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THE FRUITS OF LOGICISM

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Abstract A few remarks concerning the history of logicism in the twentieth-century and the prospects for neologicism in the twenty-first.

1. Introduction

In these remarks, I'll discuss a few of the underlying themes which have come up in this and the previous issue of the *Journal*. I won't try to talk about *all* of the papers in these issues, nor will I attempt to adjudicate any of the extra-curricular discussions which took place at the conference from which these papers are drawn. Instead, I'll limit myself to two simple points: one concerning logicism in the twentieth century and one concerning neologicism here at the start of the twenty-first.

2. Logicism

Let's begin with logicism. If you want to show that mathematics reduces to logic—or, at least, that *some parts* of mathematics reduce to *something like* logic—then there are four things you need to do.

1. You need to specify the conception of "logic" that you're working with.
2. You need to specify your conception of "reduction."
3. You need engage in the technical project of showing that some interesting parts of mathematics can be reduced to logic (in, of course, the senses of "logic" and "reduction" specified in (1) and (2) above).
4. You need a whole lot of philosophical argument to explain why the rest of us should *care* about the project laid out in (1) – (3).

With respect to this last point, for instance, you might argue that the principles of logic laid down in (1) have some special epistemological status—perhaps they're a priori or have an unusually high degree of certainty. Then, if you could show that this status is preserved through reductions of the type specified in (2), you would know something important about the epistemological status of mathematics. Similarly, if you could

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show that reductions of the type specified in (2) can be plausibly characterized in terms of definitions—and if your account of logic in (1) is relatively uncontroversial—then you might be able to make a case for the *analyticity* of mathematical truths. Whatever the particular details here, it is hopes like these which have motivated much of the philosophical work on logicism over the course of the last century.

Now, although these philosophical hopes and motivations are certainly important, I want to put them aside for a moment and look just at (1)–(3). (I'll come back to (4) in a little while.) Logicism started with a specific answer as to how (1)–(3) would proceed. Frege gave us a particular conception of logic (and, indeed, a particular *formalization* of logic), he gave us a particular conception of reduction, and he put us well on the way toward showing just how mathematics could be reduced to logic (under, again, the relevant conceptions of "logic" and "reduction").

Of course, Frege's project wound up floundering on the Russell paradox. And the verdict is still out—to say the least—on the success of later incarnations of logicism. There's still controversy about the status of Russell's type theory, and neo-Fregean approaches to logicism are too new and too underdeveloped to assess with any degree of thoroughness. But, if we step back from these *particular formulations* of logicism to examine the range of mathematics which has arisen from the *general idea* of logicism, then I think we get a fairly favorable assessment of logicism in the twentieth century.

We can start with proof theory. Frege gave a specific formalization of the notion of "proof." After the collapse of Frege's project, this formalization was extended and enriched by others—Russell, Hilbert, Gödel, Gentzen, and so on. The end result was the discipline of proof theory—a discipline which, over the course of the last century, has matured and developed into an exciting and rich field of mathematical investigation in its own right. I need only mention recent work on ordinal analysis, predicative analysis, and reverse mathematics to indicate the depths of current work in this area.

Moving on, we note that people working on the notion of proof—and especially the notion of *finitary proof*—found that they needed an account of computation, an account of what it takes for one result to be computable in terms of another result. In time, Kleene, Turing, and Church formulated the notion of a *recursive function* and used it to develop this account. The end result of their efforts is the discipline of modern recursion theory, a discipline which has uncovered a rich and beautiful structure in the lattice of r.e. sets, a discipline which has made important contributions to modern set theory (e.g., fine-structure theory), and a discipline which has only recently begun to develop deep interactions with central areas of "normal" mathematics (e.g., differential geometry).

Turn next to set theory. Although Cantor's original formulation of set theory had little to do with logicist projects, I think it is fairly clear that the *development* of set theory in the twentieth century had a fair bit to do with the quest for mathematical foundations which Frege originated. Certainly this was the "spin" which figures like Zermelo (and in some moods Gödel) gave to set theory, and it was a common enough understanding of set theory that figures like Poincaré and Skolem felt the need to object to it. And, whatever its origins, set theory has *come to play* a foundational role: it now comprises *the*—so widely accepted that it's nearly invisible—framework in which the rest of mathematics is given its canonical formulation. I think it is not unreasonable, therefore, to view contemporary set theory—complete with its array of large cardinal axioms, its elaborate forcing constructions, its fine-grained analyses of inner models, and its subtle combinatorial results—as a genuine descendent of Frege's logicism.

Finally, turn to model theory. Model theory started when people like Hilbert and Tarski needed to make sense of notions like *consistency* and *independence*. Eventually, they hit upon the notion of a *model* or an *interpretation*, and they used this notion to clarify the more problematic notions with which they began. In time, a discipline which started with logicist concerns about the independence and consistency of particular axiom systems (e.g., the axioms for arithmetic, geometry, and set theory) developed into something far richer and more mathematically central. Here, I need only mention Shelah's "main gap" theorem on the classification of first-order structures, the recent use of *o*-minimality to prove Whitney-style stratification results for certain "nice" manifolds, and Hrushovski's proofs of the Mordell-Lang conjecture and Manin-Mumford conjecture in algebraic geometry.

My point, then, is this. If we think of logicism specifically in terms of Frege's original project, then it's pretty clear that logicism failed. If we think of logicism in terms of later neologicist projects, then it's at best unclear whether logicism succeeded (or will ever succeed). But, if we think of logicism as a mathematical research program—one which tries to flesh out the idea that there are deep connections between divergent areas of mathematics, and that these connections are best uncovered and explored using specifically *logical* techniques and machinery—then I think it's clear that logicism has been a splendid success. If any of the major unification programs that are alive here at the beginning of the twenty-first century—for example, the Langland's program—bear the kinds of fruits which logicism bore in the twentieth century, then they will be considered wildly successful. In my view, logicism should be regarded from a similar perspective.

Of course, all of this focuses on the purely mathematical side of logicism (suggesting, in effect, that a deliberate fuzziness in our approach to issues (1) and (2) might be mathematically productive when we turn to issue (3)). What, then, about philosophy? In my own view, the relationship between logicism and philosophy is pretty similar to that between logicism and mathematics. Even if logicism hasn't given us an airtight account of, say, the analyticity or a prioricity of mathematics, logicist projects have occasioned an enormous amount of good philosophy. Consider, for instance, the deep philosophical reflections which have been inspired by Hilbert's program and/or Gödel's theorems. Consider work on Skolem's paradox and on the model-theoretic analysis of logical consequence. Consider recent programs for explaining how we can come to believe large cardinal axioms and/or take reasonable stands concerning the size of the continuum.

My point should, once again, be obvious. The research program which Frege inspired has generated just as much good philosophy as it has good mathematics. Once we "fuzz up" our understanding of issues (1) and (2), we find that this fuzziness opens up a collection of philosophical questions and issues that is just as large and as rich as the collection of mathematical questions and issues discussed earlier. Thus, if we can get past our insistence on viewing logicism as a specific *thesis*—or as something which could and should be sharpened up into a thesis—and start to view logicism as a fairly broad philosophical/mathematical research program, then I think we find that this program has been at least as fruitful from the philosophical standpoint as it has from the mathematical. In my view, this is the right way to view logicism in the twentieth century.

3. Neologicism

So much, then, for the twentieth century. Let's turn to neologicism of the kind with which many of the papers in this and the previous issue have been concerned. The key idea here is the idea of an *abstraction principle*, a principle of the form

$$\forall a \forall b \, [\Sigma(a) = \Sigma(b) \iff a \sim b],$$

where a and b are higher-order variables, Σ is a "type-lowering" function taking higher-order objects to lower-order objects, and $\sim$ is an equivalence relation. The canonical example of such an abstraction principle would be Hume's Principle, a principle which plays a central role in the neo-Fregean account of number and which has done much to inspire the form of neologicism here under discussion.

Now, just as logicism did, neologicism has spawned a whole collection of interesting technical and philosophical questions. Can we find abstraction principles which allow us to generate classical analysis? How about set theory? What properties make an abstraction principle good? (Clearly not every abstraction principle is good, since some are outright contradictory.) Are abstraction principles plausibly regarded as (contextual) definitions? Are they a priori? Are they analytic? Etc.

I think, however, that in our rush to answer these particular questions—questions which are motivated by the project of resuscitating logicism along broadly Fregean lines—we miss something important. In the context of Shapiro's abstractive generation of $\mathbb{R}$ [1], the following question arises: Can we use abstraction principles to *directly* generate the algebraic closure of the integers? Of course, we know that we can use abstraction principles to *get* this closure: we simply generate, in sequence, $\mathbb{Q}$, $\mathbb{R}$, and $\mathbb{C}$ and then define algebraic closure within $\mathbb{C}$. However, this approach involves a huge (cardinality $2^{\aleph_0}$!) detour; it would be nice to know whether we could generate this closure more directly.

The key point here is that answering this question about algebraic closures won't help very much with the neo-Fregean project (for that project, we have to generate $\mathbb{C}$ anyway and once we have $\mathbb{C}$, we know how to get our closures). Nonetheless, it's an interesting technical question. Further, once we focus on questions like this—questions about abstraction principles which don't connect to the project of reconstructing Frege's work—we find that such questions are fairly plentiful. Suppose, for instance, that we start with $\mathbb{R}$. Can we use abstraction principles to generate infinitesimals? If so, can we get an interesting version of nonstandard analysis which is based, at least loosely, on abstraction?

Similarly, suppose we have a topological space. Can we use abstraction principles to generate its homotopy group? What about other topological invariants? Suppose we start with a series of field extensions; can we use abstraction to generate the associated sequence of Galois groups? How much of category theory can be recovered in abstraction-theoretic terms? Again, in all of these cases we are interested in what abstraction can give us *directly*; simply using abstraction principles to generate some variety of set theory and then "defining down" to get the particular mathematical objects we are interested in is substantially less interesting.

It seems to me, then, that there is a great deal of good work to be done in what might be called "abstractive mathematics." The questions for such mathematics concern the kinds of objects which can be defined using abstraction principles, along with the things that can be proved about these objects on the basis of (some suitable axiomatization of) abstraction. In formulating these questions, we need not limit ourselves to foundational perspectives. It doesn't matter how we *get* our real numbers, topological spaces, or field extensions; what matters is what we can generate once we *have* such objects.[1] Still less need we limit ourselves to following Frege. Frege almost certainly wouldn't have been interested in infinitesimals; we, almost equally certainly, should be. There's simply no reason to take a potentially exciting collection of mathematical questions and limit our investigation of these questions to some preordained (and almost purely philosophically motivated) line of inquiry.

And what of philosophy? As I indicated earlier, neologicism has generated a whole raft of philosophical questions concerning the status of abstraction principles. Some of these questions are rather general: Can abstraction principles ever be a priori? Can they ever be analytic? Some are more specific: What, from a philosophical perspective, is the best way of generating real numbers? (Incidentally, I think that Wright's paper [2] provides a remarkably illuminating analysis of some of these issues.) What most characterizes these philosophical questions, however, is an extreme level of difficulty and, for some of us at least, an attendant level of frustration.

The philosophical difficulties here have, I think, two sources. First, we're not entirely sure what kinds of properties we *want* abstraction principles to have. Frege believed that mathematics had *lots* of nice properties—a prioricity, analyticity, certainty, necessity, and so on—and he thought that these properties fit together into a fairly nice package. Since Frege—and particularly since Kripke—we've become more skeptical of such packages. Not everything that's necessary is certain; not everything that's a priori is necessary. Further, we've come to distinguish different species of, for example, necessity and a prioricity, and we don't expect these species to live together.

Unfortunately, although we are quite able to make all these distinctions in theory, it's harder to keep a grip on them when we try to evaluate particular (abstractive) principles. This is especially true when we follow a guide who didn't make these distinctions as sharply as we do. In consequence, we tend to get ourselves into fruitless dilemmas. This abstraction principle isn't certain; that abstraction principle isn't a priori; which one should we choose? Until we can decide what properties we want our mathematics to have—and *genuinely* stop expecting all the nice properties to go together—it's hard to see how we will make progress on the philosophical side of neologicism.

That, then, is one difficulty with neologicism. There's a second, and in my view more serious, philosophical difficulty here: we don't understand abstraction principles well enough to approach the philosophical issues surrounding neologicism in a technically informed manner. A few years ago, we didn't even know how to use abstraction principles to generate real analysis (although that problem has now been cleared up in several different ways). We still don't know how to use abstraction principles to generate anything like full, classical set theory. A few moments ago, I sketched a number of simple—and relatively obvious—technical questions which we don't yet know how to approach. In the light of these purely technical shortcomings, then, I'm led to wonder whether we're really in a position to answer—or even to *consider*—the more philosophically oriented questions raised by neologicism.

An analogy might be helpful here (and, then again, it might not). It seems to me that philosophical discussions of neologicism here at the beginning of the twenty-first century are in much the same position that philosophical discussions of set theory were in at the beginning of the twentieth. At that point, we knew that naïve set theory generated paradoxes, but we didn't have a standard analysis as to what caused these paradoxes or an agreed upon method of avoiding them. (And just which abstraction principles are free from contradictions anyway?) Nor did we have any widely accepted *axiomatization* of set theory. (Do we have *any* axiomatizations of abstraction?)

Leaving paradoxes and axiomatization aside, at the turn of the twentieth century we didn't really know what kinds of theorems set theory could (and couldn't) prove; indeed, we didn't even know how to define notions like *function* or *ordered pair*. (*Can* we use abstraction to get infinitesimals or homotopy groups?) Nor did we have detailed conceptual analyses of particular approaches to set theory—the iterative conception of sets, the type-theoretic conception of sets, and so on. (Do we now have a good analysis of what *makes* something an abstraction principle? Do we have interesting *alternative* analyses?)

My point, once again, is a simple one. At the beginning of the twentieth century, we didn't know enough about set theory to engage in productive philosophical analysis of this discipline. It was only after we had spent a while *doing* set theory that we found ourselves in a position to think *philosophically* about it (e.g., in the manner exemplified by Gödel, Boolos, Maddy, Steel, Wooden, Martin, and so on). In my view, we are in the same position with regard to neologicism. We need to spend a lot more time *doing* neologicist mathematics—that is, playing around with abstraction principles—before we can productively evaluate the philosophical significance of this mathematics. Similarly, we need to focus on more local and more conceptual questions about abstraction—What makes something an "abstraction principle"? What makes some abstraction principles productive and others contradictory?—before we can address our broader questions about the a prioricity, analyticity, or logicality of abstractive mathematics.

There is, then, a unifying moral to these remarks. There are a lot of good technical questions concerning abstraction which haven't yet been answered (or even really asked). We would do well to focus more of our attention on these questions. For one thing, such focus is liable to lead to a good bit of interesting and valuable mathematics. For another, focus on these questions may well be a prerequisite to making progress on the more philosophical questions which we all really care about. As with any kind of philosophy of mathematics, our mathematics needs to be fairly well developed before our philosophy has anything substantial to talk about. I suggest, therefore, that we set about developing the *mathematics* behind neologicism before we worry too much more about the philosophy. Only thus, I think, can we hope to build neologicism in the twenty-first century into the kind of broad, rich, and philosophically productive research program which logicism itself became in the twentieth.

Note

1. This is not to say that it's uninteresting to try to generate everything from scratch; it's very interesting indeed! It *is* to say that we need not wait until we've generated topological spaces to investigate the abstractive generation of, for example, homology groups. It's also to say that the generation of such groups will be an interesting piece of abstractive mathematics *even if* the relevant topological spaces turn out to resist abstractive definition.

References

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