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A logico-philosophical tour: *The Search for Certainty*, by Marcus Giaquinto

At last! Here is a book that will be warmly welcomed by any philosopher or philosophy student interested in the foundations of mathematics. *The Search for Certainty* tells the familiar story of the crisis sparked by the class paradoxes, and the subsequent attempts to restore certainty to mathematics. It's a fascinating story, of course – its subject matter is "one of the most brilliant intellectual explorations ever", as the author says in his preface. But *The Search for Certainty* tells the story in a new way. It offers a *philosophical* examination of the developments in twentieth-century mathematical logic. Unlike a standard logic text, it takes the time to explain crucial mathematical and logical concepts, explore the philosophical significance of individual theorems, and adjudicate between competing approaches to the paradoxes and the foundations of mathematics. The author writes: "Despite the importance and allure of the subject, there has been no synoptic philosophical account of it, as far as I am aware – something I missed when first approaching the subject as a student of mathematical logic. This book is intended to fill that gap." The gap was certainly there; *The Search for Certainty* has filled it admirably.

As Giaquinto says, he has not aimed to produce a text in mathematical logic or a history – his main purpose is to engage with philosophical ideas and arguments. But it's worth emphasizing that *The Search for Certainty* (*SC*) does not skimp on formal details or rigour. Key definitions are provided in the main body of the text, along with sketches of proofs, sometimes quite detailed. Optional endnotes and appendices add more details for the interested reader; and where definitions or proofs go beyond the scope of the book, references are supplied. So readers can find their own level of technical comfort. Those less formally inclined will be able to follow the philosophical threads through the main text, which is clear, thorough and accessible throughout. Some may venture into the endnotes and appendices. And others may on occasion feel encouraged to refer to a standard text – but even then they will find that *SC* still serves as a valuable intuitive guide and philosophical commentary. As to history: the book may not be a history, but it provides a wealth of historical detail and references. Underlying motivations, earlier influences on later work, the ongoing dialectic between major figures in the field – all this is traced out with care and accuracy.

Part 1 sets the scene. (The book is divided into 6 parts, each composed of several short chapters.)[1] Earlier puzzles – which arose well before the discovery of the class paradoxes – had already influenced the direction of mathematics. Puzzles about the infinite, together with the limitations of spatial intuitions, led to the clarification of mathematical analysis. Analysis was reconstructed in arithmetical and logical terms: basic notions such as the *limit of an infinite sequence*, *convergence* and *continuity* were defined in terms of the notions of *real number*, *classes of points*, *functions on classes of points*, and the universal and existential quantifiers. But in turn these arithmetical notions stood in need of clarification. *SC* presents two standard definitions of the reals (one in terms of sequences of rationals, and the other in terms of Dedekind cuts), and then moves on to Cantor's point classes, and the ordinal and cardinal numbers. Part 1 closes with two different accounts of the natural numbers, due to Dedekind and Frege. In the context of the book, these chapters merely prepare the ground. But I think they have value in their own right, given their accessibility to the non-technician. I wish these chapters had been available for philosophy students of mine – undergraduate and graduate – who were working on Zeno's paradoxes, or time, or the notion of infinity. They would have found a clear, patient and rigorous presentation of the technical material they needed for their philosophical projects.

The nineteenth century closed on a promising note:

> "Towards the end of the nineteenth century, the drive for clarity and rigour seemed to be reaching a successful conclusion. Among its fruits were precise accounts of the real and natural numbers, the first general theory of transfinite classes and numbers, and a first account of quantifier logic – no meagre harvest." (p.34)

But the treatments of the various number systems was based on the general theory of classes. With the discovery of the class paradoxes, the foundations of mathematics became a major area of research, driven by a philosophical concern: "… how can we be certain that the theorems of mathematics are trustworthy?" (p34). It is this challenge that occupies Giaquinto for the rest of the book.

Part II presents the class paradoxes – discovered by Burali-Forti, Cantor and Russell – and the early attempts to resolve them. In response to the Burali-Forti paradox, Cantor distinguished between sets and "absolutely infinite multiplicities". The latter are the precursors of von Neumann's proper classes; according to Cantor, an absolutely infinite multiplicity cannot be thought of as a unity or as "one finished thing" (according to von Neumann, proper classes are "too big" to be sets). Burali-Forti's paradox arises if we suppose there is a set of all ordinals, but the contradiction is avoided if we take the ordinals to form instead an absolutely infinite multiplicity. As Giaquinto remarks, Cantor's way of avoiding the contradiction does not amount to a *solution* to the paradox – it is clearly *ad hoc*, and the notion of an absolutely infinite multiplicity is imprecise. Nevertheless, in the course of *SC*, Giaquinto brings out several positive features of Cantor's response, not least its anticipation of the Axiom of Replacement (see pp.124-5).

Giaquinto turns next to Frege's logicist programme. Frege sought to demonstrate the trustworthiness of the theorems of mathematics by explicitly deducing them from logical laws and definitions. Noting that for Frege logic included a theory of concept extensions, Giaquinto properly emphasizes the *predicative* nature of extensions. In the literature, Frege's notion of extension is sometimes too quickly assimilated to the modern notion of set, as if Frege was presenting a kind of set theory. Modern set theory embodies the combinatorial-iterative conception of set, according to which we combine disparate individuals to form sets, and then combine these sets to form further sets, and so on endlessly. Frege was quite opposed to this conception[2]. Unlike mere assemblages of individuals and sets, Frege's extensions were directly tied to logic: they are the extension of concepts, concepts are the referents of predicates, and predicates figure in quantificational laws of logic.

As Frege saw it, the fundamental fact about concept extensions is this: the extensions of two concepts are identical if and only if whatever falls under one concept falls under the other. This fact was embodied in Frege's ill-fated Basic Law V. Giaquinto shows how Russell's paradox arises in Frege's system: from Frege's axioms, we can derive the Comprehension Principle, that every predicate has an extension – and from this principle a contradiction follows. (A derivation of the Comprehension Principle from Frege's premisses is provided in an appendix.) In a concise discussion of Frege's response, Giaquinto surveys the diagnoses Frege considered and rejected, as well as the one Frege tentatively proposed, which was later shown to lead to contradiction itself. (It is perhaps surprising that Quine 1955 and Geach 1956 – loci classici of this refutation of Frege's way out – are not cited .)

In the final chapter of Part II, Giaquinto considers the type-theoretical response to the class paradoxes. Under consideration here is the simple theory of types, not the richer system of *Principia Mathematica* (which is the subject of Part III). This is a sensible organizational move, given the forbidding complexity of Russell's system in *PM*. How might simple type theory fare better than Cantor's or Frege's way out? At this point, Giaquinto provides significant constraints on any logicist approach to the paradoxes (p.58). If mathematics is really logic, and logic is to include class theory, then classes cannot be special mathematical objects and the theory of classes cannot be restricted: "Classes must be nothing but concept-extensions and the theory of classes must apply to all of them" (p58). It follows that the logicist cannot take Cantor's way out, otherwise there will be classes (such as the class of all ordinals) that are outside the scope of the theory. And the logicist cannot follow Frege in restricting the Comprehension schema. Give up the Comprehension schema and you must give up the logical equivalence of 'F(x)' and 'x is in the extension of F' - and then you no longer have a basis for taking extensions to be logical objects, and statements about extensions to be topic-neutral. In contrast, Giaquinto argues, simple type theory can respect Comprehension, endorse a fully general theory of concept-extensions, and still avoid contradiction. Rather than admitting concepts or conditions that have no extension, simple type theory places restrictions on what conditions there are in the first place: every condition is true or false only of things of a single given type, and there is no general condition of being a non-self-membered class, or an ordinal, or a class. No need, then, to countenance classes outside the scope of the theory: to each condition, there corresponds a class. So the logicist constraints are met. And, as Giaquinto carefully shows, the class paradoxes are blocked.

That is not to say, however, that simple type theory provides a *solution* to the class paradoxes. (It is a virtue of *SC* that the distinction between blocking a paradox and solving it is given its due weight.) There are a number of well-known reasons why simple type theory cannot be counted as a solution, and Giaquinto summarizes them crisply: it is too strict, its ban on mixed classes is unreasonable, and, by its own lights, some of its principles cannot count as meaningful propositions (as Godel pointed out).

Though the simple theory of types blocks the class paradoxes, it does not address another family of paradoxes – the so-called definability paradoxes. In *Principia Mathematica* (*PM*), Russell sought a unified solution to the class paradoxes, the definability paradoxes and the Liar paradox about truth (which Giaquinto discusses later in a brief chapter). The result was the ramified theory of types, and Part III of *SC* presents a remarkably clear account of Russell's theory. Giaquinto starts out with the definability paradoxes due to Konig, Berry and Richard, and explains explicitly why the simple theory of types does not prevent them. Some broader approach is needed, and for Russell the guiding idea is that all the paradoxes violated the *Vicious Circle Principle* (*VCP)*. Russell stated the VCP in several seemingly non-equivalent ways, none of which are very clear; the rough idea is that no collection has members that are definable only in terms of that collection. In an excellent chapter devoted to the VCP, Giaquinto carefully works his way to a clear statement of the VCP, disentangles it from closely related but distinct principles, and provides a subtle assessment of its bearing on the paradoxes. His conclusions are largely negative: while the VCP blocks the paradoxes, it is not the key to a solution. For example, consider a version of the Berry paradox generated by the phrase "the least integer not definable in fewer than eleven words". This phrase violates the VCP merely in virtue of the construction "the least integer" - but surely the root of the paradox is not located there, but rather with the notion of *definability*. And Giaquinto argues persuasively that the VCP, while it may have some plausibility for intensional entities such as attributes, is not plausible for classes. A class depends on its members, but it need not depend on the things that its specifications depend on. For example, Giaquinto asks us to consider the class of all stars beyond the light cones of every sentient being; this class cannot be specified independently of sentient beings, yet it depends only on certain stars. (For a mathematical example, Giaquinto considers the Cantor set, a class of numbers between 0 and 1 which, however, can be specified only via a class of infinitely many integers.) Giaquinto concludes: "So the fact that a class cannot depend on itself is no ground for thinking that the specifications of a class cannot depend on that very class. … Classes are extensional, and the Vicious Circle Principle is plausible only for intensional entities." (p.83)

Next, the reader is taken through the complicated world of *Principia Mathematica*: Russell's 'no-class' view, the theory of orders, the Axiom of Reducibility, the Multiplicative Axiom, and the Axiom of Infinity. The presentation throughout is clear and direct, and sensitive to the source texts. The reader unfamiliar with the specifics of PM should come away with a good understanding of the difference between orders and types, the ban on impredicative definitions imposed by orders, the need for the controversial Axiom of Reducibility, and the difficulty Russell faced in justifying the axioms. Giaquinto has some interesting things to say about Russell's inductive justification of axioms, according to which one starts with the near certain parts of basic mathematics and shows that they are derivable from the given set of axioms, and from no other known set of equally or more plausible axioms. (In turn, the more controversial parts of mathematics are justified if they are derivable from the axioms.) Giaquinto argues that the Axiom of Reducibility cannot be justified in this way, since it receives little or no support from the basic parts of mathematics. Reducibility is not needed for the derivation of the ‘nearly indubitable’ theorems of integer arithmetic; it is needed in the theory of real numbers, but by then we have left behind the near certain parts of mathematics. The Multiplicative Axiom and the Axiom of Infinity have their own problems. The Multiplicative Axiom – or the Axiom of Choice – is highly intuitive for classes: given any class of disjoint non-empty classes, there is a class containing just one member from each class. But Russell works with propositional functions, not classes – and why suppose that the axiom holds for propositional functions? Suppose we have a (possibly infinite) plurality of propositional functions ψ_1, ψ_2, … ψ_k …, each true of at least one thing, but with no two of them true of the same thing; why suppose there is a propositional function true of exactly one thing that ψ_1 is true of, exactly one thing that ψ_2 is true of, and so on for each ψ_k, and true of nothing else? As for the Axiom of Infinity, Russell himself thought there was no logical justification for it (“It seems plain that there is nothing in logic to necessitate its truth or falsehood”, quoted p.96 of *SC*), and Giaquinto conjectures that Russell did not try to justify it inductively because it goes beyond the near certainty of small number arithmetic. Giaquinto points out that the lack of justification for the Axiom of Infinity is particularly embarrassing, since it is indispensable for the theory of natural numbers.

In the face of these and other difficulties for the ramified theory, Ramsey proposed that the simple theory of types be the foundational system for mathematics. But what then of the definability paradoxes, which cannot be resolved by types alone? Ramsey famously distinguished two groups of paradox: the ‘logical’ paradoxes (Burali-Forti’s, Cantor’s, Russell’s), and the ‘linguistic’ paradoxes (the definability paradoxes and the Liar). Ramsey’s strategy, endorsed by Giaquinto (see p.106), is essentially this: side-step the linguistic paradoxes altogether. Don’t let them generate a contradiction, of course; but at the same time, don’t expend energy in trying to *solve* them. For example, it would be enough to simply exclude the predicate ‘defines’ from the foundational system (as Giaquinto suggests, *ibid.*). In the context of Russell’s search for a unified resolution of the paradoxes, this is a radical move, and it is not sufficiently emphasized in *SC*. We can discern some tension in *SC* here, I think: on the one hand, much attention is given to the logical *and* the linguistic paradoxes, and the attempts to solve them; on the other, it turns out that the linguistic paradoxes may be summarily dismissed as irrelevant to the foundation of mathematics.

Giaquinto argues convincingly that Ramsey’s attempt to rescue logicism fails. Ramsey abandoned orders, and admitted impredicative definitions that violate the VCP. An acceptable move for a domain of classes, Giaquinto suggests, since the VCP is not true of extensional entities. And a domain of classes seems appropriate for mathematics, where classes are not tied to propositional functions or conditions. Ramsey wrote: “The possibility of indefinable classes and relations in extension is an essential part of the extensional attitude of modern mathematics” (quoted in *SC*, p.108). But for the logicist, classes must be regarded as extensions of concepts or propositional functions. “Thus, Ramsey exposed an apparent incoherence at the heart of logicism.” (*ibid*.). Ramsey’s response was to allow a propositional function to be any arbitrary assignment of propositions to the individuals of a given type – consequently, there will be undefinable propositional functions, determining indefinable classes. But, as Giaquinto points out (p109), this breaks the link between conditions and propositional functions, so the connection to logic is lost. The incoherence in logicism remains unresolved, unless we admit indefinable classes as part of logic (a view that may have been Ramsey's, and which Giaquinto critically examines pp.109-113). Moreover, logicism aside, the simple theory of types will not serve as a foundational system for mathematics. For one thing, in simple type theory, the Axiom of Infinity implausibly requires the existence of infinitely many (non-mathematical) individuals; for another, simple type theory unnaturally fragments the cardinal numbers – the number κ is represented by an infinite number of entities, one for each type (above the lowest).

Ramsey failed, then – but according to Giaquinto, no-one could have done better: the logicist cause was "hopeless" (p.116). So in Part IV, Giaquinto turns his attention to the other major response to the paradoxes: *axiomatic set theory*. In 1908, Zermelo presented the first axiomatization of set theory, and his motivation was very different from that of the logicists. In view of the paradoxes, "it no longer seems admissible today to assign to an arbitrary logically definable notion a set, or class, as its extension" (Zermelo 1908, p.200). Zermelo continues:

> "Under these circumstances there is at this point nothing left for us to do but to proceed in the opposite direction and, starting from set theory as it is historically given, to seek out the principles required for establishing the foundations of this mathematical discipline. In solving the problem we must, on the one hand, restrict these principles sufficiently to exclude all contradictions and, on the other, take them sufficiently wide to retain all that is valuable in this theory" (*ibid*.)

Abandoning the predicative conception of sets, Zermelo adopts a more pragmatic stance: the task is find axioms that avoid the paradoxes and yield "the entire theory created by Cantor and Dedekind" (*ibid*.) The resulting set theory will not be part of logic – Zermelo makes no bones about that. So how will axiomatic set theory justify mathematics? This is the leading question of the second half of *SC*. But we can identify two constraints from the outset: the theory must be consistent, and it must be *natural* in some appropriate sense, and not merely an *ad hoc* response to paradox.

Giaquinto focuses appropriately on the Axiom of Separation, which he presents this way (p.120):

> For any set m and any condition F that is definite for m, there is a set whose members are exactly the members of m satisfying the condition F.

The Axiom restricts naïve Comprehension, according to which every condition has an extension – rather, it says, *given a set m*, one can separate off the set of m's members that are F. There is no set of all the non-self-membered sets; but given a set m, there *is* a set of those members of m that are non-self-members. So the class paradoxes are avoided. But as Skolem later remarked, the notion of *definite* here "is a very deficient point in Zermelo" (Skolem 1922, p.292). Zermelo characterized a definite condition this way: for each element x of the domain it must be determined whether the condition holds of x or not (See Zermelo 1908, p.202). This is vague and unsatisfactory, and the notion *holding of* seems to be a semantic relation, out of place in axiomatic set theory. Skolem's clarification is now standard. Take the language of set theory, precisely formulated in the usual recursive way; then a definite condition is simply one expressed by a 1-place predicate of the language.

What has become of the definability paradoxes? Giaquinto makes no mention of them in his discussion of Separation and definiteness, and this is a puzzling omission. Zermelo himself is quite clear: since Separation requires that conditions be definite, "all criteria such as 'definable by means of a finite number of words'', hence the 'Richard antinomy' and the 'paradox of finite denotation', vanish" (Zermelo 1908, p202). If we follow Skolem, the definability paradoxes really do vanish – the formal language of set theory does not even contain the predicate 'defines'. Get the language of set theory straight, and you can safely ignore the definability paradoxes altogether (along with the Liar). This seems to be Giaquinto's implicit view of the matter, as one might expect from his apparent endorsement of Ramsey's dismissal of the definability paradoxes. But it's surely worth making this view fully explicit, for it has substantive consequences: if your task is to justify mathematics, then the definability paradoxes and the Liar are red herrings, and the search for a unified solution to the paradoxes is irrelevant.

Is Zermelo's set theory, supplemented by the Axiom of Replacement, a suitable foundational theory for mathematics? Giaquinto considers a number of challenges to a foundational role for this set theory (henceforth ZFC). One is due to Skolem. Here the question is: what *are* sets? Sets can no longer be taken to be the extension of concepts or conditions – but perhaps the axioms tell us all we need to know about sets. This hope is scotched by the Lowenheim-Skolem theorems and Skolem's paradox. Giaquinto gives a characteristically clear and accessible account of the main ideas of the downward Lowenheim-Skolem theorem, and carefully draws out its consequences. Skolem's paradox is not of course a genuine paradox, but it is significant; it shows that the axioms do not determine a unique universe of sets, and there is an unavoidable relativity in basic notions such as *enumerability*, *finite*, *infinite*, *cardinality*, and *power set*. "The axioms cannot, therefore be taken as an implicit definition of set-hood and membership; otherwise they would also define notions built up from the notions of set and membership, such as the notion of enumerability." (p.135) Other challenges to a foundational role for ZFC came from Brouwer's intuitionism and Weyl's predicativism, and Giaquinto briefly discusses these. (Reasonably enough, Giaquinto says very little about Brouwer's intuitionist programme in *SC*. As he says in the Preface, he is concerned with the securing of accepted mathematics, not with its replacement.)

But the main challenge to ZFC was this: is it even *consistent*? We are led, then, to Hilbert. It is perhaps likely that philosophers and philosophy students will be more familiar with Frege's and Russell's logicism, the paradoxes and set theory than with Hilbert's programme. So the quality of Giaquinto's exegesis here is especially welcome – as always, there is clarity and accessibility, but no cutting of corners. The reader is led smoothly through Hilbert's finitism, real and ideal propositions, instrumentalism, and finitary reasoning. (One quibble: given the central role that it plays, primitive recursive arithmetic might have been given a somewhat fuller treatment. The presentation in SC is limited to an abstract overview (p.154), and an illustration in an appendix.) Giaquinto contrasts Hilbert's programme nicely with those of Frege and Russell. On the Fregean approach, we justify mathematics by deriving it from indubitable axioms via inference rules that are indubitably truth-preserving. Russell's way is to establish the truth of the axioms beyond a reasonable doubt, by showing that they are inductively well-supported by their known consequences (the simplest, most evident truths of arithmetic) – and then showing that the rest of mathematics is also true, because derivable from the axioms. Hilbert differs from them both. Unlike Frege, who assumed the reliability of the axioms and rules of inference, Hilbert sought to demonstrate it. And unlike Russell, Hilbert sought to show the *reliability*, rather than the truth, of mathematics. Like Russell, though, Hilbert took as his epistemological base not the axioms but the simple propositions of finitary arithmetic. And then the rest of mathematical theory is demonstrably reliable if it has no false finitary consequences, and if this can be shown by the finitary methods associated with primitive recursive arithmetic (PRA).

Giaquinto outlines Hilbert's strategy for carrying out this program. The first stage is formalization – arithmetic, analysis and set theory are set out as completely precise formal systems. Once formalized, these systems are amenable to finitary methods. The second stage is to demonstrate reliability and consistency. Giaquinto explains carefully why, in order to establish the reliability of a given theory T, it is enough to show that the translation into T of some particular finitary falsehood, say $1 \neq 1$, is not derivable in T. It seemed likely that this strategy would succeed: Giaquinto points out that results obtained by 1927 had moved the formal project forward, and philosophical objections due to Brouwer could be convincingly met. But then came Godel's theorems.

The content and significance of Godel's incompleteness theorems are easy to sloganize, and it is a real virtue of *SC* that Godel's theorems and their consequences for Hilbert's programme are treated with care and subtlety. For example, as he traces the path to Godel's first incompleteness theorem Giaquinto is careful (unlike some) to distinction between *representing* a formal property and *expressing* a formal property. A predicate φ represents a formal property F if φ is true of exactly the Godel numbers (or codes) of the syntactic objects that are F; but for φ to express F, we need a tighter connection than mere co-extensionality (which can be captured if φ is constructed in parallel with the definition of F. The point is well-illustrated using the formal property of *being a term* (pp.169-70).) Giaquinto goes on to present the Diagonalization Lemma (a sketch of its proof is given in an appendix), and he shows how it leads to Tarski's theorem on the undefinability of truth. He then sketches *two* proofs of Godel's first incompleteness theorem, one via the Diagonalization Lemma, the other via Tarski's theorem. In his balanced appraisal of the significance of Godel's first theorem, Giaquinto concludes that the theorem bears on the notions of mathematical truth and mathematical existence. It shows that you cannot pin down the notion of mathematical truth via the notion of derivability. And it shows that consistency does not guarantee existence (by Godel's theorem, the theory $T + \neg\gamma$ is consistent but has no model, where T is second-order Dedekind-Peano arithmetic and γ is the Godel sentence for T).

What about the significance of Godel's first theorem for Hilbert's programme? A standard line is that Godel's theorem blocks Hilbert's programme. Let S be a formalization of finitary number theory, and let T be a formalization of number theory (or analysis or set theory). Hilbert's goal was to show the finitary reliability of T. The strategy was to show that T is *conservative* over S (any formula of S derivable in T is derivable in S). But Godel's theorem shows that this strategy cannot succeed: the Godel sentence γ for S may be derivable in T (but not of course in S). But Giaquinto points out, following Detlefsen, that this is not quite the end of the matter. The finitary reliability of T is not the same as the conservativeness of T over S (though if you show the latter you show the former). The reliability of T is a matter of T's having no false finitary consequences, and this is a weaker condition than T's being conservative over S. This leaves open the possibility that the reliability of T might be established by some other strategy.

Godel's second incompleteness theorem casts doubt on even this possibility. Perhaps inevitably, the outline of the proof is sketchier than previous outlines, but the underlying ideas are there. A valuable feature of the presentation is its emphasis on the *intensional* character of the theorem – this is an observation that can give a reader real insight into the theorem, even if its proof is only sketched. Giaquinto states the theorem this way (p.184):

> If T is a consistent standard formal system containing PRA, a sentence in the language of T expressing the claim that T is consistent is not derivable in T.

It is the notion of *expressing* that introduces intensionality: if a candidate sentence only 'represents' the claim that T is consistent (in the sense that it is constructed from a predicate that doesn't express but only represents the relation *x is a derivation in T of y*), then the proof will not go through. An appendix is devoted to the intensionality of the Godel's second theorem, and it provides two examples (due to Feferman and Mostowski) that show the need for intensional as well as extensional correctness.

The challenge to Hilbert's programme is this. Godel's second theorem show that there is no finitary proof of T's consistency. Now suppose that T is reliable – that is, no false finitary sentence is derivable in T. It follows that T is consistent (if it wasn't, every sentence of T would be derivable, including its false finitary sentences).

> "The argument just given, from T's reliability to T's consistency, lies within the scope of finitary reasoning, and so any finitary proof of the former could be extended to a finitary proof of the latter. But we have already concluded that there is no finitary proof of T's consistency. So there is no finitary proof of T's reliability – and this applies to any formal system of concern for Hilbert's Programme. Hence the goal of the programme cannot be achieved. This argument, allowing for variations of detail, is what sustains the orthodox view of the import of the Second Underivability Theorem for Hilbert's Programme." (pp.185-6)

Giaquinto considers two ways to respond to this argument. First, there might be non-standard formalizations of arithmetic that evade Godel's second theorem; and second, there may exist finitary proofs that cannot be expressed in a system T containing PRA. In an up-to-date and sophisticated critical discussion (pp.186-193), Giaquinto expresses his strong doubts about both these responses.

Let us take stock. Our main question is this: Is ZFC an appropriate foundational theory for mathematics? We've seen that Hilbert's attempt to show the reliability of set theory (and number theory and analysis) failed. And the Skolem-Lowenheim theorems show that the axioms of ZFC do not provide an implicit definition of the notion of sethood. So shouldn't we conclude that ZFC is an ad hoc repair to an inconsistent theory, and so unsuited to play any foundational role? Giaquinto says no: there *is* a unifying picture of the universe of sets, and the axioms of ZFC are a partial articulation of it. The structure of a universe of sets is a *cumulative hierarchy of stages*. At the first stage for a given universe are the atoms or individuals, non-sets that do not have members (this first stage may be empty). At the next stage is the set of everything at the first stage, together with all sets that can be formed by combining members of the first stage. At the following stage are all sets that can be formed from sets and individuals at the previous stages. In general, a successor stage $\alpha+1$ comprises all the members and subsets of stage α, and a limit stage is the union of all previous stages. This picture – which following Godel became known as the *iterative* conception of set - was first presented in Zermelo 1930. And following Dana Scott, Giaquinto formulates principles about stages and sets, and derives axioms of ZFC from them. (The reader might also have been referred to Boolos 1971, which is mentioned in the bibliography but nowhere else in *SC*.) So ZFC

> "arises from a complex but quite natural conception of a universe of sets. Thus, we have a positive idea of a model of set theory, and because of this the fear that the theory is inconsistent has, quite reasonably, fallen away." (p.213)

What of ZFC's response to the class paradoxes? It is easy to take the ZFC solution to be simply this: certain predicates of the language of ZFC – for example, '$x=x$' and '$\neg x \varepsilon x$' – do not have extensions. And this might seem *ad hoc*; as Frege asked, "… how do we recognize the exceptional cases?" (quoted in *SC*, p.55). But there is more to the solution than this. According to the iterative conception, any candidate universe is the starting point for further iterations. The non-sets of one universe (Cantor's absolutely infinite multiplicities, or von Neumann's proper classes) are *sets* in the next universe and all those that follow. The way out taken by Cantor and von Neumann is obviously *ad hoc* – it is simply stipulated that proper classes cannot be members. But Giaquinto takes Zermelo's idea to be subtler: the paradox-producing predicates have no absolute fixed extension independent of context. Once a context is fixed – that is, once a universe V is specified – then the class paradoxes do not arise. For example, the Russell class R_V of exactly the non-self-membered members of V is not a member of itself, because R_V is not a member of V – and so the argument to a contradiction is blocked. But the Zermelo solution does more than block the paradoxes. It also diagnoses and explains the fault in our reasoning:

> "The fault lay in assuming certain predicates to have fixed extensions independent of context. That is wrong because it assumes the existence of an absolute all-

inclusive universe of sets (for a fixed basis of atoms), when in fact there is no such total universe." (p.217)

Why do we fail to see the fault? First, the relevant contextual items, the universes of set theory, come late in a study of set theory. Second, we feel no need to fix the domain: we take the domain of discourse to be fixed by our informal use of the predicate 'set' – the universe is simply *all sets*. "So there is a ready explanation of the difficulty in coming to see this particular form of context dependence." (*ibid*). ZFC doesn't merely block the paradoxes, it offers a *solution*. Add to this its embodiment of the natural iterative conception of set, and ZFC may seem well-placed to play a foundational role.

Giaquinto offers here an intriguing defence of the ZFC solution to the class paradoxes. One would like to know more, however, about the crucial notion of context-dependence. What features of context trigger shifts from one universe of sets to another? The usual contextual parameters - speaker, time, place, discourse position, presuppositions, relevant information, speaker's intentions – have no obvious application in the present case.[3] One might also press on Zermelo's (and Giaquinto's) rejection of an *absolute* universe of sets. Suppose I am asked about the range of my set theoretic quantifier – then I might well reply that it *cannot* be a set, and that there is no more inclusive universe in which it is a set. If I study more set theory, and learn about, say, the notion of an inaccessible cardinal, then I've come to a better understanding of the universe of sets – but why suppose that my universe has expanded? D. A. Martin writes: "Though in some sense my coming to hold that inaccessibles exist has perhaps changed my concept of set, the notion of all-inclusiveness, of absolute infinity, was already a central part of my concept" (Martin, pp.3-4).[4]

One further challenge. In his original 1908 paper, Zermelo proves that the domain of his set theory "is not itself a set, and this disposes of the Russell antinomy so far as we are concerned" (Zermelo 1908, p.203). So the set/non-set distinction was prominent from the very beginnings of axiomatic set theory. And as Skolem later remarked: "If we adopt Zermelo's axiomatization, we must, strictly speaking, have a general notion of domains in order to provide a foundation for set theory." (Skolem 1922, p.292). Even if we adopt Zermelo's relativized view of the set/class distinction, we must still have the conceptual resources to accommodate non-sets; once a universe V is fixed, we must be able to conceive of V itself *in some non-iterative way*. And if ZFC is to be a natural and intuitive theory, as befits a foundational theory, the conception of its domain must be natural and intuitive too. Von Neumann's artificial proper classes won't fit the bill (and the same goes for Cantor's absolutely infinite multiplicities). How *do* we grasp the notion of the domain of set theory? Presumably as a *concept-extension*: as the extension of 'x=x' or 'x is a set' (or perhaps 'x is a set or an individual'). But this suggests that we cannot so easily dispense with the predicative conception of class – it appears to be implicated at the very heart of ZFC. (One might add that the conception also lies behind the Axiom of Separation, where sets are tied to conditions – it is possible to regard Separation as a way of preserving what can be consistently preserved of the predicative conception.) So now extensions or classes are on the scene; V is understood as a predicatively given class. But then it can be argued that ZFC is only as well-understood as the notion of extension - and in my view there is much work to be done before we understand that notion.[5]

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Endnotes

[1] The author's division of parts into chapters, and chapters into short sections, is very helpful. The same cannot be said of OUP's presentation of the endnotes. Locating endnotes can be very frustrating: there may be no clue as to chapter, and if there is, still no clue as to part – and things are not helped by the typo that reads "Part V' instead of 'Part VI'. Why not tie endnotes to page numbers? It is also a pity that the endnotes are not indexed.

[2] See, for example, Frege 1895, p. 102, p.106, and pp.104-5; Frege 1893, pp.149-50; and Frege 1979, p.34.

[3] In my view, a contextual account *can* be given for the notion of an *extension of a predicate*. There are contextual accounts of the truth predicate (including Simmons 1993), and I think contextual accounts can be extended to the notion of reference (see Simmons 1994) and the notion of extension (see Simmons 2000).

[4] Martin is responding to Parsons 1974, in which the set/class distinction is treated relativistically. Further discussion can be found in Maddy 1983. These three papers, not mentioned in *SC*, are recommended to the interested reader.

[5] Martin, Maddy 1983 and Simmons 2000 are attempts to develop a paradox-free account of extensions.