"Problems with Quine's Indispensability Argument"

Chapter 2, Part 1 of *Numbers without Science*

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§1.1: Introduction

Once again, consider QI.

(QI) QI.1: We should believe the theory which best accounts for our empirical experience.
QI.2: If we believe a theory, we must believe in its ontic commitments.
QI.3: The ontic commitments of any theory are the objects over which that theory first-order quantifies.
QI.4: The theory which best accounts for our empirical experience quantifies over mathematical objects.
QI.C: We should believe that mathematical objects exist.

Field, like the proponents of modal reinterpretations, denied QI.4. I show that the problems with QI arise earlier in the argument. While I discuss problems with QI.1, and QI.2, I focus mainly on QI.3, which encapsulates QP, Quine's procedure for determining ontic commitments.

Quine seeks a single formal language for revealing our commitments because of the homogeny that underlies QI.1and QI.2. In §1.2, I account for and criticize Quine's homogeny, and mention a few difficulties with the physicalism of QI.1. In §1.3, I deny the confirmation holism which underlies Quine's homogeny.

My main concern, though, is with QI.3. QP purportedly allows us to reveal our commitments without ontic prejudice: we regiment our ideal theory and the commitments fall out of it. This method misrepresents the way in which we determine our commitments. Instead, we regiment our preconsidered commitments. If, like Quine, we are predisposed to nominalism, regimentation should not commit us to mathematical objects.

Against QP, I argue in §1.4 - §1.7 that we should not look to first-order versions of scientific theory for our commitments. There are many useful logics, some of higher order, some which include names. None of them are the unique language for expressing our ontic commitments. The method Quine suggests at QP.3 is faulty. My criticisms will thus apply both to the use of first-order logic as canonical language, and to the way in which Quine reads the commitments from a regimented theory. Without QP, Quine fails to generate ontic commitments to mathematical objects.

Quine notes a trap into which he falls, that of favoring a formal tool for the wrong reasons. He writes that anthropologists, carried away with the virtues of symbolism and first-order logic, overemphasize the notion of kinship.[1] I argue that Quine’s reliance on first-order logic arises similarly, from overemphasizing its structural virtues which do not justify choosing that language to reveal our commitments exclusively.

§1.2: Homogeny

QI.1 reflects Quine’s physicalism. Combined with QI.2 it reflects Quine’s homogeny.[2] In this section, I mention two difficulties with physicalism and argue that homogeny is ill-motivated.

First, on a literal reading, Quine’s physicalism seems to eliminate the existence of ordinary objects. There are no people or trees or brick houses on Elm Street. We may use first-order logic to regiment sentences about ordinary objects, but such work is irrelevant to questions about what exists. Our real commitments are only to be found in our physics. There are only particles, or strings, or whatever objects are yielded by a mature scientific theory.

Second, the physicalist assumes not only that all objects are reducible to the elements of complete physics, but also that explanations of all events, including intensional ones, are in principle available at that level. Putnam, in later work, voiced objection to this assumption of Quine’s.

[1] See Quine (1978a) p 154.

[2] See Chapter 1, §1.2 (physicalism) and §1.3 (homogeny).

> The fact is that most of science and meta-science cannot even be expressed in a perfectly precise notation (and all the more so if one includes philosophy under the rubric ’meta-science’ as Quine does). Words such as ‘normally’, ‘typically’, etc, are indispensable in biology and economics, not to mention law, history, sociology, etc.; while ‘broad spectrum’ notions such as ‘cause’ and ‘factor’ are indispensable for the introduction of new theoretical notions, even if they do not appear in ‘finished science’, if there is such a thing. Philosophy cannot be limited to commentary upon a supposed ‘first-class conceptual system’ which scarcely exists and whose expressive resources cover only a tiny fragment of what we care about. (Putnam (1979) p 132)

A scientific theory (e.g. a version of Newton’s laws of motion) may quantify over centers of mass or only space-time points. Still, the objects of our ordinary experience are physically real, even if our best theory does not quantify over them, and may not even be able to define them adequately. Jody Azzouni argues that a theory which described my bat hitting a ball in terms of elementary particles would not even describe that phenomenon. “Even a fundamental physical theory (on this view) could be *true* without what it is true of appearing among its predicates; for the things that *are* physically real could be too gross in nature (baseball bats, laboratory apparatus) to be even *definable* in terms of that theory’s predicates.” (Azzouni (1997b) p 203)

I will not pursue my concerns about reductive physicalism here. Even if we jettison Quine’s physicalism, we can formulate QI.1.

Quine’s homogeny rests on his insistence that there is a single way of knowing anything, that all evidence is sensory evidence. Call this his demand for a uniform epistemology.[3] Quine’s main positive argument for uniform epistemology comes from the web of belief metaphor. He cashes out this metaphor as a thesis about meaning, and the distribution of content. The resulting semantic holism is stronger, and more contentious than he needs for the indispensability thesis, which depends merely on confirmation holism. I raise concerns about confirmation holism in the next section.

Another part of Quine’s argument for his uniform epistemology involves an account of the

[3] One can see how QI relies on Quine’s uniform epistemology by noting that if we drop the requirement for uniformity, then mathematics may be justified independently of science, and the indispensability argument may become superfluous.

origins of the referential terms of our language, an account which Jaegwon Kim has criticized for lacking normativity.[4] In the remainder of this section, I show that Kim is wrong that Quine's epistemology lacks a normative element. But, Quine supports homogeny with a descriptive account which is faulty in the way Kim argues.

There are two sorts of stories which might serve as epistemology, for Quine. First, there is a purely descriptive account of how we acquire our referential vocabulary. In *Roots of Reference*, Quine traces how we learn referential terms of natural language, including mathematical terms, from observation sentences, through the distinction of singular terms from general terms, to the positing of abstracta and adoption of idioms like 'there are'. "Our general objective was a better understanding of how scientific theory can be achieved." (Quine (1974) p 81)

As a naturalist science project, Quine's account is objectionable only on scientific grounds. But Quine has another goal. He relies on these genetic musings, his descriptive epistemology, to support his uniform empiricist, broadly behaviorist, epistemology. He traces the roots of reference to show that empirical evidence suffices to include mathematical terms in our best theory.

Quine might be right about the origins of our referential terms, but this is no argument for the way in which we justify our knowledge of their referents. The origins of our beliefs are independent of their justifications. I may learn that 7+5=12 by counting apples and chairs, but my knowledge that 7+5=12 transcends experience with simple physical collections. As Kim argues, justification is normative, not descriptive. "For epistemology to go out of the business of justification is for it to go out of business." (Kim (1988) p 43)

If one were attempting to formulate reductions of the objects of knowledge to sense-data, say, then a genetic account would be most illuminating. If one starts, as Quine does, with objects, then one needs merely a good theory to account for them, however we learn the language in which our knowledge

[4] See Kim (1988).

is couched.

The divergence of origins and justification is especially important for Quine, who believes that abstracta are causally isolated from human sensory organs. For, whatever justificatory story we tell about our knowledge of mathematical objects, it will have to be independent of the experiential account of our acquisition of language.

Quine does provide a justificatory story about mathematics, of course. The indispensability argument, in fact the whole project of constructing and interpreting our best theory, is Quine's second type of epistemological story. This project is normative: we should believe in all, and only, the objects in the domain of our best theory.

Kim's claim that Quine has neglected the normative aspect of epistemology is wrong. Still, Quine does lean on the account of the origins of our beliefs in defending a uniform epistemology which leads directly to homogeny. Quine favors first-order logic in part because it unifies the referential apparatus we have acquired. That part of the account does fail to be normative. Consequently, the uniform epistemology which underlies the indispensability argument is ill-motivated.

In the next section, I continue to argue against homogeny by providing considerations against Quine's confirmation holism.

§1.3: Confirmation Holism and Disciplinary Boundaries

Homogeny consists of two claims. The first is Quine's confirmation holism, that we can construct a single best theory and any evidence we have for any part of that theory is actually evidence for all parts of the theory. The second is that our ontic commitments are found by examining the posits of that best theory. In the previous section, I argued that the uniform epistemology which underlies Quine's homogeny is wrongly motivated in part by a confusion between the origins of our beliefs and their justifications. In this section, I attempt to undermine holism.

Quine argues for holism, his allegation that our beliefs face the tribunal of experience only when taken together, from a quick, uncontroversial logical point. Any sentence can be held without contradiction and come what may as long as consequent adjustments are made to the background theory. I argue, following Elliot Sober, that Quine's holism ignores the important differences between posits of mathematical objects and posits of empirical objects.[5] In practice, we shield mathematics from empirical refutation, even if the logical point is correct.

Holism is suspect precisely when it comes to mathematics. Prima facie, no empirical evidence would force us to give up our beliefs about the numbers, even though we could, with but a little effort, imagine evidence that would force us to give up beliefs in the existence of electrons, or other subvisible particles.

Quine argues that this commonsense distinction between mathematics and empirical science is illusory because all objects are posits, including ordinary ones. "Physical objects, small and large, are not the only posits. Forces are another example; and indeed we are told nowadays that the boundary between energy and matter is obsolete. Moreover, the abstract entities which are the substance of mathematics - ultimately classes and classes of classes and so on up - are another posit *in the same spirit*." (Quine (1951) p 45, emphasis added)

In lieu of the commonsense distinction, Quine presents a continuum of commitments from our most firm and central, to our most tenuous and peripheral. We are unlikely to give up our beliefs in ordinary empirical objects either, but any posit may be questioned. Starting with ordinary objects, as Quine does, is no guarantee of ending with them. If on a scientific basis, say, Berkeleyan idealism turned out to be a better theory (e.g. more useful, simpler) then we should in fact abandon beliefs in physical objects. We can cede any belief, including our basic mathematical ones.

On the other hand, if no empirical, scientific reasons would sway us to abandon our mathematical

[5] Sober calls the doctrine epistemological holism, but it is the same holism.

beliefs, then we seem to have the basis for a distinction which will undermine holism. Alan Musgrave argues that we would not give up our mathematical beliefs because they exist necessarily. “If natural numbers do exist, they exist of necessity, in all possible worlds. If so, no empirical evidence concerning the nature of the actual world can tell against them. If so, no empirical evidence can tell in favour of them either.” (Musgrave (1986) p 91)

Appeals to necessity, and possible worlds, are notoriously tendentious, though Musgrave’s use of modality is not particularly problematic. Musgrave’s necessity is essentially the one which underlies Field’s claim of conservativeness for mathematics. Mathematical theories are supposed to be compatible with any empirical theory.

By itself, Musgrave’s contention is insufficient. For, Quine has a familiar response which refers to the centrality of beliefs we never cede. Like logical principles, mathematical beliefs are interconnected with our other beliefs in such an integral way that abandoning them would always force impractical redistributions of truth values among the remaining components. As a practical matter, we never give them up, even though we could, in principle. The appearance of necessity remains a decision, because we can always choose to give up something other than the mathematical elements of our theory. “If asked why he spares mathematics [in revising his theory in the face of recalcitrant experience] the scientist will perhaps say that its laws are necessarily true; but I think we have here an explanation, rather, of mathematical necessity itself. It resides in our unstated policy of shielding mathematics by exercising our freedom to reject other beliefs instead.” (Quine (1992) p 15)[6]

Sober calls holism into question with a different explanation of why we never cede mathematical beliefs on the basis of empirical experiment. We subject mathematical claims to completely different

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[6] Resnik, supporting Quine, argues that the holist can account for the apparent apriority of mathematics pragmatically, too. “‘Good sense’, in the form of pragmatic rationality, underwrites the special role mathematics has come to play in science and bids us to treat it *as if* it were known a priori.” (Resnik (1997) p 120)

kinds of tests, and do not hold them open to refutation on the basis of empirical evidence.

Sober calls the problems which confront science discrimination problems. We evaluate a scientific hypothesis against other hypotheses. We are only able to do this when other hypotheses are available. Sober calls this description of scientific methodology contrastive empiricism. Experiments solve discrimination problems among competing hypotheses by providing evidence in favor of one or another. For example, Sober considers these three competing hypotheses:

(Y_1) Space-time is curved.
(Y_2) Space-time is flat.
(Y_3) Space time is not curved, although all evidence will make it appear that it is.

Empirical evidence will discriminate between Y_1 and Y_2, but no evidence will discriminate between Y_1 and Y_3. Similarly, no discrimination problem can help us to confirm the truth of mathematical statements, or the existence of mathematical objects. "If the mathematical statements *M* are part of every competing hypothesis, then, no matter which hypothesis comes out best in the light of the observations, *M* will be part of that best hypothesis. *M* is not tested by this exercise, but is simply a background assumption common to the hypotheses under test." (Sober (1993) p 45)

Sober thus shows that Quine's allegation that it is always in principle possible to cede any beliefs in light of recalcitrant experience, is in fact contradicted by the ways we test our hypotheses. Our tests ensure that mathematical beliefs are never called into question. Sober provides examples of everyday failures of additivity: two gallons of salt and two gallons of water do not yield four gallons of salt water; two foxes and two chickens yield only two fat foxes and a pile of feathers. If Quine's holism were right, there should in principle be an option of giving up our mathematical beliefs in such cases. If all examples where we would cede mathematical beliefs are unavoidably abstruse, Quine's doctrine appears suspect.[7]

[7] Sober is agnostic about whether empirical evidence can ever influence our beliefs about number theory. See Sober (1993) pp 36-37, fn 5.

Quine notes that his holism is not a practical matter. Regarding "Two Dogmas of Empiricism," he writes, "All we really need in the way of holism... is to appreciate that empirical content is shared by the statements of science in clusters and cannot for the most part be sorted out among them. Practically the relevant cluster is indeed never the whole of science; there is a grading off..." (Quine (1980a) p viii) While Quine refers to the stronger semantic holism, his point is that there is a factual element in the content of any sentence, and thus an ineliminable component open to confirmation or refutation in every sentence, including those of mathematics. Quine is ceding that these elements may be undetectably subtle. Sober's contention is that they are not there.

Sober's argument relies on practical differences in testing. Resnik denies that differences in practice refute holism. "Sober is right that in practice we rarely, if ever, put mathematical laws to the sorts of specific tests that we apply to some scientific hypotheses. But this does not imply that purely logical considerations show that mathematics is immune to such testing." (Resnik (1997) p 124)

Sober need not establish a difference between mathematical and empirical posits on a logical basis in order to establish the distinction. Quine's naturalism is a commitment to the methods of science. Scientific methodology holds mathematical principles immune from revision.

Azzouni also recognizes a difference in kinds of posits despite accepting Quine's point about the logic of confirmation. "[D]espite the fact that every posit is treated in the same way, logically speaking, by quantifiers in a theory, nevertheless, mathematical posits get into scientific theories *the wrong way*." (Azzouni (1997a) p 481)

If Sober's criticism of holism is correct, then different elements of our best theory are separate and isolatable. Resnik challenges Sober to provide non-arbitrary lines between logic, science, and mathematics, admitting that holism would be refuted if one could establish, "[A]n epistemically principled division between the empirical and formal sciences. But I do not see much hope of success here." (Resnik (1997) p 135)

One way to distinguish among logic, mathematics, and science is by the ontology they require. Physical science likely entails no more than denumerably infinitely many objects, while mathematics demands more. Also, the types of space with which mathematicians are concerned exceed even the most tutored intuitions. The oddity of a space does not count against the existence of a topological surface. Odd physical spaces, such as regions near large dense masses, require significant upheaval of physical theory.

Putnam suggests that we can distinguish mathematics from science by the fact that scientific theories have viable competitors, whereas mathematical theories lack them.[8] This methodological difference echoes Sober's point.

There are two independent points here. The first is that we can make a principled distinction between mathematical objects and empirical objects, and between mathematics and empirical science, which Resnik concedes would refute holism. The second point is Azzouni's claim that mathematical objects get into our empirical theories in a different way than empirical objects do. There is a difference between positing an element into an already-existing framework, as we do with electrons, and positing an entire abstract realm.[9] Consider how mathematical objects get added to a theory on the holist's picture. We do not start with them, as we do with trees. We do not set out to describe the behavior of mathematical systems. We construct a theory without reference to mathematical objects until we find that our theory requires them for the account of other phenomena. Then, we add mathematical axioms only as far as the theory requires them.

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[8] Putnam (1967b). Putnam argues that mathematics and science require the same epistemology, but this depends on his argument that mathematics is quasi-empirical. In the absence of such a successful argument the distinguishing characteristic stands.

[9] The claim that mathematical posits are different kinds of posits from posits of subvisible objects is one that both realists and nominalists can agree upon. See, for example, Cornwell (1992), which argues for nominalism, against indispensability, on the basis of a counterfactual interpretation of the mathematical posits of a theory.

The case is different with subvisible particles. If we add axioms which commit to new particles, like those for quantum physics, for example, we do so on the basis of the inadequacy of prior axioms to account for the empirical phenomena. When we introduce electrons, we include a story about the physical relation between the electrons and the bodies which actually concern us. Trees are made of subatomic particles, they are not made of sets. When we adopt mathematical entities, there is no effect which they are postulated to cause. We never construct experiments to observe them, or seek, in Azzouni's terms, thick epistemic access to them.[10] We are just forced to quantify over mathematical objects by the desire for greater facility in manipulating descriptions of physical situations.

Quine claims that all posits are made in the same way, but theoretical posits receive justification from within the domain that posits that object. An indispensability claim bridges two independent disciplines, presuming disciplinary blur.

Parsons raises another objection to Quine's assimilation of theoretical posits and indispensability claims. High-level theoretical posits tend to be made tentatively. Propositions involving such posits are speculative, and hotly debated, in contrast to the obviousness of mathematics. In mathematics, we have, "The existence of very general principles that are universally regarded as obvious, where on [a Quinean] empiricist view one would expect them to be bold hypotheses, about which a prudent scientist would maintain reserve, keeping in mind that experience might not bear them out..." (Parsons (1980) p 152)

Within empirical science, Quine's confirmation holism may hold. Justification may well be spread throughout empirical theory and the web of belief, restricted to our empirical beliefs, may remain a useful metaphor. The extension of this picture to mathematical objects is unjustified.[11]

Quine insists that all philosophical questions are to be answered using scientific methods. To

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[10] See Azzouni (2004) p 383.

[11] Benacerraf (1973) assimilates the posits of numbers and electrons based on grammatical roles. But even if we grant that a uniform semantics shows that both numbers and electrons exist, they may be justified differently.

hold that science must test mathematical statements as it tests empirical ones is to favor a methodology based not on science but on prior philosophical prejudice. Mathematical theories are tested differently from empirical ones, undermining homogeny.

The first two steps of QI involve settling on a single physical theory in which to express all of our commitments. In the past two sections, I have argued that our commitments are not all made in the same way by the same theory. Still, even if we grant QI.1 and QI.2, QI depends on Quine’s general procedure for determining ontic commitments, which I deny in the next four sections.

§1.4: Names

In this section and the following, I argue that Quine’s preference for first-order logic over other formal languages is unjustified. First, I show that Quine’s argument that the first-order existential quantifier is the best tool for indicating existence since it is a natural cognate of ‘there is’ is undermined by his elimination of names.

Against looking to names to find the commitments of a theory, Quine points to four problems. Some names do not refer. We often find reference in terms which do not look like names on the surface, in pronouns for example. There are not enough names. And, there is a profound conflict between names and quantifiers.

The problem of non-referring names concerns ‘Pegasus’ and nouns like ‘sake’ which look, grammatically, as if they refer. That there are such terms does not decide the matter in favor of quantifiers, though, since we can easily form an existentially quantified statement which also seems to commit us to the existence of Pegasus. Both names and quantifiers may be used to reflect real and errant commitments. We can be clear about when we intend to use an empty name. Similarly, the problem of diffusion of reference seems less like an argument for eliminating names, and more an argument to be careful when approaching questions of reference.

To the problem of not having enough names for all objects, we may respond by adopting an infinite language with enough names, or by dropping the requirement that every object have a name.

Given that we must choose between names and quantifiers, Quine favors eliminating names. But, eliminating names makes Quine's formal language less natural, since names are naturally taken as indicating reference. Quine's worries about names are avoidable without eliminating them. Quine appropriately requires that theories be rigorously constructed if used to express commitments. We must beware of the sloppiness of ordinary language. This is merely counsel to be careful with whichever language we choose. We have to ensure that the language expresses the real commitments of the theory. If we are clear about our commitments, the choice to include names or not is arbitrary. Independently of Quine's preference for first-order logic, it is hard to see why names should not refer.

Quine's argument for taking the existential quantifier as indicating commitment was based on its natural equivalence with 'there is', but Quine uses this criterion to suit his independent purposes, and not as a principled guide. Languages with names are more natural in that they are more perspicuous. Using names facilitates inference. The naturalness of using the existential quantifier for 'there is' is counterbalanced by the ease of using a perspicuous language with names.

We can adopt, for the purpose of revealing the ontic commitments of a theory, a language less formal than first-order logic, a language with names and no quantifiers. We may use a cleaned-up version of our ordinary language. In the next section, I argue that if we must use a formal language, ones other than Quine's may also be useful.

§1.5: Higher-Order Logics and the Existential Quantifier

Just as he rejects names unjustifiably, Quine rejects higher-order logics for insufficient reasons. Quine presents three concerns about higher-order logics. First, he worries that they make too many commitments. Second, Quine points to a constellation of technical results: the concurrence of a variety of definitions of logical truth, the elimination of names, completeness, that every consistent first-order theory has a model, compactness, and its admission of both upward and downward Löwenheim-Skolem features. Third, he argues that first-order logic avoids referential vagueness. I argue that these concerns are insufficient grounds for choosing first-order logic as the exclusive language for revealing ontic commitments.

The quantifiers of any first-order or higher-order logic have two distinct roles: a purely formal and syntactic inferential role, and a translational role. In their inferential role, they bind variables. They may be used or removed in deductions.

The translational role of the quantifier involves the uses we might make of the formal theory and the meaning we give to the quantifier. It is common to take, with Quine, the existential quantifiers as indicating existence. But we need not do so. We can interpret them as substitutional quantifiers, focusing only on their inferential role.

Azzouni suggests separating the two roles. First-order and higher-order logics force the existential quantifier into an independent role, indicating existence, for which it is not fit. "Even if one accepts the idea that scientific theories must be regimented in first-order languages, nothing requires the first-order existential quantifier...to carry the burden of *ontological commitment*." (Azzouni (1998) p 3)

Abandoning the translational role of the quantifiers, and first-order logic as the language of commitment, leaves the inferential role of the quantifier alone. The same holds for higher-order logics, which Quine rejected for their ontic extravagance. In fact, quantification over properties in higher-order logics may be a virtue. It provides all the predicates we might need for science or mathematics. We need not take quantifications of variables attached to those predicates as indicating commitment.

The technical virtues of first-order logic do not decide the matter, either. For example, the completeness which Quine thinks favors first-order logic only means that every valid formula is derivable. It does not mean that every intuitively valid inference is representable in first-order logic.

There are intuitively valid formulas and inferences which are not valid in first-order logic. For example, first-order logic with identity can not comfortably accommodate inferences to common properties of two individuals, Frege’s definitions of numbers, and Leibniz’s identity of indiscernibles. The completeness of first-order logic is a technical virtue which can make first-order logic useful. Higher-order logics which may accommodate such inferences may also be useful.

Consider the claim EM1: $(\forall x)(Px \vee \sim Px)$. A similar sentence can be written substituting any predicate for ‘P’. This is a higher-order fact, which we can summarize as EM2: $(\forall \wp)(\forall x)(\wp x \vee \sim \wp x)$. Quine rejects EM2, though he accepts every instance of it along the lines of EM1. He prefers to take the ‘P’ in EM1 as a schematic letter. But EM2 provides a uniform representation of the underlying fact which is not present in any sentence of the form EM1.

Quine complains that logics other than first-order logic may be vague. If we focus on a language which is not Quine’s canonical notation, but with the same goal, to explicate existence, there is no reason why that language need be vague. In all cases, we must be clear, antecedently, about our commitments.

Quine’s objections to names and higher-order logics arise from his desire to formulate a single canonical language in which to represent all commitments. If we abandon that method, we may welcome names. We may regiment into first-order logic to clarify our meanings or to reveal deductive relations, or we may use other formal languages when they suit our purposes.

I have argued against QP.2, Quine’s insistence that we look to first-order logic to find our commitments. In the next two sections, I show that the way in which Quine reads commitments from first-order logic is misleading.

§1.6: Wilson’s Double Standard

If we ignore the criticisms of recent sections and adopt, with Quine, QP.1 and QP.2, we have two ways in which we might proceed to determine the commitments of a regimented theory. We can look at its theorems or we can look to the range of its bound variables. QP.3 indicates that Quine takes the latter route. In this section, I present Mark Wilson’s argument that Quine’s choice misreads theoretic commitments. The arguments of this section and the next, which also concern Quine’s way of reading commitments from regimented theories, apply only to Quine’s idiosyncratic approach, which is separable from his more central claims, QP.1 and QP.2.

Wilson demonstrates a difficulty which arises from the translational role of the quantifier. Theories with different existential claims may not differ in ontology. Consider two theories which agree in all elements except that one claims that there is a beagle in Baltimore, and the other denies this claim, putting that beagle in Philadelphia. The two theories have existential claims with different contents, but quantify over the same objects.

In choosing among theories with conflicting existential claims, we first determine if one makes false assertions. The beagle may not be in Baltimore. Second, there may be an equivocation such that the two theories really say the same things. In this case, we will have to rely on some interdefinability to determine whether two theories have the same commitments. Once we generate the appropriate translation, the choice between the two theories is arbitrary. Lastly, we might have to admit that the ontologies of the two theories really differ.

Wilson argues that claims of the former two types, which he calls type A), override claims of the last type, which he calls type B). “[I]f a plausible claim of type A) can be found, it will always *overrule* conclusions reached by B).” (Wilson (1981) p 413)

Wilson’s point is that the ontology of a theory is best found in the intended interpretation of its theorems. When we apply this lesson in science, we find that QP miscounts physical entities as mathematical. Consider items such as vectors in Hilbert space, which are used in quantum mechanics. These objects of applied mathematics do double-duty, once as mathematical in the mathematical theory used by the physical theory, and once as physical, when applied to the physical theory. When applied in the physical theory, the intended interpretation is physical, and not mathematical. Wilson correctly points out that QP counts objects only once, even if they are the subjects of purely mathematical statements, and also the subjects of applied mathematical statements. He calls this single-counting of what should be distinguished and thus counted twice univocalism, and it is a short step from Quine's homogeny. "Univocalism's stress upon amalgamation of one's 'total theoretical corpus' tends to make one miscount entities; instead of separate physical and mathematical entities, it only sees the latter. This moral I call 'the double standard in ontology'." (Wilson (1981) p 417)

Wilson's argument indicates a rift between the ontology of a formal theory, determined by the ranges of the bound variables, and the ontic commitments of the theory, determined by the theorems. He argues that we determine the commitments of a theory by looking at theorems and their intended interpretations, not at the range of variables. "Our *prior* knowledge that two theories, e.g. ZF and [ZF + the axiom of measurability], share the same 'intended structure' may overrule the 'evidence' that they differ ontically displayed in their differing existential theorems." (Wilson (1981) p 412)

Wilson argues that consideration of the double standard supports a structuralist criterion for ontic commitment.[12] Shapiro presents such a criterion, in opposition to QP. On the Quinean method, ordinary set theory and set theory with numbers as ur-elements have different ontologies. "I take this as nearly a reductio against the Quinean criterion." (Shapiro (1993) p 477) On the structuralist criterion, their ontologies are the same

Wilson encourages us, pace Quine and QP.3, to look for our commitments in the theorems of a theory and their intended interpretation. Quine looks instead to the range of the variables. In the next

[12] Wilson argues that the mathematician's grasp of this difference leads to structuralism. The theory which claims that numbers are von Neumann sets has different existential claims from that which claims that the numbers are Zermelo sets. Benacerraf argued that the option of choosing one reduction over the other is closed. To avoid choosing, the structuralist argues that the two reductions are in some sense equivalent. The mathematician, whose interest stops at the theorems, chooses structuralism because he thinks that the differences between the von Neumann and Zermelo reductions are not worth debating.

section, I argue that this is an error.

§1.7: Appealing to the Metalanguage

While QP.3 is excisable from QP, and inessential to QI, it is not a minor element of Quine’s work. It arises directly from Quine’s slogan that to be is to be the value of a variable.[13] It also leads directly to Quine’s doctrine of ontological relativity. In this section, I argue that Quine is wrong to push us to the values of variables in lieu of the intended interpretations of the theorems.

Quine defended taking first-order logic as canonical because while it is refined and precise, it is also easily interpreted. We can readily see the quantifiers as close relatives of the ordinary ‘there is’. But, Quine denies that we should interpret ordinary language at surface value. For example, Quine argues that languages in which quantifiers may be translated away, or which do not contain quantifiers, are unable to generate an ontology. A finite theory which contains names may eliminate quantifiers in favor of truth-functional connectives. This type of theory, Quine claims, will leave no ontic footprint.

> Ontology thus is emphatically meaningless for a finite theory of named objects, considered in and of itself... What the objects of the finite theory are, makes sense only as a statement of the background theory in its own referential idiom. The answer to the question depends on the background theory, the finite foreground theory, and, of course, the particular manner in which we choose to translate or embed the one in the other. (Quine (1968) p 63)

The problem Quine is raising here applies to all languages. A theory can not prescribe its own interpretation. We can not know the references of the names of a language with no quantifiers. In a first-order theory, one must look to a domain of quantification to find values of its variables. Domains of quantification are located within a model for the theory. Theories do not determine their own models, which are written in a metalanguage. Thus, Quine’s dictum forces us to construct a model in a metalanguage in order to discover the commitments of a theory.

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[13] See Quine (1939a), Quine (1939b), and Quine (1948)

Appeal to a metalanguage generates an infinite regress of formalism. If we want to know what the names in the metalanguage refer to, we have to construct a model for the metalanguage, and so on. But if we want to know what the commitments of a theory are, we have to stop somewhere. Quine’s resolution of this matter is ontological relativity, that we have no absolute answers to ontic questions. “What makes sense is to say not what the objects of a theory are, absolutely speaking, but how one theory of objects is interpretable or re-interpretable in another,” (Quine (1968) p 50)

Ontological relativity means that QI is even weaker than it seems, since it yields no commitments to abstract objects. It only yields a theory which may be interpreted as making such commitments. The theory will have other interpretations, given that it will be strong enough to ensure the impossibility of generating a unique intended model.

To avoid ontological relativity, we could take the metalanguage of our first-order theory at homophonic face value. But this pragmatic response will not resolve the problem, as Michael Devitt notes. “We do not need to move into a metalanguage discussion of our object-language claims to establish ontic commitment. Indeed, if commitment could never be established at the level of the object language, it could never be established at all.” (Devitt (1984) p 50)

We have lots of ways to express commitments. We can also make statements whose commitments we deny. We can clarify matters at the level of the object language when pressed. We need not be pushed into a metalanguage. Further, Quine’s argument that we should take the existential quantifier as indicating existence because of its proximity to the ordinary language ‘there is’ is undermined by the claim that we can not interpret ordinary languages as making ontological commitments.

In §1.2 and §1.3, I argued that the homogeny of QI.1 and QI.2 is both poorly motivated and false, and I mentioned some concerns about the physicalism of QI.1. §1.4 and §1.5 concerned problems with QP.2. §1.6 and §1.7 concerned problems with QP.3's requirement that we examine metalanguages.

While QP.3 is easily excised, these worries about the individual steps in QP should make us extremely wary of any argument, like QI, which depends on it.

The major flaw in QP is its reliance on formal theory to reveal our commitments. In the next three sections, I put aside the specific worries about the steps of QP, and present general concerns about this methodology and its application, at QI.3, to the indispensability argument.

§1.8: The Regimentation of Ontic Prejudice

One reason to favor QP is because the clarity of regimented language can help reveal the presuppositions of a theory. This clarity can help us avoid making errant claims. We can regiment scientific theory without consideration of its commitments. We focus on generating a simple and elegant axiomatization. Then, we look to the regimented theory to reveal its existence claims, which are byproducts of a neutral process.

The Quinean picture I just described is misleading, and in this section, I show how. When we regiment, with Quine, to clarify the commitments of a theory, we permit existential generalization only where we desire that the theory express commitment. A nominalist with respect to any kind of entity will cast his theory in a way which avoids commitments which a realist will make. Quine recognizes this. “The resort to canonical notation as an aid to clarifying ontic commitments is of limited polemical power... But it does help us who are agreeable to the canonical forms to judge what we care to consider there to be. We can face the question squarely as a question what to admit to the universe of values of our variables of quantification.” (Quine (1960a) p 243)

For example, consider Quine’s rejection of propositional attitudes as “creatures of darkness.” (Quine (1956) p 188) We do not construct a semantic theory, and then notice whether it quantifies over propositional attitudes. We consider the world, and our minds, and make that decision.

The picture which I called misleading is closely related to the idea that formal theories are uninterpreted, or disinterpreted. Quine (1978a) argues against the disinterpretive stance, which was held by formalists who tried to eschew metaphysical controversy by emphasizing the syntactic properties of mathematical theories. Quine rightly saw that mathematical theories are useless if taken as disinterpreted. They are about mathematical objects, and we can not pretend otherwise.

This is a fairly obvious point: translating ordinary language into regimented form can aid clarity, but the regimented language is not magically protected from errant commitments. Determining our commitments is a task prior to regimentation. We can regiment the existence of unicorns as easily as that of horses.

QI makes exactly the mistake against which I am cautioning. Quine’s indispensability argument alleges that we must admit mathematical objects into our ontology since they are required for the regimentation of formal science. Quine’s implication that we are forced to quantify over mathematical objects is misleading. We have already added mathematical theorems to our best theory prior to regimentation. We do not merely examine the domain of quantification of the regimented theory and discover them there. Quine violates his own strictures against disinterpretation, by emphasizing the needs of regimentation over the content of the theory. “Structure is what matters to a theory, and not the choice of its objects.” (Quine (1981b) p 20)

We must disconnect theory, and its structure, from ontology. Formal theory is inappropriate for revealing commitments just because disinterpretation is not possible. We construct formal theory knowing the references of the terms of the theory, in order to get it to say what we want it to say. Our ontology is a constraint on regimentation, not a result of it.

§1.9: Incompleteness, and the Limits of Formal Theories

In the previous section, I argued generally against using regimented theories to discover our commitments. In this section, I discuss specific problems with the application of QP to the indispensability argument. I argue that the incompleteness of any formal theory sufficiently strong to encapsulate scientific theory makes that theory insufficient for revealing ontic commitments. It will omit relevant information. I first sketch a bit of the history of formal theories which led to Quine's adoption of QP. Quine's linking of regimentation and ontic commitment was an innovative move, independent from the motivations of those who initially developed those systems.

A regimented scientific theory will consist of a set of axioms within a deductive apparatus which guides inference syntactically. Regimentation makes as much as possible of a field of inquiry (e.g. logic, mathematics, or physics) syntactic. The earliest formalisms, like Euclid's axioms for geometry, were more casual about their language and deductive apparatus than are present-day formal systems. Using a formal system, one can assure the validity of an inference, and avoid worries about being misled by its semantic properties, by examining only its structure.

Aristotle's syllogisms are the prototype for separating syntactic questions from semantic ones, but he was concerned with clarity, not ontic commitments. The work on formal theories with the most historical relevance to QP started in the nineteenth century, when several problems in mathematics impelled mathematicians to seek greater clarity and foundations for their work. In geometry, the questions which had been percolating about Euclid's parallel postulate reached a head around mid-century with the work of Lobachevsky and Riemann.[14] Cantor's controversial work in set theory soon followed. While Cantor looked to foundations to defend the rigor of his work with transfinites, his set theory itself, which entailed the Burali-Forti paradox and relied on the faulty axiom of comprehension, impelled increased precision. Worries about foundational questions in mathematics had reached a tipping

[14] Euclid's parallel postulate states that if a line intersects two other lines and makes the interior angles on the same side less than two right angles, then the two lines meet on that side. The parallel postulate is equivalent to Playfair's Postulate, which states that given a line and a point not on that line, exactly one line can be drawn through the given point parallel to the given line. There are two ways to deny Playfair's postulate, or the parallel postulate, both of which are consistent with the other axioms of geometry. If one can draw no parallel lines, the geometry defines the surface of a sphere. If one can draw more than one parallel line, one defines a surface called a hyperbolic spheroid, or a pseudo-sphere.

point, and formal systems came to be seen as essential within mathematics proper. For about fifty years, from, say, Frege's *Begriffsschrift* (1879) to Gödel's Incompleteness Theorems (1931), formal systems were explored with the hope that foundational questions in mathematics could be answered.

Mathematicians were encouraged by the clarity of formal theories in mathematics, especially Peano's postulates for arithmetic (1889) and Hilbert's subsequent axiomatization of geometry (1902), if not their fruitfulness. The key work in non-Euclidean geometry was done prior to axiomatization. Similarly, set theory was not axiomatized until 1908, when Zermelo presented the first rigorous system, after Cantor's success with transfinites. (Dedekind had published a fragmentary development in 1888.)

Of course, there were existence questions on the minds of those who developed these formal systems, questions about the existence and plenitude of transfinites, for example. But the main worry was antinomy. Despite resistance due perhaps to worries about specific formulations of set theory, Cantor's achievements were compelling. Hilbert, for example, refused exile from Cantor's paradise, despite profound concerns to establish finitistic foundations for mathematics.

Though mathematical proofs had long existed, once the formal theories of the late nineteenth and early twentieth centuries were developed, the notion of proof became grounded. A proof in any discipline is a sequence of statements each of which is either an axiom, or follows from axioms using prescribed rules of inference. Other notions of proof were either reducible to this kind of proof, or dismissed as unacceptably informal.[15]

The main philosophical goal of formalizing mathematics was to explicate mathematical truth in terms of provability: Mathematical theorems are true just in case they are provable in a formal system with accepted axioms. In one direction, deriving truth from provability would ground mathematics with assurance that theorems are derived from accepted postulates. We would know that our theorems are

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[15] As an example of the latter, consider Wittgenstein's picture of commutativity (Wittgenstein (1991) p 233). The rotation of a grid of inscribed dots does not strike one as a proof, since it does not conform to this formal notion. See Brown (1999) for a defense of picture proofs in mathematics.

clean. In the other direction, the equation would delimit clear boundaries on the possible theorems of mathematics.

Frege, hoping to return to the "Old Euclidean standards of rigor," (Frege (1953) p 1) looked to formalize all deduction. Formal languages like Frege's were easily adaptable to include physical axioms. All of human knowledge, it could easily have been hoped, could be derived within a formal theory. Truth and provability could be aligned in all disciplines.

Russell's paradox for Frege's nascent set theory was the first sign of a problem for formalism. Frege did not abandon his formalist projects, though one might see the paradox as a reductio on the sufficiency of axiomatizations of set theory to capture our notion of set. After Gödel's incompleteness theorems, hopes for identifying truth with provability for sufficiently complex formal theories were dashed. Mathematical truth turned out to be provably distinct from mathematical proof within a single formal system.

The divergence of mathematical truth and proof is a remarkable philosophical achievement, and it extends beyond mathematics. In any discipline whose formalization is sufficiently strong to be of interest, we must sever truth from proof within a single formal system. Any formal theory which might serve as our best scientific theory is strong enough to be shown incomplete. The commitments of scientific theory can not be found in a formal theory for the same kinds of reasons which applied to Field's incomplete reformulation.[16] In particular, the indispensability argument, which relies on the construction of formal scientific theories, is invalid.

One might think that the inference from Gödel's incompleteness theorems to the invalidity of QI is too quick. For, QI needs only the sufficiency of proof for truth, and Gödel only showed that formal theories omit commitments, not that they generate false ones. But further problems arise from relying on formal theories to reveal ontic commitments. Even a complete theory, like the first-order theory of the

[16] For examples of omissions, see Burgess and Rosen (1998) §II.A.5.b.

reals, may not be categorical. A theory is categorical if all its models are isomorphic. Failure of categoricity entails that there will be non-standard models.

Just as Gödel’s theorem cleaves truth from provability in a single formal system, the Löwenheim-Skolem theorem shows that formal models of a sufficiently strong theory can be deviant and unintended and thus do not represent our true commitments. The availability of deviant models is commonly taken to demonstrate the indeterminacy of our commitments. This indeterminacy is merely a defect in the formal representation of our independently clear commitments. Once we release our hold on that dogma, indeterminacy becomes merely a defect in the formal representation of our commitments, which may be clear, independently. Regimentations will be useful only if we have a prior conception of what we want to say and of how to make the formal theory say it.

Even in mathematics proper, formal theories have limited appeal. Consider Paul Benacerraf’s argument that we can not choose between various adequate set-theoretic reductions of the numbers. Katz responds that we do have tools to select determinately the objects which appropriately model our number-theoretic axioms. Calling numbers communal property, among different fields which share interests in their diverse properties, he argues that no formal system can capture all we know about numbers.

> It is in the nature of formalization and theory construction to select those properties of the objects that have a role in the structure chosen for study. Moreover, selectiveness is essential in the formal sciences because numbers and the other objects they study are not the private property of any one discipline... The mathematician’s special interest in numbers is with their arithmetic structure; the philosopher’s is with their ontology and epistemology. From the standpoint of the inherent selectiveness of formalization and theory construction, the assumption of Benacerraf’s argument that we know nothing about the numbers except what is in number theory seems truly bizarre. (Katz (1998) p 111)

Typical axiomatizations of number theory provide no information about the abstractness of numbers, or how we come to know about them. We can formalize the notion of circle as the locus of all points equidistant from a given point, but unless the domain is strictly larger than the rationals, we do not even really get circles. Geometric axiomatizations provide no insight into the epistemology of points, or surfaces. Set theoretic axiomatizations give us no insight into the modality of sets.

The problems I have so far discussed may give the impression that the phenomenon at issue, difficulties in determining one’s existence claims on the basis of regimented theories, is isolatable within the philosophy of mathematics. The problem is broader.

Skolemite puzzles about models arise within formal systems. We can generate such questions by appeal to indeterminacy of translation, but support for that doctrine seems strongest on appeal to a metaphor from the problems which arise within formal systems. Hillary Putnam (1980) argues for a broad anti-realism by appealing to problems constructing formal models of any theory. Saul Kripke’s Plus/Quus example (Kripke (1982)) demonstrates difficulties for formalizing even clear and simple mathematical concepts. He, too, develops broader conclusions for our ability to know and follow rules in all areas. Without the problems from formal model theory, Kripke’s puzzle is merely skeptical. In general, the problems of unintended models are either skeptical or arise from unjustifiably artificial limitations on our abilities to determine those models. We can not even successfully formalize ‘7+5=12’. We should not seek answers to general metaphysical questions this way.

Field remarks that, “To say that one accepts an informal inductive argument that cannot be formalized in one’s theory is to say in effect that one accepts a stronger theory.” (Field (1984) p 110) My claim is that whatever theory one accepts for locating one’s commitments, it is not a formal theory at all. This leaves me in muddy waters which Quine’s procedure was thought to have cleaned up. Metaphysics is dirty work.

Mathematicians and philosophers of mathematics came to rely on formal theories because they insured the healthy deductions that motivated Aristotle and Euclid and Frege. If we know that our axioms are true, a difficult task, then we can be sure that the theorems which follow from them will also be true. We may maintain formal theories for generating results within any field in which they might be useful. We also must acknowledge that the purposes of formal theoretic construction do not include answering all metaphysical questions. Formal theories are generally indifferent to philosophically interesting properties, like abstractness. And, “There is no mathematical substitute for philosophy.” (Kripke (1976) p 416)

§1.10: Devitt and the Metaphysical Horse

In this section, I draw an analogy between my concerns about reliance on formal theories to reveal our commitments and Devitt’s caution against allowing one’s semantics to lead one’s metaphysics. Our reliance on formal theories violates the spirit of Devitt’s counsel.

The most objectionable way of emphasizing semantics over metaphysics is to make one’s existential commitments subservient to a controversial semantic theory. This is Devitt’s concern, and he argues that it puts the semantic cart before the metaphysical horse. A different way to emphasize semantics over metaphysics is to turn one’s metaphysics into a semantic project. This is the error implicit in QP which forces us to construct a formal theory, and then model that theory, in order to discover our commitments.

Devitt presents several maxims which guide his metaphysical realism, including:

> Maxim 2: Distinguish the metaphysical (ontological) issue of realism from any semantic issue.
> Maxim 3: Settle the realism issue before any epistemic or semantic issue. (Devitt (1991) pp 3-4)

Devitt argues for the primacy of our metaphysical conclusions, since we are clearer and more confident about these. Semantics, on which we are much less clear, should conform to our established metaphysical picture. By making metaphysics into a project of theory construction, QP sullies metaphysics with considerations of formal theory building and modeling.

Devitt’s worry about leading with one’s semantic theory arises in the context of his defense of metaphysical realism about common-sense and scientific physical objects. The point is more broadly applicable. We always construct formal theory, in mathematics and in science in general, according to prior concerns about ontic commitment, and according to concerns about the form and structure of the theory. The latter concerns may or may not have ontic import.

Devitt's maxims themselves leave open the question of how one is supposed to determine one's commitments. There is no simple answer to this question. We look to scientific theory, and to common sense. We try to resolve tensions among various intuitions and balance formal science with casual observation. We recognize that the construction of scientific theory is ongoing, and that our currently best theory may be radically wrong or incomplete. Our currently best neuroscience and our currently best psychology radically under-explain mental phenomena. It would be silly to think that what exists is what these best theories say exists.

One might wonder why we should accept Devitt's maxims. Consider that there's an open question in the philosophy of mathematics about whether the debate over realism in mathematics should be taken as concerning the existence of objects or as concerning the objectivity of mathematical statements.[17] If the debate concerns objects, then it is clearly metaphysical. If the debate concerns the objectivity of mathematical claims, we focus on how to interpret the truth of mathematical claims, a semantic project. We may best account for objectivity by reference to a broader semantic theory. If we prioritize ontic issues over semantic ones, we resolve this debate by fiat. The objectivist position deflects our interpretation of mathematics to our theories of truth.[18] If we have to settle the ontological issues before the semantic ones, then the objects/objectivity question is a non-starter; the debate must be over whether there are mathematical objects.

There may be cases in which semantics has some role in determining ontic commitments. Some

[17] See Field (1998a) for the latter, and Field (1988) for the former.

[18] Devitt wonders, in communication, whether we can see the debate over objectivity as metaphysical, involving what he calls the independence claims of metaphysics, rather than existence claims. But those who favor objectivity over objects may do so precisely because of the benefits of assimilating mathematics into a broader semantic theory. They hope to solve the epistemic problems for mathematics by making the debate concern formal theory.

dispensabilists argue that reinterpreting mathematical existence sentences as referring to mental objects or inscriptions, or as sentences of modal logic which do not refer to mathematical objects avoids commitments to mathematical objects.[19] Those who favor reinterpreting mathematics say that the semantic is tied to the ontological, that realism is exactly a doctrine about truths of statements. For example, a structuralist who thought that his position helped solve the problems about access to abstract objects would be obviously wrong. Benacerraf (1965) seems to defend this position. If we extend Devitt's claim into an insistence that we settle all metaphysical claims before semantic ones, we can rule out these reinterpretations. Similarly, the best way to deal with posits like fields and space-time points may be via the semantics of sentences which refer to them rather than via the existence of them as objects.

Devitt holds Maxim 2 and Maxim 3 because he thinks we are clearer about metaphysics than we are about semantics. "The metaphysical issue of realism is the fundamental one in our theory of the largely impersonal world. Semantic issues arise only in our theory of people (and their like) in their relations to that world." (Devitt (1984) p 3) This may be true about the physical world, though if all of science is reducible to strings on Planckian scales, or some other fundamental particles, then I am committed to them, and I am not clear about them at all.

It is worthwhile to consider a broader explanation of why metaphysics should not generally follow semantics. This is where I think the analogy between Devitt's maxims and my argument against Quine's reliance on formal theory for revealing ontic commitments is particularly edifying. Formal methods for determining ontic commitment like QP distract our gaze from reality, where it belongs, to the linguistic shadows on the walls of the cave.

§1.11: Conclusions Opposing QI

In §1.2 - §1.3, I argued that Quine's homogeny, which arises from QI.1 and QI.2, is unfounded.

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[19] For example, the modal dispensabilists I discussed in Chapter 1, §1.6.

It relies on his insistence on a uniform epistemology and confirmation holism. Holism misrepresents the actual workings of science and its relation to mathematics.

In §1.4 - §1.7, I presented concerns about Quine’s procedure for determining the ontic commitments of a theory. He defends the simplicity of first-order logic, but it is hard to see exactly how first order logic is simple, beyond unifying reference. In some ways, natural language is much simpler. Even if one could establish that first-order logic were the simplest language available, the connection between simplicity and truth needs to be made. “It is hard enough to believe that the natural world is so nicely arranged that what is simplest, etc. by *our* lights is always the same as what is *true*...; why should one believe that the universe of sets... is so nicely arranged that there is a preestablished harmony between *our* feelings of simplicity, etc., and *truth*?” (Benacerraf and Putnam (1983) p 35)

Quine’s choice of first-order language is insufficient to establish that this language is the only one in which we can express ontic commitment. Since there are other languages in which we can easily express ontic commitment, the quick inference to the existence of mathematical objects from the need to quantify over them in our best, first-order-regimented theory does not follow.

The considerations of §1.8 - §1.10 undermine Quine’s application of QP to QI. QI can not work, for its metaphysical conclusion arises from the construction of formal theory. We are free to construct and interpret our formal theories as we wish. We can adopt semantic ascent for clarification without also turning to regimentation as the source of commitment. Ontology need not recapitulate philology.

Quine does notice that we regiment only when useful. “A maxim of shallow analysis prevails: expose no more logical structure than seems useful for the deduction or other inquiry at hand... [W]here it doesn’t itch don’t scratch.” (Quine (1960a) p 160) But he uses this maxim as merely a practical guide. We eschew full regimentation only because we can envision what it would look like, and what its yield would be. If we have ontic questions, for Quine, we have to look at the fully formal framework.

Despite the independence of philosophical issues and formal systems, we do construct formal systems with an eye to our commitments. We investigate those things we believe to exist and we do not, generally, regiment fiction. Our commitments arise prior to regimentation, just as Devitt argued that metaphysics is prior to semantics. Reasoning within a formal system can, theoretically, affect our independent beliefs about what exists. It may turn out that mathematicians discover new theorems by working within a formal theory, though mathematical reasoning does not generally work this way. Regimentations are instead used as a check on fallible, informal reasoning. The benefits of mathematical regimentation may translate to the mathematized portions of science, but it is unlikely that writing science in a formal, canonical language would lead to any scientific advances.

Quine made metaphysics acceptable, in the aftermath of logical positivism, but at the cost of reducing it to a byproduct of formal theoretic construction. He resurrected the discipline, despite the problems with his methods. It is as if philosophy went back to its infancy with the positivist program, and had to start all over again. Rejecting QP brings us back to philosophy. We must return to the good old days of the worst kind of speculation.

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