Putnam on trans-theoretical terms and contextual apriority

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Forthcoming in James Conant and Sanjit Chakraborty, eds.,
Engaging Putnam (Harvard University Press)
April 25, 2018

Putnam concludes his classic paper, "It Ain't Necessarily So," with the following suggestive yet enigmatic remark:

> The difference between statements that can be overthrown by merely conceiving of suitable experiments and statements that can be overthrown only by conceiving of whole new theoretical structures—sometimes structures, like Relativity and Quantum Mechanics, that change our whole way of reasoning about nature—is of logical and methodological significance, and not just of psychological interest. (Putnam 1962a, p. 249)

This remark launches Putnam's career-long effort to investigate and clarify the methodological roles of statements that are so central to an inquirer's current theory of the topics they concern that she cannot specify any way in which the statements may actually be false. My main goals here are to explain how the problem of clarifying the methodological roles of such statements arises in Putnam's work (§I); to explain in synchronic, practical terms why it is reasonable for an inquirer to accept such statements (§§II–VI); and to contrast this explanation with Putnam's diachronic, theoretical explanation, according to which such statements can be revised "only by conceiving of whole new theoretical structures" (§VII).

I. How the problem arises for Putnam

Like his mentors Hans Reichenbach, Rudolf Carnap, and W.V. Quine, Putnam takes for granted that our everyday and scientific judgments are for the most part reasonable and not in need of philosophical justification. The task of philosophy for Putnam is not to justify our everyday and scientific judgments but to describe and clarify them.

When Putnam began his career in the early 1950s, the dominant model for clarifying our everyday and scientific judgments was that of the logical empiricists, especially Carnap. Carnap proposed that we reject traditional philosophical questions about scientific methods, and thereby clarify the sense in which scientific judgments are reasonable, by reconstructing the methods of science in terms of explicitly formulated rules for using sentences. Given such a set of rules, he observed, some sentences, which he called analytic, are settled solely by the rules without any need for empirical observation; others sentences, which he called synthetic, are only evaluable on the basis of empirical observations. For Carnap the key methodological significance of laying down explicitly formulated rules for inquiry is that if a sentence is a logical consequence of a given system of rules, then (first) anyone who has chosen to use the system, understands its rules, and has derived the sentence from the rules is thereby committed to accepting the sentence; and (second) there is no legitimate ‘higher’ or ‘firmer’ criterion for judging whether the sentence is true (Carnap 1934, p. 46).

In “Two Dogmas of Empiricism” (Quine 1953a), Quine argues that Carnap’s method of clarifying inquiry is unscientific. He agrees with Carnap and the other scientific philosophers of the day that our pre-theoretical grasp of the supposed analytic-synthetic distinction is too vague to be of use in a properly scientific philosophy. Quine’s central criticism is that the logical and mathematical methods that Carnap and others use to try to clarify the supposed distinction between analytic and synthetic sentences do not, in fact, clarify it, but simply presuppose that it is already clear. Quine concludes that philosophers who take science seriously should not rely on Carnap’s analytic-synthetic distinction to clarify our everyday and scientific judgments. In the last section of “Two Dogmas,” Quine sketches a new project of clarifying these judgments without relying on the logical empiricists’ analytic-synthetic distinction.

Putnam was one of the earliest and most important converts to this Quinean project (Putnam 2015a, pp. 16–17). In his first major contribution to it, “The Analytic and the Synthetic” (Putnam 1962b, first drafted in 1957–58), Putnam observes that there are some statements in natural language, such as “Bachelors are unmarried,” for which (first) there is only one criterion, such as being an unmarried adult male, for applying the subject term, such as “Bachelor”, to someone; and (second) by this criterion, the statement is true. This observation superficially conflicts with Quine’s claim in §2 of “Two Dogmas” that the only clear synonymy relations are those established by explicit acts of definitional abbreviation. As Putnam knows, however, this is not a deep challenge to Quine’s arguments in “Two Dogmas”, for two main reasons. First, the relationship between a word we introduce by an explicit act of definitional abbreviation, and the expression we introduce the word to abbreviate, is similar to the relationship between our uses of a one-criterion word of an unregimented natural language and the longer phrase of that language that states the generally accepted criterion for applying the word. It is therefore not a big step for Quine to acknowledge the existence in natural language of one-criterion words, such as “Bachelor,” and the corresponding sentences, such as “Bachelors are unmarried,” to which everyone assents. Quine takes this step, acknowledging Putnam, in chapter two of *Word and Object* (Quine 1960, pp. 56–57).

Second, like Quine, Putnam saw that for any two words at least one of which is not a one-criterion word, the question whether the words are synonymous is at best unclear. In “The Analytic and the Synthetic,” Putnam develops and extends this part of Quine’s criticism of the logical empiricists’ analytic-synthetic distinction by highlighting a range of examples of theoretical statements that are not fruitfully classified as either analytic or synthetic. For instance, before the development of relativity theory, Putnam explains, physicists were unable to see any way in which ‘$e = ½ mv^2$’, an equation for kinetic energy, could be false. They held it immune from disconfirmation by new empirical evidence, and it was reasonable for them to do so. By Carnap’s logical empiricist principles, Putnam notes, the methodological role of the equation is best explained by describing it as true by definition of kinetic energy. After Einstein developed relativity theory, however, scientists revised ‘$e = ½ mv^2$’, replacing it with a more complicated equation that fits the new theory, and concluded that 'e = ½ mv^2', while approximately true, was strictly speaking false, hence not true by definition. To make sense of such cases, Putnam introduces the idea of a "law-cluster" term, which figures in many different laws of a theory. He observes that we can give up one of the laws in which such a term figures without concluding that the reference of the term has changed. For instance, we can continue to use a given term to refer to kinetic energy while radically changing our theory of kinetic energy. Putnam calls such terms *trans-theoretical*.

He deepens and extends these criticisms of logical empiricism in "It Ain't Necessarily So" (Putnam 1962a), where he observes that our theories of the geometry of physical space have changed since the eighteenth century, when the principles of Euclidean geometry were so central to our way of thinking about physical space that we could not then specify any way in which those principles may actually be false. He argues that while our theory of physical space has changed radically since the eighteenth century, it is nevertheless correct to regard the terms that scientists in the eighteenth century used to refer to paths through physical space as trans-theoretical and to conclude that many of the sentences about physical space that scientists accepted in the eighteenth century, such as "The sum of the interior angles of any triangle formed by joining three points in physical space by the shortest paths between them is 180°," are false. Putnam concludes that some statements are so central to our way of thinking at a given time that it would not be reasonable to give them up at that time, even if our failure to be able to specify any way in which they may actually be false is no guarantee that they are true.

Even logical and mathematical statements that we now regard as obvious and beyond any doubt should not be described as analytic, Putnam argues, since there is no methodological guarantee that we will not later judge the statements false without changing the references of the terms we use to express the statements. He therefore rejects Carnap's proposal that we clarify the sense in which scientific judgments, including scientists' acceptance of logical and mathematical truths, are reasonable and not in need of any philosophical justification, by adopting systems of rules that imply logical and mathematical truths. The problem for Carnap's proposal, according to Putnam, is that, in contrast with sentences that contain only one-criterion terms, the sentences of logic, mathematics, and much of natural science, including physical geometry, contain trans-theoretical (i.e. law-cluster) terms, and therefore have "systematic import" (Putnam 1962b, p. 39). Hence even if we are currently unable to specify any way in which a statement we express by using a sentence with systematic import may actually be false, there is no methodological guarantee that we (or future inquirers) will not revise the statement, translate the terms we use to express it homophonically into our (or their) new theory, and thereby judge it to be false.

Putnam concludes that "there is no sensible distinction between *a priori* and *a posteriori* truths" (Putnam 1983a, p. 88). The statements that traditional philosophers label *a priori* — statements of logic and mathematics, as well as, for instance, the eighteenth century scientists' statement "The sum of the interior angles of any triangle formed by joining three points in physical space by the shortest paths between them is 180°"—should be regarded, instead, as *contextually a priori* (Putnam 1983a, p. 95). To a first approximation, a statement is *contextually a priori* for an inquirer at a time if and only if she accepts it, it has systematic import for her overall theory, and she cannot specify any way in which it may actually be false. Although we treat such statements as immune to empirical disconfirmation, they are not immune to revision without a change in subject, for the reasons explained above, and are therefore not analytic. Putnam nevertheless remains firmly committed to his guiding methodological principle that our everyday and scientific judgments, including our acceptance of contextually a priori statements, are for the most part reasonable and not in need of philosophical justification. Instead of abandoning this principle, he seeks an alternative explanation of why it is reasonable for inquirers to accept contextually a priori statements (Putnam 1983a, p. 95). He therefore takes his arguments in "The Analytic and the Synthetic" and "It Ain't Necessarily So" to pose an internal problem for his account of the methodology of inquiry—the problem of explaining why it is reasonable for scientists to accept statements that are contextually a priori for them even though there is no methodological guarantee that the statements are true.

II. A reconstruction of Putnam's problem

It will help to have a sharper formulation of Putnam's problem. I see him as starting with the following

> *Methodological Principle* (MP): In our pursuit of truth, we can do no better than to start in the middle, relying on already accepted beliefs and inferences, and applying our best methods for reevaluating particular statements, beliefs and inferences and arriving at new ones.

I take (MP) to imply, as both Quine and Putnam argue, that no belief, inference, or method is immune to revision, and that there are no epistemological standards higher or firmer than the ones we express in our actual endorsements of particular statements, beliefs, inferences, and methods for arriving at beliefs. We may find fault with our grounds for accepting any of our statements, beliefs, inferences, or methods, and revise them accordingly, but only if such findings or revisions are rooted in our latest, best sense of which ones to accept.

Let us consider, to begin with, an unproblematic application of (MP). Suppose, for example, that we wish to know whether or not there is an occurrence of the word 'octopus' on a given page of text that we have not seen before. To make things as simple as possible, assume that prior to looking carefully at the page, we have no reason to believe there is an occurrence of the word 'octopus' on it and no reason to believe there isn't. In such circumstances, to determine whether or not to believe that there is an occurrence of 'octopus' on the page, we rely on a vast background of beliefs about such things as what pages are, what occurrences are, what counts as an occurrence of 'octopus' on a page, and so on, and we focus on the question whether or not there is an occurrence of 'octopus' on the page. To answer this question, of course, we examine the page. If we find an occurrence of 'octopus' on it, we will for that reason come to accept the statement that there is an occurrence of 'octopus' on the page.

In this and many other cases, both in everyday life and in science, the statements S into which we inquire satisfy two conditions: (a) before our inquiry into whether or not S, it is both epistemically possible for us that S and epistemically possible for us that not S, and (b) we can see how we may come to have a reason for accepting S or for accepting not S. In the above case, for instance, condition (a) is by hypothesis satisfied, and so is (b), since I suppose that I can tell by looking whether or not there is an occurrence of 'octopus' on the page. In other similar perceptual cases, I might satisfy (b) if I realize that I can tell by listening, touching, smelling, or tasting, whether or not S, for some statement S.

There are of course many statements that satisfy (a) but that we cannot evaluate by perceiving alone. In practice, for any such statement S, we rely on non-perceptual ways of answering the question whether or not S, such as deducing S from statements we already accept, inferring that S by induction, from initial conditions and a well-confirmed, high-level explanatory law, or, more generally, by reasoning to the best explanation. I shall assume that our resources are varied and comprehensive enough that if (a) holds, so, typically, does (b).

To make sense of such cases I shall rely on a partly regimented sense of the English word "reason," i.e., a sense of "reason" that satisfies the following condition:

(R1) A person has a reason for believing that S only if she can say why she believes that S without presupposing that S.

Just as in the 'octopus' example, in general, when conditions (a) and (b) hold for a given person *x* and statement S, *x* may come to have a *reason* for accepting that S, in the sense of "reason" regimented by (R1).

We have no difficulty explaining how it can be epistemically reasonable for us to accept a statement S for which conditions (a) and (b) hold, since we understand how we may come to have a reason for accepting S, in the sense of "reason" regimented by (R1). By contrast, it is not so easy to see explain how it can be epistemically reasonable to accept a contextually a priori statement. For a statement that is contextually a priori for us is one we cannot coherently suppose to be false, where the "cannot" is practical, a matter of what cannot now do: when we try to specify a way in which a belief that S of this sort may actually be false, we find we are unable to do so. Hence condition (a) does not hold for S. Moreover, a statement that is contextually a priori for us has systematic import for our theory. For reasons I shall explain a few paragraphs below, this implies that if S is contextually a priori for us, then we cannot say why we believe that S without relying on S. We therefore cannot give a reason for accepting S, in the sense of "reason" regimented by (R1).

Putnam's problem is to explain how it may be epistemically reasonable to accept contextually a priori statements given that they are not analytic, so there is no methodological guarantee that they are true, and we cannot why we accept them without presupposing them.

To solve this problem we need a clearer characterization of the methodological roles of contextually a priori statements. Let us examine a particular example: the statement that no statement of the form 'S and not S' is true. Both my expression of this statement and my belief that no statement of the form 'S and not S' is true presuppose a background of beliefs about how to define truth, about the semantics of negation and conjunction, about what follows from what, and so on. These are not beliefs that I can now suspend or reject without losing my grip on what I say when I use my sentence, "No statement of the form 'S and not S' is true." Although I have tried to make sense of challenges to my belief that no statement of the form 'S and not S' is true, including challenges from Graham Priest and other dialethiests, I find I am (so far) unable to describe coherently any situation in which the belief is false.[1] Moreover, all my attempts to argue for the conclusion that no statement of the form 'S and not S' is true rely at some point or other on a premise or inference rule that is either stronger than the conclusion, or equivalent to it. Consider, for instance, the following argument:

> Start with two semantic premises: (1) ⌜Not S⌝ is true if and only if S is not true, and (2) ⌜S and S′⌝ is true if and only if S is true and S′ is true. Now suppose (toward contradiction) that for some S, ⌜S and not S⌝ is true. Then, by (2), S is true and ⌜Not S⌝ is true. Hence, by (1), S is true and S is not true. *This is a contradiction, hence not true*, and so it is not the case that for some S, ⌜S and not S⌝ is true.

This argument begs the question by assuming that there can be no true contradictions (Priest 1998, p. 418). More generally, my failure to find a non-circular argument for the conclusion that no statement of the form 'S and not S' is true convinces me that I have no reason for accepting this, in the sense of "reason" regimented by (R1). The statement that no statement of the form 'S and not S' is true is part of my best current theory and it has systematic import for my overall theory. I accordingly take my acceptance of the statement to be epistemically reasonable. By (MP), moreover, I can do no better than this—there is no higher or firmer perspective from which to judge whether the acceptance (or belief) is epistemically reasonable.

We need a word to describe the epistemological standing of such beliefs. I propose that we use the English word "entitled" (or "entitlement"), regimented as follows:

(R2) A person is entitled (or has an entitlement) to accept S if and only if she has no reason (in the sense of 'reason' regimented by (R1)) for accepting S—she cannot

[1] Why am I not convinced by the dialethiest's arguments? This is not the place for a full explanation, but here is a characteristic detail. Priest takes the existence of certain mathematical structures—e.g. an assignment of "t-r-u-e" and "f-a-l-s-e" to the same quadrant of a Euclidean plane—to show that a statement may be both true and false (Priest 1998, p. 414). I do not see, however, how the fact that one can assign "t-r-u-e" and "f-a-l-s-e" to the same quadrant of a Euclidean plane specifies a way in which a statement can be both true and false.

say why she accepts S without relying on S—but it is epistemically reasonable for her to accept S.

To make use of this regimentation, we must be guided by our own best judgments about whether a person's acceptance of the statement is epistemically reasonable. For instance, as noted above, if my acceptance of the statement that no sentence of the form 'S and not S' is true is epistemically reasonable, then given (R2), I am entitled to accept it. In this case, of course, I am unable to specify a way in which the statement is false. And that may seem to suggest that if a person cannot specify a way in which a statement S may be false, then by (R2) she is epistemically entitled to believe that S. But this latter claim is problematic, for three main reasons. First, a person might not understand a given statement S at all, and hence might not be in a position even to consider the statement, let alone to accept, or reject it. How much understanding is required is a subtle, context-sensitive matter, some level of understanding is required for her even to accept S, and hence also for her to have entitlement. Second, a person who understands a given statement S might not have tried to say why she accepts S without presupposing S. She may wrongly suppose she cannot say why she accepts S without presupposing S, simply due to lack of trying. In such a case, (R2) does not apply to S. Third, a person who understands a given statement might not have tried to specify a way in which it may be false. The point is that a special kind of *due diligence* is required—it is only once an inquirer has done the right kind of due diligence that her inability to specify a way in which a statement S may actually be false and her inability to say why she accepts S without presupposing S may amount to an entitlement to accept S of the kind regimented by (R2). I investigate and try to clarify the relevant kind of due diligence in §IV–VI below.

As noted above, a statement is contextually a priori for an inquirer only if it has systematic import for her theory. As I shall understand "systematic import" in what follows, a statement S has systematic import for an inquirer's theory if and only if (a) his explanations of a significant range of phenomena depend on S, (b) despite doing his due diligence he has no reason for accepting S in the sense of "reason" regimented by (R1)—i.e., he cannot say why he accepts S without presupposing S, and (c) he knows that (a) and (b) are true. In the direct, practical way in which inquirers know what they are doing when they explicitly formulate and affirm an explanation and when they search for a reason for accepting a statement yet fail to find one, an inquirer can and typically does know whether conditions (a) and (b) obtain for a statement that he accepts. Thus all three conditions are synchronic and practical. There is therefore no special difficulty in determining whether or not conditions (a)–(c) hold for a given person and a given statement that he accepts. My statement that no statement of the form 'S and not S' is true, for example, clearly satisfies all three of the conditions (a)–(c).

With these definitions in place, I propose that we explicate "statement S is contextually a priori for person x" as follows:

(R3) A statement S is contextually a priori for person x if and only if x understands a statement S well enough to raise the question of whether or not to accept S, x

accepts S, S has systematic import for *x*'s total theory, *x* tries to specify a way in which S may actually be false, *x* exercises due diligence in this effort, but *x* is nevertheless unable to specify a way in which S may actually be false.

With "statement S is contextually a priori for person *x*" explicated in this way, let us now try to evaluate the following conditional:

(R4) If statement S is contextually a priori for person *x*, then it is epistemically reasonable for *x* to accept S.

Consider the following outline of an argument for (R4). Suppose statement S is contextually a priori for a person. Then, by (R3), S has systematic import for her, so (by the explication of "systematic import" in the previous paragraph) she is unable to say why she accepts S without presupposing S, despite doing her due diligence, and hence she has no reason (in the sense of "reason" regimented by (R1)) for accepting S. By (R2), then, we may infer that she is entitled to accept S if and only if it is epistemically reasonable for her to accept S. Also by (R3), however, she is unable to specify a way in which S may actually be false, despite doing her due diligence. She is therefore unable to see any good reason to revise S, and so, by (MP), she can do no better than to accept S—i.e., it is epistemically reasonable for her to accept S. This reasoning supports (R4). Finally, (R1)–(R4) together imply that she entitled to accept S, in the sense of "entitled" regimented by (R2).

Putnam's problem, as I reconstruct it, is to explain in a more compelling and possibly more substantive way why (R4) is true, given (MP) and (R1)–(R3).

III. Rejecting a traditional epistemological assumption

Many epistemologists assume that one can be epistemically entitled to accept a statement only if there is something about the statement, or its place in one's overall theory, that makes it likely that the statement is true. (See, for example, Boghossian 2000 and 2001, Bonjour 1998, Katz 1998, Peacocke 2000, and Rey 1998.) I shall call this the *traditional epistemological assumption*. It implies that there is no solution to Putnam's problem, as I have presented it.

The reasoning is as follows. By (MP), there is no methodological guarantee that a contextually a priori statement is true. And none of the methodological characteristics of contextually a priori statements imply that there is something about the statement, or its place in one's overall theory, that makes it likely that the statement is true. Granting the traditional epistemological assumption, then, none of the methodological characteristics of contextually a priori statements imply that one is epistemically entitled to accept a contextually a priori statement. As we have seen, however, the conjunction of (R1)–(R3) with (MP) implies (R4): if a statement is contextually a priori for a given inquirer, then she is epistemically entitled to accept it. The traditional epistemological assumption is therefore logically incompatible with the conjunction of (R1)–(R3) and (MP).

If one refuses to give up the traditional assumption, one must reject the conjunction of (R1)–(R3) and (MP). But my goal in this paper is to investigate the consequences of accepting (R1)–(R3) and (MP), which I take to frame Putnam's problem. I will therefore set aside the traditional epistemological assumption. I shall assume, provisionally at least, for the reasons explained above, (R1)–(R3) and (MP) imply (R4). The problem—Putnam's problem, as I understand it—is to explain in a more detailed and compelling way why this is so.

IV. Tests, riddles, and due diligence

The key to solving Putnam's problem, as I shall now try to show, is to describe the methodological roles of contextually a priori statements from an engaged, practical point of view. My starting point is the observation that whether or not a statement is contextually a priori for a given person at a given time depends on what she can do at that time.[2] As expressed in (R3), the point is that a statement S is contextually a priori for a person if and only if she understands a statement S well enough to raise the question of whether or not S is to be believed, she accepts S, S has systematic import in her total theory, she tries to specify a way in which S may actually be false, exercises due diligence in this effort, but she is unable to specify a way in which S may actually be false. In this context, I propose that we clarify the open sentence "person *x* is unable to specify a way in which S may actually be false" in terms of an oral or written test with some (perhaps vague) time limit, as follows: at the start of the test *x* is prompted to utter or write down a way in which S may actually be false, and *x* tries to do so. If, by the end of the allotted time, *x* has failed to utter or write down any such way, the open sentence is true; otherwise it is false.

To highlight the importance of such practical failures to the methodology of inquiry, Putnam once compared them to our failure to see the solution to a riddle, such as the following one, which he takes from Wittgenstein: A person arrived at a ball neither naked nor dressed. What was he wearing? Solution: a fishnet. Putnam wrote:

> Concerning such riddles, Wittgenstein says that we are able to give the words a sense only after we know the solution; the solution bestows a sense on the riddle-question. This seems right. … If someone asked me, "In what sense, exactly, was [the person] neither naked nor dressed?" I could not answer if I did not know the solution. (Putnam 1994, p. 254)

Putnam's point here is partly obscured by the fact there are several different possible solutions to the riddle—i.e., several different ways in which a person might qualify as neither naked nor dressed. By contrast, consider the following riddle described by Reichenbach:

[2] I am guided here by Wittgenstein's remark that "When I say 'I don't know my way about in the calculus' I do not mean a mental state, but an inability to *do* something" (*Remarks on the Foundations of Mathematics* III, §81; quoted in Floyd 2000, p. 249).

> Three matches are laid on the table in the shape of a triangle; the problem is to form four triangles by adding three more matches. (Reichenbach 1958, p. 41)

It is natural to try first to form the required four triangles by placing the three additional matches on the table. But there is no way to form the required four triangles in this way. If one does not revise one's understanding of the problem, one will fail to find a solution to it. Reichenbach writes:

> Rarely somebody conceives the idea of arranging the three matches spatially on top of the triangle lying on the table so that a tetrahedron results. (Reichenbach 1958, p. 41)

Like Putnam's riddle, Reichenbach's riddle entertains us by making us aware of "conditions we impose upon our imagination" (Reichenbach 1958, p. 41). The riddles are fun and instructive because they do not stump us for long. We either solve them by ourselves or are told how to solve them, and thereby become aware of conditions we imposed upon our imagination before we knew how to solve them. By contrast, if a statement is a contextually a priori for us in the sense defined by (R3), we are unable to specify any way in which it may actually be false even after doing our due diligence.

But what is due diligence? We may assume, to begin with, that it includes searching on our own for a way in which the statement may be false, and asking others if they know of a way in which it may be false. Just as in other areas of inquiry, neither our own or other's verdicts on such questions are final. As I noted above, for example, I am unconvinced by Graham Priest's arguments for his conclusion that the statement that no statement of the form 'S and not S' is true is false. Despite his efforts, I am unable to specify a way in which the statement that no statement of the form 'S and not S' may actually be false. A proper search for ways in which a given statement may actually be false—a search that demonstrates due diligence—must weigh a host of factors and is always provisional, subject to revision, yet no less central to our methods of inquiry for that. To clarify the relevant notion of due diligence, in the next two sections I briefly review some of the methods of inquiry in which the statements that are contextually a priori for us are embedded.

V. Due diligence in logic and mathematics

Consider first the role of due diligence in our evaluations of logicians' and mathematicians' efforts to solve three different types of problems: exercises in logic and mathematics texts; open problems in mathematics for which we conjecture that there are proofs that have not yet been discovered; and problems in mathematics that are now known to be unsolvable, but that mathematicians previously did not have the methods to prove unsolvable.

Exercises in logic and mathematics texts. Some of the exercises in logic and mathematics texts are very difficult to solve. Most of them, however, have already been solved—the solutions are known to the textbook writer, at least, and perhaps many others. A person who tries to solve a logic or mathematics exercise, sees no way to do so, yet also cannot produce a counter-example to it, should not conclude that the problem cannot be solved. In such a context, the person's failure to see any way to prove the statement has no special methodological significance. He has good reason to believe that others can prove the statement or provide a counter-example to it, and that his failure to see how to proceed is mainly of personal, psychological interest. If he announces that a statement cannot be proved simply because he has not been able to see how it to prove it, he has not done his due diligence, and his announcement is of no methodological significance.

Open problems in mathematics. A mathematician who can produce no counter-example to a plausible conjecture about a well-understood topic, yet is stumped about how to prove it, does not and should not conclude that there is no proof of it. Due diligence requires more of him than that. He cannot even be sure that if there is a proof of the conjecture, it makes use solely of proof methods that are traditionally associated with the topic of the conjecture. Logic, mathematics, and the sciences more generally sometimes advance by radical transformations of the methods for establishing statements. One spectacular recent example is Andrew Wiles's proof of Fermat's last theorem.

Problems in mathematics that are now known to be unsolvable, but that mathematicians previously did not have the methods to prove unsolvable. Here are three examples:

1. Trisecting the Angle. A classic problem from ancient Greek mathematics is to find a procedure for trisecting any given angle by means of compass a straightedge alone. Mathematicians searched for centuries without success for such a procedure, and gradually began suspecting that there isn't one. They therefore started to investigate the question: How is it possible to prove that certain problems cannot be solved? (Courant and Robbins, p. 118.) It was not until the 19th century that mathematicians discovered a method for finding, for any given angle, an algebraic equivalent to the question whether there is a procedure for trisecting the angle by using only a compass a straightedge. They discovered, for instance, that there exists such a procedure for trisecting a 60° angle if and only if the equation $8z^3 - 6z = 1$ has rational roots. They then proved algebraically that $8z^3 - 6z = 1$ has no rational roots, and concluded that one cannot trisect a 60° angle using a compass a straight edge alone (Courant and Robbins, pp. 137–138). Prior to the discovery of a method for finding algebraic equivalents for questions about geometrical constructions, mathematicians had no way to prove that there is no procedure for trisecting any given angle using only a compass and straightedge. The discovery of such algebraic equivalents transformed the methods of geometry: after the discovery it was no longer compatible with having done one's due diligence as a mathematician to search for a procedure for trisecting any given angle using only a compass and straightedge.

2. Hilbert's Tenth Problem. In 1900 David Hilbert listed 23 of the most important mathematical problems left open by work in 19th century mathematics. He stated the tenth of these problems as follows:

> Given a Diophantine equation with any number of unknown quantities and with rational integral numerical coefficients: *To devise a process according to which it*

can be determined by a finite number of operations whether the equation is solvable in rational integers.

(A Diophantine equation is a polynomial equation in one or more unknowns with integer coefficients, such as the equation noted above, $8z^3 - 6z = 1$, and $(y - xu)^2 = 0$.) As Yuri Matiyasevich, the person whose work finally solved this problem, explains, "Hilbert asks for a universal method for deciding the solvability of all Diophantine equations" (Matiyasevich 2016, p. 36). The proof that Hilbert's problem is unsolvable—i.e., that there is no decision procedure for determining whether or not any given Diophantine equation has solutions in rational integers—depends on a foundational result of the theory of computability: that there exist listable sets of natural numbers for which there is no algorithm for recognizing, given a natural number n, whether it belongs to the set or not (Matiyasevich 2016, p. 39). The strategy for relating this result to Hilbert's tenth problem is due to Martin Davis, who conjectured in the 1950s that *every listable set of numbers is Diophantine*, where a set S of numbers is Diophantine if and only if for some Diophantine equation $P(x_1,\ldots, x_n, y_1,\ldots, y_m) = 0$,

$(x_1,\ldots,x_n) \in S \leftrightarrow (\exists y_1,\ldots, y_m)[\ P(x_1,\ldots, x_n, y_1,\ldots, y_m) = 0]$ (Davis 1982, pp. 200–201).

Many mathematicians, including Alfred Tarski and George Kreisel, regarded Davis's conjecture as implausible. However, in a series of steps established by Davis, Hilary Putnam, Julia Robinson, and Matiyasevich, Davis's conjecture was finally proved in 1970. The truth of Davis's conjecture and the foundational result summarized above together immediately imply that Hilbert's tenth problem is unsolvable. In 1900, when Hilbert announced his tenth problem, it was compatible with having done one's due diligence as a mathematician to conjecture that tenth problem could be solved. Now, given the foundational result and the Davis-Putnam-Robinson-Matiyasevich proof of Davis's conjecture, it is no longer compatible with due diligence to conjecture that Hilbert's tenth problem can be solved.

3. Gödel's Incompleteness Theorems. In 1931 Gödel showed how to assign numbers to symbols and thereby to encode logical syntax, including all the deductive proofs that can be constructed in a given system, in elementary arithmetic. By exploiting this method, he proved his *first incompleteness theorem*: given any consistent proof system PS for a language L that is rich enough to express all the arithmetical truths, there exists a true arithmetical sentence S of L such that neither S nor the negation of S can be proved in PS (Kleene 1952, Theorem 29 (Rosser's form)). Gödel's *second incompleteness theorem*, closely related to the first, is that the consistency of a consistent proof system PS for a language L rich enough to express arithmetic cannot be proved in PS (Kleene 1952, Theorem 30). Gödel's theorems radically transformed the way logicians and mathematicians think about proof systems for arithmetic—it is no longer compatible with having done one's due diligence as a mathematician to assume that there are complete and consistent proof systems for arithmetic and to conjecture that in any such system PS, one can prove that PS is consistent.

VI. Contextual apriority explained

Each in its own way, the examples in §V show how new developments in logic and mathematics transform the methodology of these disciplines, so that what seemed like reasonable questions or assumptions prior to the developments (e.g. "How can one trisect any given angle using only compass and straightedge?", "By what universal method can one determine whether any given Diophantine equation has solutions in rational integers?", "There exist complete deductive systematizations of elementary arithmetic", and "The consistency of consistent proof system PS for a language L rich enough to express arithmetic cannot be proved in PS") are later seen to be without solutions or false for reasons we did not previously even conceive. I want now to suggest that the examples of §V, by extension, also help us to explain contextual apriority, by helping us to see how it may happen that at one time we may reasonably accept a given statement S, regard S as of systematic import for our current theory, and, despite doing our due diligence, be unable to specify any way in which S may actually be false, yet later come to realize, after viewing S in a new way that no one in our scientific community noticed or even formulated before, that S is false.

There are statements of this kind in logic and mathematics. Consider, for instance, Gottlob Frege's acceptance of his Basic Law V, according to which "we can convert the generality of an equality into a value-range equality and vice versa" (Frege 2013, Volume I, §9, p. 14). Frege emphasized that Basic Law V is indispensable to previous work in logic—"The entire calculating logic of Leibniz and Boole rests upon it." (Frege 2013, Volume I, §9, p. 14)—and that "one thinks in accordance with it if, e.g., one speaks of extensions of concepts" (Frege 2013, Volume I, Forward, p. VII). He recognized that it is more doubtful than his other basic laws, but he still took it to be purely logical, and at least as obvious as any proof one might propose for it. He nevertheless immediately understood the letter he received from Bertrand Russell in 1902 (Russell 1902)—the famous letter in which Russell derives a contradiction from Frege's Basic Law V. This does not imply that Frege and other logicians had been negligent, failing to do their due diligence. Frege's commitments to rigor and to establishing firm foundations in logic are unsurpassed. Moreover, commenting on Russell's discovery of the contradiction, Frege noted, "Every one who has made use of extensions of concepts, classes, sets in their proofs is in the same position [of having regarded law V as a logical law]" (Frege 2013, Volume II, Afterword, p. 253). For my purposes here, the key methodological point is that Russell's derivation of a contradiction from Basic Law V, though it now appears obvious and inevitable, went unnoticed by all previous logicians, despite their diligence in seeking solid foundations for logic. Russell's derivation therefore transformed how logicians and mathematicians think about the relationship between concepts and extensions of concepts. As a result of Russell's discovery, what was previously contextually a priori for Frege and other logicians immediately lost that status, and was shown to be false.[3]

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[3]There are some fine points that need to be sorted out for a final judgment about Frege's due diligence to be made. As van Heijenoort explains, in 1879 Burali-Forti observed that

> On naïve set-theoretic assumptions, the set of ordinals is well-ordered, hence has an ordinal; this ordinal is at once an element of the set of ordinals and greater than any ordinal in the set. (van Heijenoort 1967, p. 104)

As we saw above, there are also cases in which we accept a statement S, regard it as fundamental to our best current science, and are unable to specify any way in which it may actually be false, yet we later discover not that S is inconsistent, like Frege's Basic Law V, but that S, though consistent, is false. Recall that according to Putnam the terms that the scientists in the eighteenth century used to refer to paths through physical space are trans-theoretical. The statements that scientists in the eighteenth century made by using such terms—statements such as *The sum of the interior angles of any triangle formed by joining three points in physical space by the shortest paths between them is 180°*—were so central to their theory of physical space that even after due diligence, they could not specify any way in which they may actually be false. As Putnam emphasizes, their failure to be able to specify any way in which these statements may actually be false was no guarantee that they were true. Today, after a great deal of sophisticated new theorizing in logic, mathematics, and physics, we know that the eighteenth century scientists' statements about physical space are false.

These reflections about the role of due diligence in Frege's acceptance of Basic Law V and the eighteenth century scientists' acceptance of statements about physical space help us to solve Putnam's problem. To see why, recall first that if a statement S is contextually a priori for person, then by (R3) S has systematic import for her, so (by the definition of "systematic import" in §II) she is unable to say why she accepts S without presupposing S, despite doing her due diligence, and hence she has no reason (in the sense of "reason" regimented by (R1)) for accepting S. By (R2), then, we may infer that she is entitled to accept S if and only if it is epistemically reasonable for her to accept S. Also by (R3), however, she is unable to specify a way in which S may actually be false, despite doing her due diligence. She is therefore unable to see any good reason to revise S, and so, by (MP), she can do no better than to accept S—i.e., it is epistemically reasonable for her to accept S. This reasoning supports (R4)—i.e. if statement S is contextually a priori for person *x*, then it is epistemically reasonable for *x* to accept S. Finally, (R1)–(R4) together imply that she epistemically entitled to accept S. The two cases described in this section both satisfy all of these conditions. We may therefore infer that Frege's Basic Law V and the eighteenth century scientists' statements about physical space were contextually a priori for Frege and the eighteenth century scientists, respectively, and that by (MP) and (R1)–(R4), both Frege and the eighteenth century scientists were entitled to accept those respective statements. Together with the

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This paradox was much discussed at the time but there was little agreement about what it showed. Burali-Forti's paper also contained errors. In a letter to Dedekind in 1899 (Cantor 1899), Cantor corrected the errors, and clarified Burali-Forti's observation in Cantor's own terms, concluding that we must distinguish between consistent and inconsistent "multiplicities." Cantor called the former, but not the latter, sets. In retrospect, it may appear that Cantor was already drawing something like our contemporary distinction between sets and classes. But Cantor 1999 neither formulates it sharply nor explores its relationship to logical laws about extensions of concepts. W. V. Quine, citing Fraenkel's claim that Cantor sensed paradox in his conception of sets, calls the appearance that Cantor distinguished between sets and classes "myopic" and "hindsightful" (Quine 1974, p. 102). In any case, however, since Cantor distinguishes between consistent and inconsistent "multiplicities" in a personal letter to Dedekind in 1899, it is unlikely Frege would have known of Cantor's reasoning about such "multiplicities" before 1902, when Frege received Russell's letter.

observations in this and previous two sections about how the notion of due diligence in (R3) is to be understood, I submit, the examples in this section make clear why, given (MP) and the regimentations and explications of §II, it is epistemically reasonable to accept a contextually a priori statement.

VII. Putnam's conceptual schemes explanation

Putnam offers a different explanation of contextual apriority. His proposed explanation depends on his idea, first articulated in "It Ain't Necessarily So," that a statement may be necessary relative to a "body of knowledge" or "conceptual scheme":

> when we say that a statement is necessary relative to a body of knowledge, we imply that it is included in that body of knowledge and that it enjoys a special role in that body of knowledge. For example, one is not expected to give much of a reason for that kind of statement. But we do not imply that the statement is necessarily *true*, although, of course, it is thought to be true by someone whose knowledge that body of knowledge is. (Putnam 1962a, p. 240)

Putnam later noted that this talk of necessity relative to a body of knowledge is not accurate, since to say that a statement is necessary or that a belief is knowledge implies that they are true, and the truth if the relevant statements and beliefs is never guaranteed. He recommended, instead, that we speak of "*quasi*-necessity" relative to a "conceptual scheme" (Putnam 1994, p. 251).

It is in terms of these notions that Putnam understands the notion of a contextually a priori statement. He presupposes, in effect, the following explication of "contextually a priori":

(1) A statement or belief is contextually a priori for a given speaker at a given time if and only if it is quasi-necessity relative to her conceptual scheme at that time.

Given (1), the key additional idea is that contextually a priori statements "can be overthrown only by conceiving of whole new theoretical structures." The result is what I call Putnam's

> *Conceptual scheme explanation*. What explains why it is epistemically reasonable for an inquirer to accept a statement or belief that is *quasi*-necessary relative to her conceptual scheme, despite the fact that the quasi-necessity for her of the statement or belief is not a guarantee that it is true, is that such a statement "can only be overthrown by a new theory—sometimes by a revolutionary new theory—and not by observation alone…." (1983a, p. 95)

The problem with this explanation is that it does not fit with the practical methodological considerations that I highlighted in §§IV–VI. For the explanation presupposes that

(2) A statement is *quasi*-necessity relative to an inquirer's conceptual scheme if and only if the statement can only be overthrown by a new theory.

And (1) and (2) together imply

(3) A statement is contextually a priori for a given speaker at a given time if and only if the statement can only be overthrown by a new theory.

If one accepts (R3), my proposed explication in §II of the notion of contextual apriority, however, the practical methodological considerations that I highlighted in §§IV–VI show that both the left-to-right and the right-to-left directions of the conditionals in (3) are false. Consider first the left-to-right direction—namely, if a statement can only be overthrown by a new theory, then it is contextually a priori. For a scientist in the 18th century who did not do her due diligence, it was not epistemically reasonable to accept that physical space is Euclidean, hence that statement was not contextually a priori for her, despite the fact that it would take over a century of work to see an alternative to Euclidean geometry. Hence the truth of the antecedent of the left-to-right conditional is not sufficient for the truth of its consequent. The right-to-left direction of (3)—namely, if a statement is contextually a priori for a given speaker at a time, then it can be overthrown only by conceiving of whole new theoretical structures—is also false. As Russell's discovery that Frege's Basic Law V is inconsistent shows, sometimes all it takes to undermine a contextually a priori statement is to ask a question about it that no one even considered before. Thus a person (e.g. Frege) may have a contextually a priori entitlement to accept a given statement (Basic Law V) even if little, if any, new theorizing is needed to specify a way in which the statement is, or may actually be, false.

There are, I grant, important methodological differences between Einstein's discovery that the geometry of physical space is non-Euclidean and Russell's discovery that Frege's Basic Law V is inconsistent. The problem with Putnam's conceptual schemes explanation is that these methodological differences do not explain the difference between having and not having a contextually a priori entitlement to accept a statement. A statement S is contextually a priori for a person at a given time only if she is unable to specify a way in which S may actually be false, in the practical sense of "unable" that I explained in §§IV–VI. Putnam's conceptual schemes explanation of contextual apriority leaves out these practical methodological considerations, and substitutes for them the very different methodological consideration of whether it would take a great deal of new theorizing to overthrow the statement. One key difference between these considerations is that former are *synchronic*—they concern what an inquirer can do at a given time (i.e., whether at that time she can specify a way in which a given statement may actually be false)—whereas the methodological considerations that Putnam highlights, by contrast, are *diachronic*—they concern whether an inquirer could later come to reject or revise a given statement without developing a whole new theory.

One might think that these two types of considerations have a closer relationship to each other than my explanation of contextual apriority implies. Let us say that a statement is *deep* for a person if and only if she would have to develop a fundamentally new way of thinking even to conceive of how that statement may actually be false. One might suppose that in order for an inquirer to have a contextually a priori entitlement to accept a given statement S, he must both satisfy the synchronic criteria I describe and in addition, believe that S is deep for him. But this cannot be quite right. For if he believes that S is deep for him, he will likely also believe that with enough time and hard work the statement would cease to become deep for him. And if he believes that with enough time and hard work the statement would cease to become deep for him, he should conclude that has not yet completed his due diligence—he should continue searching for a way in which the statement may actually be false until he is confident that more work will not uncover some hitherto overlooked way in which the statement may be false. We may infer that if he has done his due diligence, hence satisfies the synchronic criteria I described above, and he believes that S is deep for him, then he does not also believe that with enough time and hard work the statement would cease to become deep for him.[4] But when an inquirer satisfies these conditions, the truth of his belief that the statement is deep for him is not relevant to whether he has a contextually a priori entitlement to accept the statement. For example, as I argued above, Frege had a contextually a priori entitlement to accept Basic Law V, but that law was, in fact, not deep for him.

One might nevertheless think that an inquirer could not take himself to have done his due diligence in accepting a statement that is contextually a priori for him unless he believes that the statement is deep for him. As we have seen, however, what matters for contextual apriority is what an inquirer can do at a given time—whether or not he can specify a way in which a given statement may actually be false—and this consideration is independent of whether or not the statement is deep for him. Moreover, it is unclear how S's being deep for him could be viewed as an additional requirement for it being epistemically reasonable to him to accept S. Inquirers cannot survey their own conceptual scheme from a standpoint outside of it.[5] They may form beliefs about whether various statements they accept are deep for them. However, unless they also believe that with enough time and hard work the statements would cease to become deep for them—an attitude that, as we saw in the previous paragraph, is incompatible with the supposition that they have done their due diligence—their beliefs that the statements are deep for them simply reflect, and do not provide any additional support for, their own best practical and synchronic judgments about whether the statements have systematic import

[4]One of my central arguments in Ebbs 2015 against the conceptual schemes explanation in effect presupposes that an inquirer believes that a statement is deep for him only if he also believes that with enough time and hard work the statement would cease to become deep for him. Putnam 2015b, p. 417, rightly objects that there is no incompatibility between an inquirer's believing a statement is deep for him and his having a contextually a priori entitlement to accept it. This is so, I now see, if the inquirer does not also believe that with enough time and hard work the statement would cease to become deep for him. As I explain in the text following this footnote, however, in such situations the assumption that a person's belief that a given statement is deep for him is not what explains why it is contextually a priori for him.

[5]Here I agree with Putnam: "The illusion that there is in all cases a fact of the matter as to whether a statement is "necessary or only quasi-necessary" is the illusion that there is a God's-Eye View from which all possible epistemic situations case be surveyed and judged; and that is indeed an illusion." (Putnam 1994, p. 258)

for them and whether they can specify any ways in which the statements may actually be false.[6]

It might appear that this objection to Putnam's conceptual scheme explanation rests on a classical skeptical worry—the worry that if an inquirer judges that a given statement is deep for her at a given time, she may have overlooked something which, if she later considers it, will show her immediately that the statement was not deep for her.[7] But my central objection to Putnam's conceptual scheme explanation is not based on skepticism about what is deep for us. The objection is not that we can never know that a statement is deep for us (although that may be true), but that the methodological criteria for being contextually a priori are practical and synchronic, not theoretical and diachronic, as are the criteria for a statement's being deep.

In reply to my observation that Frege's acceptance of his Basic Law V was contextually a priori for him before he received Russell's letter—an observation I first made in "Putnam and the Contextually A Priori" (Ebbs 2015) —Putnam writes:

> There is a sense in which any significant group of beliefs could be overthrown by showing them to be *logically* inconsistent. The task would then the rise of finding a way of repairing the breach in our scientific system. If you like, instead of saying, as I did, that a framework principle can be refuted "only by conceiving of whole new theoretical structures," I could have written (had I worried about the problem of hidden logical inconsistencies): "only by conceiving a whole new theoretical structures, or by showing it to be inconsistent, in which case the scientific community will be forced to conceive of a whole new theoretical structure." And "conceive of a whole new theoretical structure" is precisely what Russell did with his theory of types. A logical contradiction can, indeed, sometimes be overlooked; but the possibility of non-Euclidean geometry (or, in the case of the principle of determinism, the possibility of indeterministic quantum mechanics) is not something one simply "overlooks." (Putnam 2105b, p. 416)

I agree with all of these methodological observations. The problem, however, is that they do not address Putnam's problem as I formulate it in §II. Among the statements that are contextually a priori for us at a given time are some logical and mathematical statements that we accept without proof at that time. As Putnam of course knows, Gödel's second incompleteness theorem implies that we cannot prove the consistency of any logical or mathematical proof system PS that we use unless we presuppose a stronger logical or mathematical proof system PS′. But PS′ must then also be part of our total theory, and, by Gödel's second incompleteness theorem, again, the consistency of PS′ cannot be proved within PS′. There is no way to avoid taking a stand on whether one's logic and mathematics are consistent, and no such stand has the logical or mathematical resources

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[6]That these latter judgments are practical and synchronic follows from my proposed explications of "systematic import" (in §II) and of "person *x* is unable to specify a way in which S may actually be false" (in §§IV–VI).
[7]Putnam raises this objection in Putnam 2015b, p. 415.

to prove its own consistency. Our acceptance without proof of the consistency logical and mathematical truths is of a piece with the synchronic, practical point of view on the methods of inquiry that is key to understanding why it is epistemically reasonable for us to accept statements that are contextually a priori for us. The case of Frege's Basic Law V and the methodological investigations in §§IV–VI therefore show that the question whether a statement is deep for us — i.e., the diachronic question whether we would have to develop a fundamentally new way of thinking even to conceive of how that statement may actually be false —is different from the question whether it is epistemically reasonable for us to accept it. Finally, while it is true, as Putnam observes, that when a statement we once relied on is found to be inconsistent, we are "forced to conceive of a whole new theoretical structure," this observation does not help to explain the sense in which, before we discovered that the statement is inconsistent, it was epistemically reasonable for us to accept it.

I conclude that Putnam's conceptual schemes explanation is unsuccessful. If we adopt the methodological principle summarized by (MP), as I recommend, then it is only by investigating the methods of inquiry and related requirements of due diligence in which our contextually a priori statements are embedded that we can explain (make clear) why it is epistemically reasonable for us to accept them.

Acknowledgements

This paper is a development of ideas I first articulated in my paper "Putnam and the Contextually A Priori" (Ebbs 2015), which was completed in 2003. In July 2009, Putnam showed me his generous and brilliant reply to that paper; his reply was published as Putnam 2015b. Despite Putnam's reply, I remain convinced, for essentially the same reasons I presented in "Putnam and the Contextually A Priori," that his conceptual schemes explanation is unsuccessful. In the present paper I offer what I take to be a clearer, more direct explanation of why the conceptual schemes explanation fails. Several paragraphs of §I are lightly revised versions of paragraphs in Ebbs 2017 and Ebbs Forthcoming. Much of §II and parts of §§VI and VII draw on material from unpublished papers that I presented in 2011 at a Harvard University Philosophy Department Colloquium, and at a meeting of the Kentucky Philosophical Association; in 2012 at a University of Illinois-Urbana Philosophy Department Colloquium, at a Midwest Epistemology Workshop, and at a conference at the University of Sydney, Australia. For helpful comments on these and other occasions I thank Kate Abramson, Selim Berker, Albert Casullo, Matt Carlson, Sandy Goldberg, Mark Kaplan, Maria Lasonen-Aarnio, Adam Leite, Kirk Ludwig, David Macarthur, Richard Moran, Tim O'Connor, Jim Pryor, Mark Richard, Tim Scanlon, Brian Weatherson, Steve Wagner, and Joan Weiner. I also thank Joan Weiner for pointing out to me passages by Frege in which he explains his methodological attitude toward his Basic Law V, and Matt Carlson for very helpful written comments on the penultimate draft.

Works cited

Auxier, R., Anderson, D. R., and Hahn, L. E. (eds.) 2015. *The Philosophy of Hilary Putnam*. Chicago, Illinois: Open Court.

Boghossian, Paul. 2000. "Knowledge of Logic," in Boghossian and Peacocke 2000, pp. 229–255.

2001. "How Are Objective Reasons Possible?" *Philosophical Studies* 106: 1–40.

Boghossian, Paul, and Peacocke, Christopher, (eds.), 2000. *New Essays on the A Priori*. Oxford: Oxford University Press.

Bonjour, Laurence, 1998. *In Defense of Pure Reason*. Cambridge: Cambridge University Press.

Cantor, Georg, 1899. "Letter to Dedekind," in van Heijenoort 1967, pp. 113–117.

Carnap, Rudolf, 1934. "The Task of the Logic of Science," an English translation of a paper originally published in German in 1934. In McGuinness, Brian, (ed.), *Unified Science: the Vienna Circle Monographs Series Originally Edited by Otto Neurath*. Dordrecht: D. Reidel, 1987, pp. 46–66.

Courant, Richard, and Robbins, Herbert, 1958. *What is Mathematics? An Elementary Approach to Ideas and Methods*. London: Oxford University Press.

Ebbs, Gary 2015. "Putnam and the Contextually A Priori," in Auxier, Anderson, and Hahn, 2015, pp. 389–411.

2017. "Putnam on Methods of Inquiry," *Harvard Review of Philosophy*, Vol. XXIV, pp. 121–125.

Forthcoming. "Analyticity: the Carnap-Quine Debate and its Aftermath," in Becker and Thomson, eds., *Cambridge History of Philosophy:* 1945–2015.

Davis, Martin, 1982. *Computability and Unsolvability*. New York: Dover Publications, Inc.

Floyd, Juliet, 2000. "Wittgenstein, Mathematics, and Philosophy," in Alice Crary and Rupert Read, eds., *The New Wittgenstein* (London: Routledge), pp. 232–261.

Frege, Gottlob, 2013. Basic Laws of Arithmetic, Volumes I and II.

Katz, Jerrold, 1998. *Realistic Rationalism*. Cambridge, Mass.: MIT Press.

Kleene, S. C. 1952. *Introduction to Meta-Mathematics*. Amsterdam: North-Holland Publishing Co.

Matiyasevich, Yuri, 2016. "Martin Davis and Hilbert's Tenth Problem," in Eugenio G. Omodeo and Alberto Policriti, eds., *Martin Davis on Computability, Computational Logic, and Mathematical Foundations* (Cham, Switzerland: Springer International Publishing, 2016), pp. 35–54.

Peacocke, Christopher, 2000, "Explaining the A Priori: The Programme of Moderate Rationalism," in Boghossian and Peacocke 2000, pp. 255–286.

Priest, G. 1998. "What is so Bad about Contradictions?" in *The Journal of Philosophy* 95/8 (1998), pp. 410-426

Putnam, Hilary 1962a. "It Ain't Necessarily So," reprinted in Putnam 1975a, pp. 237–249.

1962b. "The Analytic and the Synthetic," reprinted in Putnam 1975b, pp. 33–69.

1975a. *Mathematics, Matter, and Method: Philosophical Papers, Vol. 1*. Cambridge: Cambridge University Press.

1975b. *Mind, Language, and Reality*: *Philosophical Papers, Volume 2*. Cambridge: Cambridge University Press.
1983a. " 'Two Dogmas' Revisited," in Putnam 1983b, pp. 87–97.
1983b. *Realism and Reason: Philosophical Papers, Volume 3*. Cambridge: Cambridge University Press.
1994. "Rethinking Mathematical Necessity," in Putnam, *Words and Life* (Cambridge, Mass.: Harvard University Press, 1994), pp. 245–263.
2015a. "Intellectual Autobiography," in Auxier, Anderson, and Hahn 2015, pp. 3–110.
2015b. "Reply to Gary Ebbs," in Auxier, Anderson, and Hahn 2015, pp. 412–417.
Quine, W. V. 1953a. "Two Dogmas of Empiricism," first published in *The Philosophical Review*, in 1951; reprinted, with changes, in Quine 1953b, pp. 20–46.
1953b: *From a Logical Point of View*. Cambridge, Mass.: Harvard University Press.
1960: *Word and Object*. Cambridge, Mass.: MIT Press.
1974: *The Roots of Reference*. La Salle, Illinois: Open Court Press.
Rey, Georges, 1998. "A Naturalistic A Priori," *Philosophical Studies* 92 (1998): 25–43.
Reichenbach, Hans, 1958. *The Philosophy of Space and Time.* English translation, by Maria Reichenbach and John Freund, of *Philosophie der Raum-Zeit-Lehre*, first published in 1928. New York: Dover Publications, Inc.
Russell, Bertrand, 1902. Letter to Frege, in van Heijenoort 1967, pp. 124–125.
van Heijenoort, Jean (ed.) 1967. *From Frege to Gödel. A Source Book in Mathematical Logic, 1897–1931*. Cambridge, Mass.: Harvard University Press.