there is literature and that it can be read and 'understood' by different people. Now, languages have existed, in their oral manifestations, long before literature, as we conceive it, appeared on the face of the earth. It seems completely unreasonable to think that before the invention of literature words did not have meaning and statements did not express thoughts, or, at least, that there was no way to establish that words have meaning and that statements express thoughts. Indeed, it is not hard to imagine a possible world in which people communicate orally as well or as badly as we do in our world, although in that possible world there is no literature. What is not possible is a world in which there were literature but there were no languages. In a similar way, the historical origins of mathematics can be traced back many centuries before the advent of physical science, which, as is conceived nowadays, seems to trace its origins up to Galileo (and the physical theories currently considered as true are products of the last two centuries). Now, although applicable to the physical (and other) sciences, mathematical theorems seem to be true even if all actually accepted physical theories were false and, thus, the claim that only after the advent of modern physical science can we argue that mathematical theorems are true seems really amazing, to say the least. It is also extremely unreasonable to think that before the advent of modern physical science there was no way to establish the existence of mathematical entities, thus, e.g., that there exists an immediate successor of 3 in the natural number series. Moreover, it is perfectly conceivable that there exists a world in which all mathematical theorems known to present day mathematicians are true (supposing that current mathematics is consistent), and that mathematicians know as much mathematics as they actually know, but in which none of the physical laws accepted as true nowadays were known to humanity. What is not possible is a world in which physical science were as developed as it actually is, but in which our present mathematical theories (especially those applicable to present day physical science) were not valid, or, at least, were not considered to be valid. If we are going to argue on behalf of the existence of mathematical entities or of the truth of mathematical theorems, we have to do it, as, e.g., Brown correctly maintains[22] from within mathematics itself.

Quine's and Putnam's belief that the indispensability argument represents the only (or, at least, the best) argument on behalf of mathematical realism and the truth of mathematical theorems, is based on an inadequate view both of science and of our cognitive capacities. His criticism of logical empiricism notwithstanding, Quine defends a sort of empiricism that is also incapable of doing justice to the eminently theoretical character of physical science. His whole conception of knowledge and experience originates in a behaviorism of doubtful scientific credentials. The whole epistemological tradition that originates with Quine, and of which Putnam and Benacerraf are prominent members, limits our cognitive capacities to sense perception, possibly garnished with a causal dressing. Its rejection of any other argument on behalf of mathematical realism is bounded to their fear of admitting what Putnam, in an axiologically loaded terminology, has called 'non-natural mental powers' and 'mysterious mental powers' -but which could be more cor-

22 Op. cit., p. 99.

rectly called 'categorial intuition' or 'intellectual intuition'-, or even of overtly admitting the existence of abstract mathematical entities. This is the reason why in 'On what there is'[23] Quine refers to mathematics (and even to physics) as a convenient myth, as a useful fiction of high explanatory value. Indeed, according to Quine, we cannot free ourselves of theoretical fictionalism as soon as we transcend the so-called sensory data, without taking into account that precisely these pure sensory data are the first great fiction. (As has been argued by many authors[24], it is extremely difficult to isolate constituents of physical theories that are not theoretically contaminated.) On the other hand, Putnam made more explicit than Quine his reluctance to admit the existence of abstract mathematical entities when in 'What is Mathematical Truth?'[25] he defended a sort of realism without mathematical entities. Prima facie, it looks as if the indispensability argument -contrary to other possible arguments on behalf of mathematical realism- would allow Putnam to defend a sort of mathematical realism, without having to admit the existence of numbers, sets and other 'undesirable' entities. (We are not going to dwell here on the issue of the cogency of such a mathematical realism, which has since been abandoned by its proponent.) What seems much clearer, however, is that Quine's view of mathematics as a convenient myth, together with his adhesion to the indispensability argument and Putnam's view of a mathematical realism without mathematical entities paved the way for Field's philosophy of mathematics.

§4 Paul Benacerraf's ontological arguments

In 'What Numbers could not be'[26] Paul Benacerraf has claimed that since there is more than one way -possibly infinitely many ways- to characterize numbers as sets, e.g., von Neumann's characterization and Zermelo's characterization, and since they possess incompatible properties, numbers cannot be sets. Benacerraf argues[27] that at most one of

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23 'On what there is', p. 18.

24 See, e. g., Mario Bunge, *Philosophy of Physics*, 1973, pp. 3 and 226. See also Dudley Shapere, *Realism and the Search for Knowledge*, 1984, specially Ch. 16. For some recent discussion on the nature of physical theories, see, e.g., W. Balzer, D. A. Pearce and H. J. Schmidt, eds, *Reduction in Science*, 1984.

25 'What is Mathematical Truth', pp. 69-74.

26 See footnote 3 above.

27 Ibid., pp. 284-285.

those characterizations, which are not even extensionally equivalent, can be true. But since there is no ground to prefer one of them over any other, numbers cannot be sets. A second argument of Benacerraf in the same paper attempts to show that numbers cannot be objects. On this point he refers both to Russell and Quine, who had conceived arithmetic as the study of recursive progressions. The only numerical properties that would seem relevant for arithmetic are those had in virtue only of being members of a recursive progression. No single numerical property that would individualize them as particular objects would be relevant for arithmetic. Hence, Benacerraf concludes that numbers are not even objects.

Benacerraf's two ontological arguments should be clearly distinguished, since they are not only different, but have different purposes. Let us consider the first one. The fact that there are various characterizations of numbers by means of different sets is not an unusual event in mathematics. As, e.g. Field has observed[28], the real numbers are sometimes identified with Dedekind cuts and sometimes with equivalence classes of Cauchy sequences. In the same vein, ordered pairs, topological spaces and other mathematical entities can be characterized in different ways. Mathematicians, Platonists or not, recognize the existence of such a variety of characterizations without extracting from those situations any Benacerrafian argument. In particular, the fact that in different axiomatizations of set theory we can show that entities characterized in very different ways possess the properties that we usually attribute to numbers, although they do not have any other property in common, does not allow us to conclude anything about the nature of numbers. The situation described by Benacerraf is not very dissimilar to that which occurs when two radically different senses have the same referent, as is the case of the senses of the following two definite descriptions : 'the French leftist who was Leon Trotsky's secretary from 1932 to 1939' and 'the mathematician and historian of logic who edited *A Source Book in Mathematical Logic'*. (If we consider not the definite descriptions but the corresponding conceptual expressions, the similarity seems even more plausible. The referent is in that case -following Husserl or Carnap, not Frege- the same unit set.) The fact that Jean van Heijenoort can be

28 See 'Fictionalism, Epistemology and Mathematics' in his *Realism, Mathematics and Modality*, pp. 1-52, especially pp. 20-21 See also Saunders Mac Lane's *Mathematics : Form and Function*, p. 106.

referred to in such extremely different ways, and that his life from more or less 1948 onwards has almost nothing to do with his former life (except for serving as a consultant for the Trotsky archives) -which makes him an excellent choice to illustrate Kripke's puzzle in 'A Puzzle about Belief'- does not allow us to extract any ontological conclusions about van Heijenoort. It could well be the case that numbers were sets, or that they were any other kind of abstract entity, and that due to our cognitive limitations we would be forced to characterize them by means of sets which, apart from the properties that they have in common with numbers, do not possess any other property in common and, thus, are both inadequate but 'manageable'. (Moreover, it is possible that Benacerraf is going too far when he attributes to set-theoretical reductionists the identification of numbers with particular sets. Indeed, in his paper reproduced in Benacerraf's and Putnam's anthology Carnap underscores[29] that the logicist merely produces, by means of explicit definitions in a system of logic, constructions of logical objects that, in virtue of those definitions, have the properties that numbers have. Perhaps, if asked by Benacerraf to comment on the situation under discussion, both Zermelo and von Neumann would have answered : 'This is the way in which numbers are represented in my system and that is the way in which numbers are represented in his system. I am not attempting to tell you what numbers really are'. Even in Frege's *Die Grundlagen der Arithmetik* there is a passage[30] in which he admits that his identification of the number 0 with the set which is the extension of the concept 'different from itself' involves some arbitrariness, since prima facie the extension of any other concept under which no object falls could have been identified with the number 0. On the basis of that passage someone could try to render Frege's endeavor not as claiming to have shown that the number 0 is the extension of the concept 'different from itself', but as claiming that by means of that and the subsequent definitions, one can construe a system of logical objects for which one can prove, from logical axioms only, all the properties usually assigned to numbers. It should be clear that we are not arguing here for such an interpretation of Frege, but merely indicating that it would have textual evidence from that passage.)

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[29] See R. Carnap's 'The Logicist Foundations of Mathematics', p. 43, in P. Benacerraf and H. Putnam, eds., *Philosophy of Mathematics*, 2nd edition, pp. 41-52.

[30] G. Frege, *Die Grundlagen der Arithmetik*, p. 88.

Now, even if Benacerraf had succeeded in showing that numbers are not sets, that would not allow him to conclude that numbers are not objects. It could well occur that numbers were not sets, and even that there were no mathematical entity to which all others were reducible, but that there were various fundamental mathematical entities, e.g., numbers, sets, relations, functions, etc., and that, because of their level of abstraction, it were possible to characterize each of them in one or more ways in terms of each of the others. Thus, e.g., relations could be characterized, following Frege, as functions of the same number of arguments whose value is a truth value, and, on the other hand, functions of n arguments could be characterized as relations of n+1 arguments uniquely determined in their last arguments. Moreover, relations could also be characterized as sets of ordered pairs, and, on the other hand, sets could be characterized as relations of a single argument. Non-fundamental mathematical entities could then be characterized either as specializations of the fundamental entities, or as combinations of them, or as combinations of their specializations, or as equivalence classes of some already recognized entities.

With respect to Benacerraf's claim that numbers are not objects, since arithmetic is the theory of recursive progressions, and, thus, only structural properties are relevant for arithmetic, i.e., properties that do not individualize them, the following comments seem appropriate. First of all, it is not sufficiently clear from Benacerraf's paper in which sense is the system of natural numbers indistinguishable from other systems of (as Benacerraf would say) 'supposed' objects that constitute recursive progressions. He could be thinking of some kind of formalization of Dedekind-Peano arithmetic, or of something like the hierarchy of (species of) structures that constitutes mathematics according to the school of Nicolas Bourbaki. Let us suppose first that Benacerraf is considering some kind of formalization of Dedekind-Peano arithmetic in a system of logic. Then you have to distinguish at least two cases, namely : (1) the formalization is made in first order logic, or (2) the formalization is made in second order logic. In this last case we obtain a categorical theory, i.e., a theory all of whose models are isomorphic, but the theory would have -at least according to the Quinean tradition- a strong ontological commitment not to the taste of Benacerraf. On the other hand, if the formalization is made in first order logic, the theory is not categorical -not even $\aleph_0$-categorical (because of a theorem of Skolem)-, and, thus, it would have models non-isomorphic to the standard one -even some of cardinality $\aleph_0$. In that case, to determine the standard model completely and exclude such undesirable models, one has precisely to take into account those properties of the system of numbers that would distinguish it from the other models.

On the other hand, if Benacerraf is thinking that the system of natural numbers is a particular case of a structure of recursive progression in the same way in which any group is a particular case of the group structure as determined by the group axioms, then he is not justified in denying the existence of numbers. Of course, a group G and a group G* share the same group structure. But that does not mean that they do not exist -in the sense in which Platonists conceive the existence of mathematical entities. Indeed, the fact (if general relativity is true) that the space-time in which we live has the structure of a particular Riemannian manifold of four dimensions with variable curvature does not allow us to conclude either that physics is the study of Riemannian manifolds of four dimensions with variable curvature, or that the space-time in which we live and the space-time points of which it is constituted do not exist. There are various thousands of copies of Benacerraf's and Putnam's *Philosophy of Mathematics*, and all of them have the same 'structure', since they share all the 'relevant' properties (for potential readers of the book). By a reasoning similar to that applied to numbers, Benacerraf should conclude that none of the copies of his and Putnam's book exist. And if a mischievous Platonist genetic engineer succeeded to produce an exact human copy of Paul Benacerraf, by a reasoning similar to that applied to numbers, Benacerraf should conclude -his Cartesian cogito notwithstanding- that he really does not exist. Of course, Benacerraf will not extract in such cases the conclusion analogous to the one he extracted in the case of numbers. But this difference brings to the fore the hidden ground behind Benacerraf's argumentation, namely, his prejudice against the existence of mathematical entities. Thus, Benacerraf has not established that numbers are not objects : he has presupposed that they are not.

§5 Benacerraf's epistemological argument

In his excessively influential paper 'Mathematical Truth'[31] Benacerraf has claimed that treatments of the nature of mathematical truth have been motivated by two different kinds of concerns that are not easy to reconcile, namely : (1) the concern of having an homogeneous semantic theory, in which the semantics of mathematical statements parallels that of non-mathematical statements, and (2) the concern of the compatibility of the treatment of mathematical truth with a reasonable epistemology. Benacerraf considers that there is only one adequate semantic treatment of mathematical statements that is similar to that of non-mathematical statements, namely, the one offered by Tarskian semantics. This claim, however, does not seem completely correct. With respect to natural languages, Kripke has shown that Tarski's semantics is not completely adequate -as Tarski himself very well knew, but some so-called Tarskians had forgotten-, and he has proposed a more adequate one that seems to have been almost immediately superseded by the Gupta-Herzberger-Belnap revision theory of truth. (The relation between this last theory and Tarski's theory of truth applied to natural languages seems to be similar to that between relativity theory and Newtonian mechanics, namely, the Tarskian theory is false in the light of the Gupta-Herzberger-Belnap theory, but in very special limiting cases they coincide.) On the other hand, although Tarskian semantics seems, in general, to be adequate for formalized languages, we are convinced that as soon as a substantial portion of mathematics is formalized, Tarskian semantics will require some modification to do justice to the fact that in mathematics there are many statements that are mathematically equivalent, although they seem to be theoretically unrelated and even belong to somewhat distant areas of mathematics.

Our interest here, however, is in the other sort of concern mentioned by Benacerraf and presented by him as a sort of requisite, namely that the treatment of mathematical truth be compatible with a reasonable epistemology. In other words, Benacerraf claims[32] that an acceptable semantics should be compatible with a reasonable epistemology. But reasonable epistemology is for Benacerraf[33] one that admits essen-

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[31] See footnote 4 above.

[32] 'Mathematical Truth', p. 409.

[33] See p. 409.

tially sense perception as the sole form of knowledge, but that, according to him,[34] differs from former empiricism because it has integrated a causal component. Thus, for someone to know that some particular statement is true there has to exist a causal relation between that person and the referents of the names, predicates and even quantifiers[35] occurring in the statement. Moreover, Benacerraf also believes in a causal theory of reference, as that propounded by Kripke and Putnam. Specifically, Benacerraf claims[36] that in the case of medium sized objects there should exist a direct causal reference to the facts known and to the objects that constitute them, whereas other sorts of knowledge, among which he includes our knowledge of laws and general theories, should be explained as based in some way or other on our knowledge of medium sized objects. It should be clear from such a characterization of a 'reasonable epistemology' that it practically excludes per definitionem the possibility of having knowledge of abstract mathematical entities. But since Benacerraf requires[37] of the truth of mathematical statements - which, at least, in the case of existential statements seem to involve the existence of the entities spoken about- that they do not make it impossible that at least some of the mathematical truths be known according to the canons of his 'reasonable epistemology', and since abstract mathematical entities are not causally related to us, his requirement also practically excludes per definitionem their existence.

Now, it seems very strange that the existence of mathematical entities and the truth of mathematical theorems (e.g., existential ones) be in jeopardy on the basis of an epistemological view, since in the history of philosophy not a single epistemological theory has succeeded in establishing itself with the firmness of at least the theories in the less developed areas of the natural sciences. More strange, however, is the fact that, as John P. Burgess has argued in his 'Epistemology and Nominalism'[38], one of the authors cited by Benacerraf as a propounder of the causal theory of knowledge, namely, Alvin I. Goldman, has later questioned his own causal theory of knowledge, and -what is much more im-

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[34] See p. 413.

[35] See p. 413.

[36] Ibid.

[37] Ibid., p. 409.

[38] 'Epistemology and Nominalism', in A. D. Irvine, ed., *Physicalism in Mathematics*, pp. 1-15. See p. 6.

portant-, contrary to what Benacerraf does in 'Mathematical Truth', had limited his causal theory to contingent knowledge, in contrast to necessary knowledge and, thus, as Burgess comments, seems to have excluded the application of the causal theory to mathematics. Benacerraf's convenient misinterpretation of Goldman is exclusively motivated by his desire to exclude abstract mathematical entities from any possible epistemological access. (Indeed, even Field -of all people- in 'Fictionalism, Epistemology and Modality'[39] recognizes that nowadays nobody believes in a causal theory of knowledge as that on which Benacerraf bases his epistemological critique of mathematical Platonism. Nonetheless, as we shall see below, Field commits himself in his writings to some sort of modified causal theory of knowledge, when he admits the Quine-Putnam indispensability argument as the only possible argument on behalf of realism in mathematics.)

But the causal theory of knowledge is an inadequate epistemology even for the physical sciences. First of all, as was observed by James R. Brown in 'Π in the Sky'[40], such a view has difficulties with generalizations. Indeed, it seems very difficult to reconcile an epistemology based on sense perception, together with a causal component and possibly some sort of induction, with the laws of high generality and the sophisticated theories of physics. The causal dressing does not add anything to the solution of the difficulties of a similar character that have haunted other variants of empiricism and, specially, logical empiricism, in their attempts to explain the nature of physical theories. Empiricism, with or without a causal dressing, is a non-starter in the philosophy of physical science. On the other hand, as has been underscored by various authors, such a view results specially inadequate in microphysics. As Michael D. Resnik comments in 'Beliefs About Mathematical Objects' [41], physicists sometimes theorize about physical particles before having the least empirical evidence about them.[42]

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[39] Op. cit., p. 25.

[40] Op. cit., p. 100.

[41] 'Belief About Mathematical Objects', in A. D. Irvine, ed., *Physicalism in Mathematics*, pp. 41-71. See, specially, pp. 45-46.

[42] On this issue, see also James R. Brown's detailed argumentation both in "Π in the Sky', pp. 111-118 and in Ch. 5 of his *The Laboratory of the Mind*. However, the situation on which Brown bases his argumentation does not seem as clear as Brown would like.

On the other hand, even in macrophysics scientists often postulate the existence of physico-geometrical entities, as the so-called singularities of space-time in general relativity and cosmology- e.g., the center of black holes-, which presumably have causal effects but with which the possibility of being directly causally connected seems excluded, and even indirect causal connections seem theoretically loaded. Now, if the causal epistemology presupposed by Benacerraf in his rejection of any possible access to abstract mathematical entities seems to be inadequate to explain our knowledge of physics, it's inadequacy to explain mathematical knowledge should be even more evident, since mathematical knowledge is both more abstract and firmer than physical knowledge. Moreover, even if there were some causal connection present in mathematical knowledge, it would have a contingent nature and, thus, would not be decisive for a discipline that seems to produce necessary knowledge. Once again, only the prejudice against the existence of mathematical entities and our possible cognitive access to them constitute the real ground for Benacerraf's argument.

We have shown that the four cornerstones of current discussion in the philosophy of mathematics are really empiricist dogmas of a new sort, based essentially on the prejudice against full blown ontological and epistemological Platonism. Although we have considered them separately, those dogmas are in some sense intertwined and tend to reinforce each other. We will now briefly examine the impact of the above displayed argumentation on the most daring of current philosophies of mathematics, namely, Field's programme.[43]

§6 The consequences for Field's programme

Hartry Field's philosophy of mathematics is, in some sense, the culmination of that philosophical current originating in Quine, Putnam and Benacerraf that we have been considering, since it boldly extracts the ultimate consequences from its framework of shared beliefs. If the only argument on behalf of realism in mathematics and on behalf of the truth of mathematical statements is the indispensability argument, if mathe-

[43] We will not attempt to deal here with all aspects of Field's views, but only with those relevant to the foregoing discussion, which are without doubt the most central. Thus, e.g., nothing is said about Field's treatment of modalities in some of his recent papers and very little about his discussion with Hale and Wright.

matics is -as Quine said in 'On what there is'[44]- a convenient fiction, if any epistemological access to abstract mathematical entities is excluded, since they do not relate causally with us, and if we cannot characterize numbers uniquely and do not even know if they are objects at all, then why not explicitly conclude that such entities do not exist, and that existential mathematical statements are strictly false, even though they are useful devices in the derivation of physical statements, but without any ontological import?

Prima facie, however, Field's views seem to abandon the Quine-Putnam tradition, and in some details he even clashes with it. Nonetheless, most of his views are really radicalizations of ideas to be found in Quine, Putnam and Benacerraf. As is well known, Field claims that mathematics does not need to be a corpus of truths to be successfully applied to the physical sciences. Moreover, he argues that existential mathematical statements are all false (and the universal ones just vacuously true), since there do not exist the mathematical entities whose existence would make such statements true. According to Field, the property that mathematical statements need to have in order to be successfully applied to the physical sciences is not truth, but conservativeness, which is a strong form of consistency. (Mere consistency would be insufficient, since a theory can be consistent and still imply false consequences about the physical world. [45]) A mathematical theory is conservative if it is consistent with any internally consistent theory about the physical world. In other words, a mathematical conservative theory, when added to the corpus of physical statements does not imply any physical statement not implied by the corpus of physical statements - even though, in practice, the obtention of that statement would be much more complicated without the help from the mathematical theory. (It should be mentioned here that Field conceives the role of mathematics in physical science in a very similar fashion to Quine's and, thus, Brown's critique of Quine mentioned above, would also apply to Field.)

Field's view of mathematics has probably been the most discussed and criticized in recent years by people working in the philosophy of mathematics in the Anglo-American world, and many of these critiques,

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44 'On what there is', pp. 17-18.

45 See 'Realism and Anti-Realism about Mathematics', in *Realism, Mathematics and Modality*, pp. 53-78, specially p. 55.

e.g., those by Hale, Burgess, Brown and Irvine, seem adequate. Thus, e.g., Burgess seems to be correct when he claims that Field's program -which is really just a project- is not only of very improbable success, but even if successful in its nominalist reconstruction of the physical sciences, such a reconstruction would be of little interest to physicists. Indeed, as Burgess puts it in 'Epistemology and Nominalism'[46], ontological economy does not seem to have played any important role in the history of science. On the other hand, as many have argued[47] and, moreover, is sufficiently clear, Field's views conflict with the manifest evidence of many elementary mathematical theorems, e.g., arithmetical theorems that seem to be not only true and even necessarily so, but also trivially so. One would have to offer extremely good reasons to at least consider the possibility that they were not necessarily true and that their manifest obviousness is merely an illusion. However, ontological economy and the rejection of abstract mathematical entities seem to be very poor reasons to embrace such an antiintuitive enterprise with such a low probability of success as Field's. Nonetheless, we are not interested here in criticizing Field's views directly, but will try to argue that they presuppose the truth of both Quine's and Putnam's claim concerning the indispensability argument and of Benacerraf's ontological and epistemological arguments. Hence, with the collapse of the foundations, the whole structure will fall down, including Field's views.

First of all, it should be clear from Field's writings that the only argument on behalf of mathematical realism that he takes seriously is the Quine-Putnam indispensability argument. On the other hand, as can be seen from his discussion of Benacerraf's ontological argumentation in 'Fictionalism, Epistemology and Mathematics'[48], Field accepts Benacerraf's ontological arguments and extracts from them the strong conclusion that, since numbers are not objects, they do not exist at all. With regard to Benacerraf's epistemological argument, it should be said that, notwithstanding the above mentioned passage of 'Realism and Anti-Realism about Mathematics', in which Field says that nobody believes anymore in the causal theory of knowledge propounded by Benacerraf, it should also be clear that he presupposes some sort of causal theory of

46 Op. cit., pp. 11-12.

47 E. g., Irvine in the Introduction to *Physicalism in Mathematics*, p. xiii.

48 'Fictionalism, Epistemology and Mathematics', pp. 20-22.

knowledge. Thus, in 'Fictionalism, Epistemology and Mathematics'[49] he argues that there exists a remarkable difference between the roles played by mathematical and physical entities in our explanations of the physical world. The role played by physical entities in those explanations is usually causal, namely, as causal agents that produce the phenomena that are to be explained. But since mathematical entities are supposed to be acausal, their role in such explanations has to be different. Now, Field confesses[50] that explanations which involve reference to entities not causally connected with the phenomena to be explained seem to him rather strange. Here we have a possibly somewhat more cautious version than Benacerraf's of the causal theory of knowledge.

Now, in the foregoing sections of this work we have shown that the Quine-Putnam thesis with respect to the indispensability argument, as well as both Benacerraf's epistemological argument and his ontological arguments (and also Putnam's skolemization argument) rest on the unwarranted assumption that we cannot have epistemological access to abstract mathematical entities, and even on the stronger assumption that mathematical entities do not exist. But since Field's programme presupposes both the validity of Benacerraf's arguments and the Quine-Putnam claim that the indispensability argument is the only argument on behalf of mathematical realism that is worth considering, the pillars of Field's views turn to be extremely shaky, and this devoids such a desperate philosophy of mathematics of any interest or atractiveness (except perhaps as a technical exercise in logical ingenuity).

However, not everything said by Field about mathematics is devoid of interest (as is to be expected of such an ingenious author). There are some interesting points made by him that should be mentioned here. Thus, e.g., we think that Field is correct when he asserts that the so-called Frege-Wright argument, based on the fact that in the languages usually known to logicians numerical expressions are singular terms, is insufficient to establish mathematical realism. Nonetheless, the analogy used by Field between the presumed mathematical fiction and literary fiction ignores completely the manifest necessity with which some elementary mathematical statements impose themselves upon us, what makes us suspect that the so-called mathematical myth would be not merely a convenient one -as Quine and Field would like us to believe-,

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49 Ibid., p. 19.

50 Ibid., pp. 18-19.

but a necessary one. On the other hand, Field seems to have better grasped the difference between mathematics and physics than Quine and Putnam when he claims that mathematics is conservative but physics is not, although his grounds for such a claim are, of course, different from those that we would give. In our case it is simply that, as Bob Hale puts it in his paper 'Nominalism'[51], if you consider that mathematical theorems are necessarily true, then you are committed to the conservativeness of mathematics. Field's claim that conservativeness and truth are mutually independent also seems convincing, since, as he argues, physics is not conservative but is probably true—if present physics is not true, one can expect that at some time in the future the physics considered true at that moment will really be true—, and, on the other hand, conservativeness does not imply truth either, since ZF+AC and ZF+¬AC seem both to be conservative but only one could be true. Finally, it should be pointed out that by claiming that mathematics is conservative but physics is not, and, thus, by being a nominalist (or fictionalist) with regard to mathematical existence but a realist with regard to that of physical entities, Field separates himself from the tradition on which he dwelled, since, contrary to that tradition, which considers mathematics (and even logic) as continuous with physics and, thus, as an empirical science, he clearly states in 'Realism and Anti-Realism about Mathematics'[52] that he cannot accept that mathematics is continuous with physics. Once more we coincide with Field, but for different reasons, namely, since for us mathematical theorems seem to be necessary whereas physical statements are not. It is interesting to observe, on this issue, that if Field's views are consistent, he turns to be a counterexample to Putnam's claim in 'What is Mathematical Truth?'[53] that it is not possible to be a realist with regard to physical entities but a nominalist with regard to mathematical ones.

§7 OPENING THE DOORS

Let us conclude this paper with some comments on our knowledge of mathematical entities. We will follow closely the little known episte-

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[51] 'Nominalism', in A. D. Irvine, ed., *Physicalism in Mathematics*, pp. 121-144. See p. 123.

[52] 'Realism and Anti-Realism about Mathematics', pp. 59-61.

[53] 'What is Mathematical Truth?', p. 74.

mology of mathematics developed by Edmund Husserl in his masterpiece Logische Untersuchungen[54], specifically in the Sixth Investigation.

We usually say that an empirical statement like 'The ball is red' or 'Joe is taller than Charles' is true or false, depending on how things are in the world, thus, depending on which states of affairs hold. Statements, pace Frege, refer to certain states of affairs, and are true if those states of affairs hold, and false if they do not hold, e.g., if the ball under consideration is not red but green. Sense perception presents us with the existing states of affairs, and in this respect confirms or disconfirms the proposition expressed by the statement. But in sense perception only physical objects and sensible properties are given. Only the 'material' constituents of statements, namely, terms and predicates, have 'correlata' in sense perception (or even in imagination). Particles like the connectives 'and', 'or', 'if..., then', the quantifiers, and even expressions like 'is larger than' have no correlata in sense perception. Nonetheless, we usually say that we can empirically confirm or disconfirm statements which contain such particles, and we recognize the difference in truth conditions between the statements 'Mary and Julia are in the park' and 'Mary or Julia is in the park'. Constituents of statements that do not have any correlata in sense perception can be called 'formal constituents of statements', and the act of knowledge that fulfills such a constituent of statements is not a simple sense perception, but a categorial perception. Categorial perception builds on sense perception, but does not reduce to sense perception, and makes us acquainted with categorial objects. Categorial perception does not modify the underlying sense perception, since that would be a distortion that would produce another sensible object, but rather leaves untouched the sense perceptions on which it is founded and in which the sensible objects are given to us on which the new objectualities are built.

Husserl considers sets and states of affairs as examples of categorial objectualities. Hence, e,g., the state of affairs that Joe is taller than Charles is a categorial objectuality that builds on and structures itself on the sensible objects Joe and Charles given in sense perception. The state of affairs that Joe is taller than Charles is a categorial objectuality and, indeed, a different one from the state of affairs that Charles is shorter than Joe. (To both of them underlies the proto-relation, called by Husserl

54 *Logische Untersuchungen*, 1900-1901, reprinted as Vols. XVIII, 1975 and XIX, 1984, of the Husserliana edition.

'situation of affairs', that Joe has a bigger size than Charles. We will not comment here on Husserl's notion of a situation of affairs, since it is not required to understand what follows.[55]) Analogously, given various objects in sense perception, in categorial perception we are given the set of those objects. Such a set is not given to us in sense perception—we do not see it with our own eyes nor touch it—, but rather builds on what is given by sense perception and is—to use Husserl's terminology—'constituted' in categorial perception. The set is a new objectuality founded on sensible objects, and given to us in a categorial perception founded on sense perception. (Instead of 'perception', from now on we will frequently say 'intuition'—both sensible and categorial—, and, thus, following Husserl, include also the imagination, since for most of our present purposes an imaginative act can play the same role as a perceptual act.)

We have seen that categorial perception is founded on sense perception but does not reduce to it, and that categorial objectualities are founded on sensible objects but do not reduce to them. Now, once categorial objectualities of this first level—like sets or relations—are given to us, new categorial intuitions can be built on the corresponding categorial intuitions of first level, and in such categorial intuitions of second level new categorial objectualities of second level are constituted—e.g., relations between sets, say bijections between sets, and also sets of relations, sets of sets (as, e.g., the power set of a given set), and so forth. In this way, repeating this process indefinitely, a hierarchy of categorial intuitions is obtained and a corresponding hierarchy of categorial objectualities is given to us, so that in categorial intuitions of the nth level categorial objectualities of the nth level are constituted.

From what has been said up to now, it does not seem clear how it is that the objects of pure mathematics are independent of experience, since the categorial objectualities given in the different levels of the hierarchy of categorial intuitions seem to be founded on sensible objects. Indeed, there are categorial objectualities that seem to possess a sensible component, and Husserl calls them 'mixed categorial objectualities'. An additional component plays a decisive role in the constitution of mathematical objectualities, namely, categorial (or formal) abstraction, which should not be confused with generalization. One can say some-

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[55] For a discussion of this Husserlian notion, see our papers of 1982, 1986 and 1991 included in the bibliography.

what schematically that for Husserl mathematical intuition is categorial intuition plus categorial abstraction. Categorial abstraction, as categorial intuition, is neither a mysterious nor a non-natural process, but something perfectly usual in mathematical knowledge. If we have a relation like that of being taller than between Joe and Charles, we can substitute the terms by indeterminates, thus, by variables, say 'x' and 'y', and the relation of being taller than by a transitive, asymmetric and irreflexive relation. Analogously, given a concrete set M, we can substitute its elements by indeterminates, and if we also substitute by indeterminates the concrete objects that belong to another concrete set C, and, moreover, we abstract, in general, from the peculiarities of the two sets, we can then consider bijections between one of those sets and a (not necessarily proper) subset of the other, and then consider their respective cardinalities.

It is in the manner described, namely, by means of categorial intuition purified by categorial abstraction that the basic mathematical objectualities are constituted. Other mathematical entities can then be obtained either by combining different objectualities to form more complex objectualities—as Husserl and Bourbaki have seen[56]—, or by means of the formation of equivalence classes based on a congruence relation—as discussed by Frege[57]—, or by other similar formal means. There is nothing non-natural or mysterious in this cognitive process, since categorial intuition builds on sensible intuition, and its objectualities build on and structure themselves on the objects of sense perception—which, after all, is even insufficient to give us a physical world coherently structured, as studied by the physical and biological sciences. On the other hand, categorial abstraction is a perfectly common procedure in mathematics, which is responsible for the level of formal generality attained by mathematics, as exemplified by universal algebra, general topology, category theory and other areas of contemporary mathematics. Without fear of paradox, it can be claimed that, although

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56 See Husserl's *Logische Untersuchungen*, Vol. 1, Ch. XI, and also his *Formale und Transzendentale Logik*, 1928, specially Part 1; reprinted in Husserliana Bd. XVII, 1974. See also our dissertation *Edmund Husserls Philosophie der Logik und Mathematik im Lichte der gegenwärtigen Logik und Grundlagenforschung*, 1973. For Bourbaki, see his 'The Architecture of Mathematics', in American Mathematical Monthly 57, 1950, pp. 221-232.

57 See his *Die Grundlagen der Arithmetik*, 1894, reprinted 1986 by Felix Meiner Verlag, Hamburg. See specially §§ 62-69.

categorial intuition builds on sensible intuition, there is no trace of sensible foundation in mathematical intuition[58].

APPENDIX I

In the course of our refutation of the skolemization argument—and elsewhere—we have argued that second order logic is more adequate than first order logic to render mathematical theories not only, in general, because of its superior expressive power, but, specifically, because it is immune to the cardinality-indeterminacy related to the Löwenheim-Skolem-Tarski theorems. However, it seems at first sight that second order logic is vulnerable to another, probably worst, form of indeterminacy. Second order logic admits some non-standard semantics besides the standard one. In particular, there is the Henkin non-standard semantics with its non-full models rivaling with the standard semantics, all of whose models are full models, i.e., they have as many relations and operations as possible. As is well known, for Henkin's and other similar non-standard semantic renderings of second order logic one can obtain a semantic completeness result with its not less famous corollaries, namely, compactness and the Löwenheim-Skolem-Tarski theorems, whereas for the standard semantic rendering such theorems are false. In what follows, we will argue that such an indeterminacy in the semantic rendering of second order logic is in an important sense an illusion and, moreover, that it is possible to obtain corresponding deviant semantic renderings for first order logic, namely, renderings under which first order logic would be decidable.

Firstly, there is a theorem of Per Lindström that establishes that there is no proper extension of first order logic for which both the Compactness theorem and the Löwenheim-Skolem theorem are true. Second order logic is clearly a proper extension of first order logic in any reasonable sense of the word 'extension' and, thus, at least one of these two theorems should be false for second order logic. This is precisely what occurs with second order logic endowed with its standard semantics. Under the standard rendering both the Compactness theorem and the

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58 For a much more detailed treatment of Husserl's epistemology of mathematics see our paper of 1987 included in the bibliography.

Löwenheim-Skolem theorem are false. Since, as we mentioned above, those theorems can be obtained for the Henkin and other non-standard renderings, it is a corollary of Lindström's theorem that such non-standard semantics are inadequate for second order logic. In fact, what such semantic renderings really do is to interpret second order logic in first order logic, for which, as is well known, the Compactness theorem and the Löwenheim-Skolem theorem (and also the Semantic Completeness theorem) are true.

Moreover, one can obtain for first order logic similar deviant semantic renderings, which essentially interpret first order logic in propositional logic and allow us to derive a decidability result for 'theoremhood' in such a 'first order' logic without contradicting Church's undecidability result. It is, however, a corollary of Church's theorem that such non-standard semantics are inadequate for first order logic (in the same fashion as Henkin's and other non-standard semantics are inadequate for second order logic).

To make our ideas somewhat precise, let us consider a first order language with the usual logical and auxiliary signs—we need not specify them except for the sign '$\neg$' for negation and the now usual sign for the universal quantifier '$\forall$'—, denumerably many individual variables x_1, $x_2,\ldots$, denumerably many monadic predicates A_1, $A_2,\ldots$, and for $n \geq 2$, denumerably many n-adic predicates R_1^n, $R_2^n,\ldots$. We can now proceed in any of two different ways to obtain a monadic rendering of the n-adic predicates. We can either establish a one to one correspondence between the n-adic predicate letters and the monadic predicate letters in a fashion similar to Cantor's one to one correspondence between the natural numbers and the rationals, or we can simply reinterpret the n-adic predicates as 'new' monadic predicates, deleting all but the first individual variable of any n-tuple of variables to which it was supposed to apply, or even deleting all individual variables and the corresponding auxiliary signs. In the first and second case we would have a rendering of first order logic in monadic first order logic, whereas in the last case the rendering would be in propositional logic. Thus, the formula $R_i^n(x_1, x_2,\ldots,x_n)$ of first order logic would be assigned the formula $A_k(x_1)$ under the first rendering, where A_k is the monadic predicate assigned to R_i^n under the one to one correspondence, the formula $R_i^n(x_1)$ under the second rendering, where R_i^n is seen just as a new monadic predicate, and the letter R_i^n, which is now simply a propositional variable, under the third rendering. In any of the three renderings, logical signs are rendered by themselves, as are also rendered the auxiliary signs (if not deleted, as occurs with many of them under the last rendering), and vacuous quantifiers are deleted. Thus, e.g., under the first rendering the closed sentence $(\forall x_1)...(\forall x_n)\ \neg R_i^n(x_1,...,x_n)$ is assigned to $(\forall x_1)\ \neg A_k(x_1)$, where A_k is the monadic predicate paired with R_i^n under the one to one correspondence, whereas it is assigned to $(\forall x_1)\ \neg R_i^n(x_1)$ under the second rendering and to $\neg R_i^n$ under the third rendering. One can easily establish that all three renderings are consistent in the sense that if T is a first order theory and T* its corresponding theory under any of the three renderings, and there is a formula ψ^* in L(T*) such that $\vdash \psi^*$ and $\vdash \neg\psi^*$ in T*, then $\vdash \psi$ and $\vdash \neg\psi$ in T, where ψ is the formula in the original first order language that corresponds to ψ^*. Hence, T* is consistent if T is. Moreover, one can easily show that the interpretation of a first order formula is a theorem of monadic first order logic under any of the first two renderings or of propositional logic under the third rendering if and only if the original formula is a theorem of full first order logic[59]. But both monadic first order logic and propositional logic are decidable. Thus, under any of these renderings first order logic would be decidable. This is a very similar situation to that which occurs when you give second order logic a non-standard Henkin interpretation[60]. To interpret an n-adic predicate by a monadic predicate is consistent, but you are not giving the n-adic predicate its full or adequate interpretation. As indicated above, Church's theorem can play for first order logic a similar task as Lindström's theorem for second order logic, namely, that of excluding such consistent but clearly deviant interpreta-

59 This argumentation is similar to that used in, e. g., Elliott Mendelson's classic *Introduction to Mathematical Logic* to establish the consistency of first order logic relative to the consistency of propositional logic.

60 Someone could argue that this is really a syntactic interpretation of a theory in another theory, whereas the Henkin interpretation of second order logic is a semantic one. However, one could transform the three syntactic interpretations of first order logic given above in semantic interpretations precisely in a fashion similar to that of Henkin's construction of the canonical model in his proof of the semantic completeness of first order logic, in which the semantic interpretation is essentially a mirror image of the new theory (endowed with all the desired properties of syntactic completeness, etc.) obtained on the basis of the original theory.

tions. Hence, after all, in the important sense under discussion, second order logic is not essentially more indeterminate than first order logic.

APPENDIX II

In this paper we have argued only indirectly against Field's philosophy of mathematics, namely, by showing that it is based on theses developed by Quine, Putnam and Benacerraf that, although widely accepted, are really empiricist dogmas of post-Quinean Anglo-American analytic philosophy. On the other hand, in our paper 'Interderivability of Seemingly Unrelated Mathematical Statements and the Philosophy of Mathematics' we had argued that more traditional philosophies of mathematics, like constructivism, formalism, pre-Fieldian nominalism, empiricism and Fregean Platonism, have serious difficulties to assess the (meta-) mathematical fact that there exist mathematical statements, e.g., the Axiom of Choice and Tychonoff's Theorem[61], which seem to be completely unrelated with regard to their content, but nonetheless are interderivable. We spared Field's philosophy of mathematics of a similar critique. However, it should be sufficiently clear that for a conception of mathematics which denies the existence of mathematical entities and, thus, for which all existential mathematical statements are false and all universal ones vacuously true, the existence of seemingly unrelated mathematical statements must look somewhat puzzling. For, first of all, it is very doubtful that, e.g., the Axiom of Choice and all its many equivalents can be rendered in the same logical form, since some of them, e.g., the Trichotomy of Cardinals, are clearly universal statements (and, thus, vacuously true, according to Field), whereas others seem to admit an existential rendering (and, according to Field, would have to be false). Hence, in that case, if Field is right, we would have interderivable statements with different truth values. (But even if all those interderivable statements admitted the same formal rendering, e.g., as universal statements, the fact is that not all universal statements in the language of mathematics are interderivable with them. Thus, even under such an extremely improbable assumption as that all those statements have the

[61] Tychonoff's Theorem affirms that the product of a family of compact topological spaces is a compact topological space.

same logical form, we would not be able to give an adequate assessment of the interderivability of seemingly unrelated mathematical statements.)

A related but more simple and straightforward argument against Field's philosophy of mathematics is the following. Consider any theory T in a first order language L. As is well known, a theory T is model complete exactly when for any two models M and M* of T, if M is a substructure of M*, then M is an elementary substructure of M*[62] . A criterion for the model completeness of a theory T in L is that for any existential sentence ψ in L there exists a universal sentence ψ in L such that the interderivability of ϕ and ψ is a theorem of T. But, according to Field, ϕ is false, since it is existential, and ψ vacuously true, since it is universal. Hence, if T is consistent and Field's conception were correct, ϕ and ψ could not be interderivable in T and, thus, T could not be model complete. Thus, if Field were right, there could not be any model complete theory. However, there are model complete theories. Therefore, Field's conception is false. Hence, Field's philosophy of mathematics, as it stands, is not only totally unwarranted, as was argued in the main text, and intuitively false, but also demonstrably false.

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[62] Speaking somewhat loosely, we can say that a model M of a theory T in a language L is a substructure of another model M* of T if and only if the universe of M is a subset of the universe of M* and for any atomic formula of L (in n variables) and for any n-tuple of members of the universe of M, the n-tuple satisfies the atomic formula in M if and only if it satisfies the formula in M*. It is easy to prove that the same is valid for all quantifier-free formulas in L. M is an elementary substructure of M* if and only if the same is valid for all formulas of L (including those that contain quantifiers).

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