MATHEMATICAL THOUGHT AND ITS OBJECTS

In *Mathematical Thought and Its Objects*, Charles Parsons examines the notion of object, with the aim of navigating between nominalism, which denies that distinctively mathematical objects exist, and forms of Platonism that postulate a transcendent realm of such objects. He introduces the central mathematical notion of structure and defends a version of the structuralist view of mathematical objects, according to which their existence is relative to a structure and they have no more of a "nature" than that confers on them.

Parsons also analyzes the concept of intuition and presents a conception of it distantly inspired by that of Kant, which describes a basic kind of access to abstract objects and an element of a first conception of the infinite. An intuitive model witnesses the possibility of the structure of natural numbers. However, the full concept of number and knowledge of numbers involve more that is conceptual and rational. Parsons considers how one can talk about numbers, even though they are not objects of intuition. He explores the conceptual role of the principle of mathematical induction and the sense in which it determines the natural numbers uniquely.

Parsons ends with a discussion of reason and its role in mathematical knowledge, attempting to do justice to the complementary roles in mathematical knowledge of rational insight, intuition, and the integration of our theory as a whole.

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Mathematical Thought and Its Objects

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For Marjorie

Contents

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Charles Parsons
Frontmatter
More information

Preface

The present work is largely concerned with a limited number of themes in the philosophy of mathematics. The first is the notion of *object* as it is deployed in mathematics. I begin in Chapter 1 with a general discussion of the notion of object, not on the whole focused on mathematics. One of the motives of this discussion is to defuse too-high expectations of what the existence of objects of some mathematical type such as numbers would entail. We proceed to discuss issues surrounding the structuralist view of mathematical objects, which has had a lot of currency in the last forty years or so but has much earlier roots. The general idea of this view is that mathematical objects do not have a richer "nature" than is given by the basic relations of some structure in which they reside. The problem of giving a viable formulation developing this idea is not trivial and raises a lot of issues. That is the concern of Chapters 2 and 3. Chapter 2 is mainly devoted to pursuing a program that uses the structuralist idea to eliminate explicit reference to mathematical objects. Along the way, I discuss some questions about nominalism, about second-order logic, and about how structuralism understands the application of mathematics. Some difficulties of the eliminative program call for using modal notions, and their use in mathematics is a subject of Chapter 3. But in the end even the modal version of the eliminative program is rejected, and in §18 a version of structuralism is sketched that takes the language of mathematics much more at face value. Chapter 4 responds to an objection to the application to set theory of the version of structuralism I defend. Along the way, it considers other questions about the concept of set and the axioms of set theory.

The second main theme is a particular notion of mathematical intuition, which has its origin in the thought of Brouwer and Hilbert about the most basic elements of arithmetic but whose original inspiration comes from Kant. In Chapter 5, I lay out some basic distinctions concerning the notion of intuition, about which writers who use the notion (even to criticize it) are often unclear. But the main point of the chapter is to explain the particular conception of intuition that concerns me, develop some of its implications, and reply to some possible objections to it. Intuition so conceived offers part of the entry of mathematical thought into the infinite. The structure of natural numbers is shown to be witnessed by what can be called an intuitive model.

Chapters 6, 7, and 8 all concern the arithmetic of natural numbers, and it is in the first two of these that the work done by the conception of intuition of Chapter 5 is visible. Chapter 6 discusses the role of natural numbers as finite cardinals and ordinals and considers in a rather idealized way how language referring to natural numbers might originate. This genetic method is inspired by W. V. Quine's *Roots of Reference*. A conclusion of the chapter is that in the sense of Chapter 5, there is not intuition of numbers properly speaking. We also explore theories of finite sets and discuss the question of intuition of such sets, with the conclusion that the analogy with perception that such intuition requires would be too much stretched if it is claimed that the theory of hereditarily finite sets rests on intuition in the sense in which Hilbert and Bernays claimed that finitary arithmetic does.

The latter thesis is the main subject of Chapter 7, which assumes an interpretation of the language of arithmetic as referring to formal expressions and inquires how much arithmetic is intuitively known. Primitive recursion appears as an obstacle, and we are not able to conclude that exponentiation or faster-growing functions can be intuitively seen to be everywhere defined. The Hilbert school maintained that finitist arithmetic included primitive recursive arithmetic. Our conclusion is that it is quite doubtful that intuitively evident arithmetic extends that far.

Chapter 8 deals with some issues concerning the principle of mathematical induction, that any predicate that is true of 0 and is true of $n+1$ if it is true of n is true of all natural numbers. As Poincaré pointed out a hundred years ago, this is the principle that makes arithmetic serious mathematics. I emphasize the open-endedness of "any predicate" in the principle. It is this that makes it possible to recognize the nonstandardness of a nonstandard model of formalized arithmetic and underlies Dedekind's proof that elementary axioms plus induction characterize the natural numbers up to isomorphism. The rest of the chapter discusses the uniqueness of the number structure and issues about impredicativity.

Chapter 9 turns to epistemological issues. After observing that mathematics has been characterized as rational knowledge, it introduces some issues about Reason and rational justification. Then it considers what can be said about the justification of principles in arithmetic and set theory.

A theme that appears in various places in the book,[1] but especially in Chapter 8, concerns schematic or second-order principles in mathematics, of which the most prominent examples are mathematical induction and the schemata of separation and replacement in set theory. As have other writers, I emphasize what is called the open-endedness of these principles, in that outside the context of specific formal systems they are not intended to be limited in their scope to particular formalized languages. But unlike some writers I do not see in this feature a convincing reason for regarding formulations in terms of second-order logic as canonical. Reasoning with second-order logic only moves the schematic character of principles into the logic. Furthermore, full second-order logic introduces a new assumption, that the instances of the relevant schemata are closed under second-order quantification, whatever one takes second-order variables to range over.

Certain issues that have been rather prominent in philosophy of mathematics in the last generation are commented on in the present work at most in passing. In one case, the question whether mathematical knowledge is a priori, the reader may find the omission surprising. The traditional affirmative view was vigorously attacked by W. V. Quine in some of his central writings. In more recent years it has been defended more than it has been attacked. I don't have a clear position to offer on this question. I am not convinced that the notion of a priori knowledge is as clear as is often assumed. It is quite obvious that experience and perception do not play the direct role in the justification of mathematical propositions that they play in natural science or in most factual statements of everyday life. The mathematician in proving a theorem does not appeal to experimental results or other forms of observation. Rigorous proofs can generally be represented as deductions from axioms. The question of the justification of axioms is a complex one, about which something is said in this work. Again there is no straightforward appeal to experiment and observation, but it is less easy to show that experience does not have some more subtle role that goes beyond the heuristic and motivating. In particular, that makes it harder to rule out the possibility that some unforeseen turn in

[1] And in places in *Mathematics in Philosophy*, for example, Essay 3.

the development of science might lead to the rejection as false of some assumption used in current mathematics.

But there is a consideration that makes this seem very unlikely.[2] That is that a mathematical theory of an aspect of the empirical world consists of taking a supposed actual system of objects and relations as an instance of a mathematical structure (where not every item in the structure is necessarily regarded as physically real). Then what confronts the "tribunal of experience" is the identification of a structure of this type with something in the world. That identification is falsifiable and has on many occasions been falsified. But then the resulting modification would consist of replacing the mathematical structure appealed to by another one, without abandoning the pure theory of the structure as mathematics. Thus, Euclidean geometry still plays a basic role in mathematics even though the view that space is Euclidean was questioned more than a hundred years ago and abandoned in the early twentieth century. It seems that some conceptual revolution of which we don't now have an idea would be required for us to abandon Euclidean geometry as mathematics. So it may be that much of current mathematics is "contextually a priori" in a sense proposed some years ago by Hilary Putnam.[3]

Another issue only glancingly commented on in this work is Benacerraf's dilemma. If it is put in terms of a causal theory of knowledge, according to which knowledge of certain objects requires causal relations of those objects and our minds, then I think the problem can be dismissed: mathematical objects are simply a counterexample to that theory of knowledge. But a more general form of the dilemma, expressed by W. D. Hart soon after Benacerraf's classic paper, is the difficulty of giving a naturalistic epistemology for mathematics.[4] That cannot be dismissed so easily. The more descriptive approach to mathematical

[2] I summarize here a point made in *Mathematics in Philosophy*, pp. 195–197.

[3] It would be hard to maintain this about the part of mathematics where there is uncertainty that might be serious, namely, the further reaches of set theory where very large cardinal axioms or other principles of high consistency strength are assumed. In this case, however, the mathematics makes no contact with actual natural science. If some of it is upset, it is far more likely that this will be a result of its internal development.

[4] See Hart, Review of Steiner, pp. 124–127. Hart's later paper "Benacerraf's Dilemma" corrects the exclusively ontological focus that many have given to the problem; in particular he points out that modal knowledge would raise similar questions. Insofar as Benacerraf's dilemma motivates nominalist constructions, that raises a question about the widespread use of modality in these constructions. Hartry Field may be influenced by a problem of this kind in seeking to limit his own use of modality to "strictly logical" modality. See "Is Mathematical Knowledge Just Logical Knowledge?" and "Realism, Mathematics, and Modality."

knowledge adopted in this work would probably not pass muster as naturalistic in the eyes of many contemporary philosophers. I don't see this as a serious problem for the foundations of mathematics. Naturalism as a philosophical tendency relies heavily on the authority of natural science. But modern science would be inconceivable without the application of mathematics. The actual methodology of mathematics, about which a descriptive approach aims to say something, has been found adequate (at least with the corrections arising in its own development, certainly influenced by applications) for the development of science over a more than two-thousand-year period. The absence of a naturalistic epistemology may mean that a kind of explanation or understanding of mathematical knowledge that would be desirable has not been attained. The search for it, like any enterprise in naturalistic epistemology, is on the boundary of philosophy and psychology. But even if it faces fundamental difficulties, they would not offer a convincing reason for abandoning current mathematics, or for reformulating it along some nominalistic or other lines, or for denying the claim of mathematical results to be true. To what extent we are still left with a challenge may depend on what counts as naturalistic, a matter that I leave to those who espouse naturalism to determine.[5] Furthermore, there are at least some reasons for doubting that what the naturalist seeks can be attained for our rational capacities in general, apart from the more special problems posed by mathematics.

Another omission is of any sustained discussion of the issues raised by constructivism in general and intuitionism in particular. Such a discussion would have been natural given the role played in this work by a conception of intuition that owes something to Brouwer. In fact, my original plan for the book called for a chapter on constructivism. The main reason why it is not there is that I have had my hands quite full with the other subjects I have taken on. It would be a large task to assess what the status of constructive mathematics and logic ought to be in the present day, or even to say accurately what it is. One thing, however, is clear: The use of classical logic in mathematics has survived Brouwer's attack on it, and mathematics obeying restrictions to ensure constructivity is a minority pursuit. A philosophical work that deals almost entirely with classical mathematics does not have to apologize for itself.

This work has been in the making for an unconscionably long time. During that time I have become indebted to a large number of

[5] For an argument that it is a challenge, and a proposal to answer it, see Linnebo, "Epistemological Challenges."

individuals, especially for intellectual stimulation and instruction, and to institutions for support. Columbia until 1989, and Harvard since then, have provided an academic home and an excellent work environment. Friends and colleagues in logic and philosophy of mathematics have been sources of stimulation, instruction, and questioning over many years. The late George Boolos, Solomon Feferman, Warren Goldfarb, Allen Hazen, Richard Heck, Geoffrey Hellman, Daniel Isaacson, Yiannis Moschovakis, Hilary Putnam, Michael Resnik, Stewart Shapiro, Wilfried Sieg, and William Tait deserve special mention, as well as Mark van Atten, Peter Koellner, and Agustín Rayo from more recent years. I still owe much to my longtime Columbia colleagues Isaac Levi and the late Sidney Morgenbesser. A consequence of the move to Harvard was more interaction with moral philosophers, which stimulated my interest in rational justification, dovetailing with an effort to understand ideas of Kurt Gödel. Without that, Chapter 9 of this work might not have been written at all. The late John Rawls provided the initial stimulus and helped me to understand his own views, and T. M. Scanlon has also been especially helpful. My fellow editors of Gödel's works, John W. Dawson, Jr., Feferman, Goldfarb, Sieg, Robert M. Solovay, and our Managing Editor Cheryl Dawson all contributed to my understanding of Gödel and his contemporaries.

None of my three principal teachers, Burton Dreben, W. V. Quine, and Hao Wang, lived to see the completion of this work. Some of the ideas were discussed with one or another of them, and their influence is no doubt more or less visibly present. I doubt that any of them would approve of what has finally come of the project.

Like many of us I have learned from my Ph.D. students. Most relevant to this work are R. Gregory Taylor, Richard Tieszen, Gila Sher, and Ofra Rechter at Columbia and Emily Carson, Michael Glanzberg, Øystein Linnebo, Michael Rescorla, and Douglas Marshall at Harvard. Tieszen especially has followed my writing over a long period and commented on earlier versions of many parts of this work.

In the long time that this work has been in progress, I have lectured on parts of it (sometimes as papers that have since been published) to many audiences. I am undoubtedly indebted to more members of these audiences than are mentioned here in connection with specific points. A special debt is owed to logicians and philosophers at the University of Padua, who twice invited me for extended series of lectures, which were accompanied by warm hospitality.

In the fall semester of 2006, the manuscript was discussed in a seminar at Harvard with both student and faculty participants. Questions and objections have led to many changes, nearly all of which I hope are improvements. In particular, Koellner and Vann McGee saved me from mathematical errors. Doubtless other errors remain, for which I am responsible.

Mihai Ganea conscientiously examined an earlier version of Chapters 1–8 for bugs of various kinds, and Jon Litland has done the same for the final version of the whole work. Litland also has prepared the index and assisted with proofreading.

This work has had more institutional support than any single book deserves. Both Columbia and Harvard have provided sabbatical leaves. The project was begun when I was an NEH Fellow and a Visiting Fellow of All Souls College, Oxford, and work on it was done when I was a Guggenheim Fellow, a Fellow of the Netherlands Institute for Advanced Study, and later a Fellow of the Center for Advanced Study in the Behavioral Sciences, the last with the support of the Andrew W. Mellon Foundation. I am very grateful to all these institutions. (The leaves they financed also encouraged other projects, especially the editing of Gödel's posthumous works.)

Terence Moore of Cambridge University Press welcomed the project and offered a contract on the basis of a very incomplete text. I regret that the work was not ready to submit before his untimely death. Two referees, one unmasked as Arnold Koslow, offered helpful suggestions. I am grateful to the Press for its continuing interest, in particular to the present editor Beatrice Rehl. I thank the production editor Laura Lawrie for her work and attentiveness to my concerns.

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I owe more to my wife, Marjorie Parsons, than I will venture to say here. I will, however, thank her for two specific things: for her unfailing support and assistance during two episodes of illness and for her patience in waiting so many years for a book to be dedicated to her.

Cambridge, February 2007

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Chapter 6 draws on "Intuition and Number," in Alexander George (ed.), *Mathematics and Mind* (Oxford University Press, 1994), pp. 141–157.

Chapter 7 overlaps considerably with "Finitism and Intuitive Knowledge," in Matthias Schirn (ed.), *The Philosophy of Mathematics Today*

(Oxford: Clarendon Press, 1998), pp. 249–270, and also draws on "On Some Difficulties."

In Chapter 8 §47 and §§50–51 draw on "The Impredicativity of Induction," in Leigh S. Cauman, Isaac Levi, Charles Parsons, and Robert Schwartz (eds.), *How Many Questions: Essays in Honor of Sidney Morgenbesser* (Indianapolis: Hackett, 1983), pp. 132–153, revised and expanded in Michael Detlefsen (ed.), *Proof, Logic, and Formalization* (Routledge, 1992), pp. 139–161. Material from the original paper is used by permission of the editors, who hold copyright. Material from the revision is included by permission of the editor and publisher.

§§48–49 rework the argument of "The Uniqueness of the Natural Numbers," *Iyyun: A Jerusalem Philosophical Quarterly* 39 (1990), 13–44. The later version appears in "Communication and the Uniqueness of the Natural Numbers," in the informally distributed *Proceedings of the First Seminar on Philosophy of Mathematics in Iran* (Shahid Beheshti University, Tehran, 2003).

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