

Structural Relativity and Informal Rigour

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Abstract

Informal rigour is the process by which we come to understand particular mathematical structures and then manifest this rigour through axiomatisations. Structural relativity is the idea that the kinds of structures we isolate are dependent upon the logic we employ. We bring together these ideas by considering the level of informal rigour exhibited by our set-theoretic discourse, and argue that it is best captured by a logic intermediate between first- and second-order. We argue that the usual division of structures into *particular* (e.g. the natural number structure) and *general* (e.g. the group structure) is perhaps too coarse grained; we should also make a distinction between *intentionally* and *unintentionally* general structures.

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Introduction

Mathematicians are often concerned with elucidating structure. In this paper, I'll examine some issues arising under the following assumption:

(Weak Structuralist Assumption) Part of mathematics and its practice can be understood as isolating and studying different structures.

Why is this assumption 'weak'? Well, the usual statement of structuralism is that mathematics just *is* the study of structure.¹ We do not make such a strong claim. Rather, we are just assuming the highly plausible claim that mathematics is at least partly concerned with the specification and study of structure.

Two questions are immediately pertinent:

1. What kinds of different structures are there?
2. How to we isolate them and/or talk about them?

The first question is often answered by distinguishing between two kinds of structure; *particular* and *general*. Isaacson explains the distinction as follows:

The particularity of a particular structure consists in the fact that all its exemplars are isomorphic to each other. The generality of a general structure consists in the fact that its various exemplars need not be, and in general are not, isomorphic to each other. ([Isaacson, 2011], p. 21)

Exactly what different branches of mathematics have an underlying 'particular structure' is a contentious issue (we discuss this later). However, almost everyone agrees that we can talk about various kinds of *finite* particular structure (e.g. the structure of the number ten under the less-than relation). Normally it is assumed that most of our

¹A good example here is Shapiro:

For our first (or second) approximation, then, pure mathematics is the study of structures, independently of whether they are exemplified in the physical realm, or in any realm for that matter. ([Shapiro, 1997], p. 75)

Examples can be multiplied (e.g. [Resnik, 1997], [Hellman, 1996]). See [Reck and Schiemer, 2019] for a survey.

arithmetical talk is concerned with a particular structure; the standard model for arithmetic.²

General structures, by contrast, are not determined up to isomorphism. Examples of these include groups, rings, and fields. For example the group of symmetries on a triangle and the group of integers both possess the general structure of being a group, but the former is finite where the latter is infinite.

It is a somewhat controversial question as to whether these two kinds of structure are of the same ontological kind or not, since particular structures *seem* more fundamental than general structures, in the sense that the latter are properties that the former can possess. We speak, for example, of the particular structure of the integers *exemplifying* the ring structure or the particular structure of the natural numbers under addition *exemplifying* the general structure of a monoid.³ Still more concrete are the *systems* exemplifying particular structures. For example, the face of the clock on my wall (with the usual operations of addition) is a *system* exemplifying the *particular* structure of the integers mod 12, which in turn exemplifies the *general* group structure.

The second question (how we isolate and talk about the different kinds of structure) is then easy in the case of general structures for the Weak Structuralist; she can simply state the properties she is interested in for some general structure, and in doing so talks about any particular structures and/or systems that exemplify these properties. The question is harder for particular structures, since here there is the additional challenge of convincing ourselves that we have isolated a structurally unique entity (at least up to isomorphism⁴).

One way of tackling the question of when we have isolated a particular structure can be derived from the work of Kreisel⁵, and has been taken up subsequently by Isaacson⁶. They suggest that we have a process of *informal rigour* by which we obtain mathematical understanding and isolate different particular structures. The rough idea

²See [Hamkins, 2012b] for a dissenting voice that we discuss a bit later.

³Isaacson (in [Isaacson, 2011]) seems to take the view that particular structures are somehow more fundamental, referring to a general structure with no particular instances as “vacuous” (p. 25). Similar remarks can be found in [Leitgeb, 2020], where unlabelled graphs are taken as the particular ‘ground level’ structures, and general structures are viewed as higher-order properties or classes of particular structures.

⁴There is a substantial discussion around whether isomorphism is too strong, and perhaps something weaker like *definitional equivalence* would be better. We set aside this issue for now, things are complicated enough without opening that can of worms, despite its interest. For an overview, see [Button and Walsh, 2018], Ch. 5.

⁵In [Kreisel, 1967]

⁶In [Isaacson, 2011].

(which I discuss in more detail below) is that we isolate a particular structure by becoming more rigorous about a topic, and manifest this rigour by providing a categorical axiomatisation.

A categorical axiomatisation is a set of axioms T which determines a unique model up to isomorphism (i.e. any two models of T are isomorphic). Where categoricity is concerned, one must talk about different logics. The insight provided by Löwenheim-Skolem Theorem shows that first-order logic cannot provide categorical axiomatisations for infinite structures. It is in the work of Resnik (in particular [Resnik, 1997]) where we find a notion of *structural relativity*; the idea that the structure isolated for different parts of mathematics depends on the logical resources we consider.

This paper brings together these ideas focussing on set theory as a case study. We argue for the following claims:

1. We have good reason to doubt that we are *fully* informally rigorous about set theory. Rather our level of informal rigour is *partial*.
2. Given this level of informal rigour, it is reasonable to hold that our set-theoretic thought might be axiomatised in a logic *stronger* than first-order, but *weaker* than second-order.
3. This shows that the usual distinction between *particular* and *general* structures corresponding to different concepts is more fine-grained than we might have initially thought. There are concepts that correspond to *intentionally general structures* in that the concept is designed to talk about many non-isomorphic structures. Other concepts correspond to *unintentionally general structures*, where we do not *intend* for the structure we talk about to be general, yet we do not pin down a *particular* structure with our discourse.

Here's the plan: §1 examines the notion of informal rigour as it appears in Kreisel's 1967 paper and how it relates to the problem of the Continuum Hypothesis. §2 presents three possible interpretations of informal rigour; a quasi-idealist one, a weakly platonistic one, and a strongly platonistic one (we'll see shortly what I mean by these terms). §3 presents the idea of structural relativity. §4 then examines different states we may be in with respect to informal rigour, and examines some possibilities for axiomatisations of our thought. §5 examines some objections and replies. Finally §6 concludes with some open questions.

1 Informal rigour and the Continuum Hypothesis

In this section we explain *informal rigour* and the idea that it might be used to show the existence of particular structures. We'll do this by explaining Kreisel's rough idea, and then formulating a more precise thesis (that particular structures are determined via informal rigour) at the end of the section. We'll also explain how Kreisel thought that his account of informal rigour leads to a determinate truth value for the Continuum Hypothesis.⁷

Kreisel (in [Kreisel, 1967]) discusses the notion of *informal rigour*. This represents a development of the idea that we work mathematically simply by examining our intuitive notions and laying down axioms for them. Kreisel expands this a little, saying that the process is not quite so simple, but we can (by successively becoming more clear about a mathematical subject matter) come to successful axiomatisations for intuitive notions. He writes:

Informal rigour wants (i) to make this analysis as precise as possible (with the means available), in particular to eliminate doubtful properties of the intuitive notions when drawing conclusions about them; and (ii) to extend this analysis, in particular not to leave undecided questions which can be decided by full use of evident properties of these intuitive notions. ([Kreisel, 1967], pp. 138–139)

Kreisel's point is well-taken, and the history of mathematics is replete with notions that were initially unclear but slowly came to be made precise through development and reflection. Examples include ideas of completeness and denseness (initially these were confused), the notion of derivative (we will discuss this later in §5), Cantor's analysis of the size of sets, and indeed the notion of set itself was gradually made clearer. However, whilst Kreisel's remarks are suggestive,

⁷Interestingly, it certainly seems like Kreisel held something like the Weak Structuralism. For example, he writes:

if one thinks of the axioms as *conditions* on mathematical objects, i.e. on the structures which satisfy the axioms considered, these axioms make a selection *among* the basic objects; they do not tell us what the basic objects are. ([Kreisel, 1967], p. 165, emphasis original)

Whilst the extent to which Kreisel *really was* a structuralist (rather than merely provided resources *useful* to structuralism) is certainly an interesting question, I lack the space to address it fully here.

he does not provide a detailed account of exactly what informal rigour is like. Largely speaking, he takes it for granted that we know what it is when we see it (at least as far as his [Kreisel, 1967] is concerned).

Despite this, we can make some progress by examining specific questions:

- (1.) What are the targets of informal rigour?⁸
- (2.) How do we achieve informal rigour?
- (3.) What are the consequences of informal rigour?

For (1.) some taxonomy will be useful. When we talk about mathematical structure, there are several important aspects:

- (a) The *concepts* we employ in thinking about mathematics (I'll refer to these using C, C_0, C_1, \dots etc.).⁹
- (b) The *mathematised natural language(s)* we use when speaking about structure(s). We will refer to these as *discourses*, and denote them by (D, D_0, D_1, \dots) .
- (c) Different formal mathematical *theories* ($\mathbf{T}, \mathbf{T}_0, \mathbf{T}_1, \dots$).
- (d) Different mathematical *structures*, both particular and general (S, S_0, S_1, \dots).
- (e) Different *systems* exemplifying structures, which for convenience we'll assume are model-theoretic structures ($\mathfrak{M}, \mathfrak{M}_0, \mathfrak{M}_1, \dots$).

It is important to be clear about these distinctions if we are to provide a fully worked-out account on Kreisel's behalf. Nowhere is he fully explicit about the matter, but his discussion (and a reasonable understanding of the notion) seems to suggest that informal rigour concerns how the concepts underlying discourses can be refined in coming to be precise about structures. Mathematical practice involves communicating in a mathematised natural language, and how we interpret this language is contingent upon the concepts being employed.

⁸I thank Verena Wagner for pressing this question in discussion.

⁹Juliette Kennedy suggests that talk of concepts is too unclear, and we would be better off eliminating this language altogether. I am somewhat sympathetic to this position, and certainly feel that it can sometimes muddy the waters. Despite this, language of this kind is useful for setting up the debate, and I'm happy to use it here. For the reader who has doubt about the coherence of concept-talk, I suggest that they read all mention of concepts as shorthand for their favourite account of what the constituents of thoughts are.

For example, the interpretation we ascribe to a computer scientist using the term “set” (in a context where we can have non-well-founded ‘sets’) is different from the interpretation we would ascribe to a set theorist working in some extension of **ZFC**. This isn’t a contradiction; they are simply employing different concepts with their use of language and *mean* different things with their usage of the term “set”. Correspondingly, there are different ways we could systematise or represent their language formally, and in turn different interpretations of this formal language. At the bottom level, the formal theories representing different pieces of mathematised language can be interpreted (contingent on the concepts employed) as about different kinds of structure.

In the rest of the paper, we will assume that the main *target* of informal rigour is the *concepts* we employ when speaking or writing in mathematical language (i.e. *discourses*). Perhaps there is more to be said here, but I’m happy to make this assumption for the purposes of the paper.

With the targets and rough idea of informal rigour in play, we can begin to address (2.) How do we achieve informal rigour? Kreisel provides four examples¹⁰, key to each is the idea that we develop informal rigour concerning a concept via working with it in practice. In this way we can develop our intuitions, and come to be rigorous about a notion. This rigour can then be formally codified. Our interest will be especially in his remarks about the difference between the independence of the Parallels Postulate from the second-order axioms of geometry, and the independence of CH from the axioms of **ZFC**.

Discussing Zermelo’s isolation of the axioms of **ZFC**₂¹¹. Kreisel discusses the following:

Theorem (Zermelo-Shepherdson¹²). Let \mathfrak{M} and \mathfrak{N} be models of **ZFC**₂. Then either:

¹⁰These include: (I) analysing the difference between independence results, such as the parallels axiom in geometry and the independence of CH in set theory (the focus of this paper), (II) the relation between intuitive consequence and syntactic/semantic consequence (here he gives his famous ‘squeezing’ argument, arguing that the informal notion of consequence can be squeezed between the formal classes of a syntactic derivation in first-order logic and semantic consequence in first-order logic), (III) Brouwer’s ‘empirical’ propositions, and (IV) showing that the use of certain models is a conservative extension of arithmetic.

¹¹**ZFC**₂ denotes second-order Zermelo-Fraenkel Set Theory with the Axiom of Choice.

¹²[Shepherdson, 1951], [Shepherdson, 1952], and [Shepherdson, 1953] take [Zermelo, 1930]’s sketch and make it substantially more rigorous.

1. \mathfrak{M} and \mathfrak{N} are isomorphic.
2. \mathfrak{M} is isomorphic to proper initial segment of \mathfrak{N} , of the form V_κ for inaccessible κ .
3. \mathfrak{N} is isomorphic to proper initial segment of \mathfrak{M} , of the form V_κ for inaccessible κ .

The core point is the following; whilst there is no¹³ full categoricity theorem for second-order set theory \mathbf{ZFC}_2 , there is for initial segments. In particular, many versions of \mathbf{ZFC}_2 with a specific bound on the number of large cardinals (e.g. “There are no inaccessible cardinals” or “There are exactly five inaccessible cardinals”) are categorical.

Concerning this theorem, Kreisel writes:

the actual formulation of axioms played an auxiliary rather than basic role in Zermelo’s work: the intuitive analysis of the crude mixture of notions, namely the description of the type structure, led to the good axioms: these constitute a record, not the instruments of clarification. ([Kreisel, 1967], p. 145)

Abstractly speaking, Kreisel’s position might then be described as follows. We begin to work with an informal concept C , employing it in some mathematical discourse D . Gradually we begin to become clearer about D and C via using them in practice, and developing our intuitions about the subject. Once we are eventually clear about the right concept C' underlying D (it is at least possible that $C' = C$ here), we will have obtained sufficient precision to lay down a theory \mathbf{T} for C' , which is *categorical* in that any system $\mathfrak{M} \models \mathbf{T}$ is isomorphic to any other system $\mathfrak{M}' \models \mathbf{T}$. In this way, by employing our concept C' and using \mathbf{T} , we have determined a particular structure S up to isomorphism. In the case of set theory, we can think of the development of the idea of *cumulative hierarchy* and *iterative conception of set* after 1900 as yielding some particular set-theoretic structures by 1930 when Zermelo gave his axiomatisation.

This brings us on to (3.) What are the consequences of informal rigour? Our focus will be how informal rigour affects our attitude to the truth value of CH. Key here is the Zermelo-Shepherdson quasi-categoricity theorem: In this way *given an interpretation of the second-order variables* (this will be important later), \mathbf{ZFC}_2 determines several

¹³Without further meta-theoretic assumptions. See [McGee, 1997] for a full categoricity result using urelements.

particular structures corresponding to initial segments of the cumulative hierarchy.

Kreisel took this to show that our talk concerning the cumulative hierarchy, as axiomatised by \mathbf{ZFC}_2 , was unambiguous. He writes:

Denying the (alleged) bifurcation or multifurcation of our notion of set of the cumulative hierarchy is nothing else but asserting the properties of our intuitive conception of the cumulative type-structure mentioned above. ([Kreisel, 1967], pp. 144–145)

Why is this significant for CH? Well, since the truth value of CH is settled by $V_{\omega+2}$ (well below the least inaccessible) and if we think that all models of \mathbf{ZFC}_2 agree up to the first inaccessible (by the Zermelo-Shepherdson quasi-categoricity theorem), then CH has the same truth-value in all particular structures meeting our informally rigorous concept of set (so the thinking goes). This, as Kreisel points out, makes the independence of CH from set-theoretic axioms markedly different from the independence of the Parallels Postulate (PP) from the axioms of geometry; PP can have different truth-values across models of the *second-order* axioms of geometry (once we fix upon some interpretation of the second-order variables), whereas CH has the same truth value in all models of \mathbf{ZFC}_2 with the same interpretation of the range of second-order quantifiers.

To make the state of the dialectic precise, and given the difficulty of interpreting Kreisel, it is worth pulling out the key moving parts of our interpretation of Kreisel's presentation:

(Assumption of Informal Rigour) A mathematical discourse D determines a particular structure S when we are informally rigorous in employing the relevant concept C corresponding to D , and this informal rigour can be manifested in a categorical axiomatisation \mathbf{T} of C such that for any systems \mathfrak{M} and \mathfrak{M}' exemplifying S , both \mathfrak{M} and \mathfrak{M}' satisfy \mathbf{T} and are isomorphic.

(Manifestation Thesis) We become informally rigorous about a concept C not by using an axiomatisation to clarify it, but rather through developing our mathematical understanding of C by working with it in practice. This understanding can then be *manifested* by a categorical axiomatisation \mathbf{T} . (In other words, the existence of a categorical axiomatisation is necessary for us to have informal rigour.)

(Segment Particularity Thesis) We are informally rigorous about the concept *cumulative type structure below the first inaccessible*, and this concept is axiomatised by the theory $ZFC_2 +$ “There are no inaccessible cardinals” and determines a particular structure.

(CH-Determinateness Thesis) The concept *cumulative type structure* suffices to determine CH.

(Difference Thesis) The kind of independence exhibited by CH (relative to ZFC_2) and PP (relative to the axioms of geometry) are of fundamentally different kinds.

In what follows, we shall take the Assumption of Informal Rigour as an assumption (though we’ll discuss how to flesh it out in more detail). This is just because I’m interested in exploring the idea; it’s clearly a very controversial assumption! We’ll argue that the Segment Particularity Thesis and CH-Determinateness Thesis are false (or, at the very least, we have good reason to doubt them). We’ll then argue that the Manifestation Thesis suggests that our thought is perhaps best axiomatised by something weaker than ZFC_2 . We’ll also argue that the Difference Thesis still holds true.

2 Three interpretations of informal rigour

In the last section, we saw some theses that one might extract from Kreisel’s paper on informal rigour. In this section, I’ll present three ways of interpreting this process of informal rigour that will be important for later.

2.1 Isaacson’s Kreisel

One way of interpreting the process of informal rigour has been proposed by Dan Isaacson (in [Isaacson, 2011]). There he seems to commit himself to the Assumption of Informal Rigour in the following passage:

We achieve understanding of the notion of mathematical structure not by axiomatizing the notion but by reflecting on the development of mathematical practice by which particular mathematical structures come to be understood, the

natural numbers, the euclidean [plane], the real numbers, etc. how do we know that such structures exist? The question is likely to be construed in such a way that it is a bad question. There is nothing we can do to establish that particular mathematical structures exist apart from articulating a coherent conception of such a particular structure. ([Isaacson, 2011], p. 29)

as well as the Manifestation Thesis:

...if the mathematical community at some stage in the development of mathematics has succeeded in becoming (informally) clear about a particular mathematical structure, this clarity can be made mathematically exact. Of course by the general theorems that establish first-order languages as incapable of characterizing infinite structures the mathematical specification of the structure about which we are clear will be in a higher-order language, usually by means of a full second-order language. Why must there be such a characterization? Answer: if the clarity is genuine, there must be a way to articulate it precisely. if there is no such way, the seeming clarity must be illusory. ([Isaacson, 2011], p. 39)

However, his interpretation of these notions is decidedly *not* objective in the platonistic sense of concerning mind-independent abstract objects:

The basis of mathematics is conceptual and epistemological, not ontological, and understanding particular mathematical structures is prior to axiomatic characterization. When such a resulting axiomatization is categorical, a particular mathematical structure is *established*. Particular mathematical structures are not mathematical objects. They are characterizations. ([Isaacson, 2011], p.38, my emphasis)

So, for Isaacson, the process of informal rigour can be understood as a *mind-dependent* activity in some sense. The process informal rigour should not be understood as one where we pick out some pre-existing ontological objects, but rather as the determination of a particular structure using our thought and language, one that does not exist in advance of our characterising activity (in this sense, his view is quasi-idealist). This precision *in our concept* is then manifested by a categorical axiomatisation T.

Isaacson's claim that particular structures just *are* characterisations is a little puzzling; the claim that particular structures are literally numerically identical with theories (i.e. characterisations) has the whiff of a category mistake about it. However, it serves to show further how we might think of informal rigour as a process of mathematical claims being dependent upon our epistemological and conceptual activity, rather than any independently existing structural domain.

Isaacson's version of informal rigour does not commit him to an 'anything goes' version of conventionalism. First, given some employed concepts about which we are informally rigorous, there can be objective facts about what follows from that concept.¹⁴ This is visible from Isaacson's endorsement of the CH-Determinateness Thesis.¹⁵ Moreover, we are able to fix infinitely many structures in this way.¹⁶ For example, the categoricity of the natural numbers establishes infinitely many particular structures, e.g. the structure exemplified by $(n, <)$ for any chosen n . Since it is unclear whether or not Kreisel would have accepted Isaacson's interpretation, I shall refer to a character I call 'Isaacson's Kreisel' as a proponent of this view of informal rigour.

¹⁴A good question, one we do not have space to address here, is how Isaacson's version of Kreisel relates to [Ferreirós, 2016]'s account of mathematics as *invention cum discovery*.

¹⁵He writes:

...the independence of the continuum hypothesis does not establish the existence of a multiplicity of set theories. in a sense made precise and established by the use of second-order logic, there is only one set theory of the continuum. it remains an open question whether in that set theory there is an infinite subset of the power set of the natural numbers that is not equinumerous with the whole power set. ([Isaacson, 2011], pp. 48–49)

¹⁶He writes:

While indeed there are up to any given moment of course only finitely many theorems establishing categorical characterizations of structures, e.g. of the natural numbers, the real and complex numbers, the euclidean plane, the cumulative hierarchy of sets up to a particular ordinal, one such theorem may establish categorical characterization of infinitely many particular substructures. ([Isaacson, 2011], p. 38)

2.2 Weak Kreiselian Platonism

Isaacson's Kreisel represents a version of informal rigour which feeds into a quite anthropocentric characterisation of the notion of structure. On his characterisation, informal rigour concerning the concepts employed in a discourse is constitutive of establishing the relevant structure in question.

Instead, we might have a more platonistic conception of informal rigour. One might instead hold that structures are objects of some kind, and there are many (mind-independent) abstract concepts we can employ in talking about those structures.

Given a discourse D and employment of a concept C_0 underlying this discourse,¹⁷ informal rigour on this picture consists of a successive narrowing down and improvement of the concept C_0 . If C_0 does not already determine some particular structure S , this may then necessitate moving to a sharper concept C_1 to underwrite D . Once we have become sufficiently informally rigorous about the concept underlying D (this might take several iterations of conceptual refinement) and have pinned down some independent structure S with some concept C_2 , we are then able to provide our categorical axiomatisation T corresponding to C_2 .¹⁸

In many ways, at a practical level, the Weak Kreiselian Platonist and Isaacson's Kreisel have much in common. They both think that mathematics depends in some way on us, the Weak Kreiselian Platonist because the ways we refine our concepts are presumably dependent upon us (even though they may be constrained), and Kreisel's Isaacson because mathematical structures are determined by our activity. They differ in that the Kreiselian Platonist thinks that the structures we talk about, and plausibly the concepts employ, are independent of us and informal rigour allows us to make a selection between them. Isaacson's Kreisel, on the other hand, thinks that the structures are determined by us, rather than discovered.

¹⁷Of course, there may be more than one concept involved, in which case we might have to consider a concepts C_0, \dots, C_α instead. I suppress this complication; nothing in my arguments hangs on there being just one concept or many.

¹⁸One question, that we shall leave as an open question at the end of the paper, is how we should understand this process of conceptual refinement. For example: Do the concepts stay the same, or do they change when we refine our concepts? For the purposes of discussing informal rigour and whether or not CH is determinate, I'm not sure this matters so much, but for the future development of set theory (and mathematics more generally) we might wonder how conceptual refinement figures in debates about, for example, the temporal continuity of subject matter in mathematics. I am grateful to Chris Scambler for many hours of interesting discussion here.

2.3 Strong Kreiselian Platonism¹⁹

There is a stronger version of Kreiselian Platonism. The key additional assumption is the following:

(Set-Theoretic Uniqueness) There is one and only one correct concept C for discourse that is sufficiently ‘set-like’ (i.e. concerns extensional objects), and it is possible for us to have informal rigour about C . Informal rigour should be understood as a way of approximating C ever more closely.

So, for the Strong Kreiselian Platonist, it is not only the case that we may refine concepts in coming to be informally rigorous but also that we tend towards exactly one such way of filling out the concept in the case of set theory.

We then have three figures; Isaacson’s Kreisel, the Weak Kreiselian Platonist, and the Strong Kreiselian Platonist. We shall argue that for Isaacson’s Kreisel and the Weak Kreiselian Platonist, the status of the informal rigour of the universe of sets (and in particular the Continuum Hypothesis) is questionable. The Strong Kreiselian Platonist can hold on to the full informal rigour of the set concept up to a certain level, but we will argue that their position has some implausible consequences (even if it is strictly speaking coherent).

3 Structural relativity

We are now at a point where we have said a little more about how we might fill out an account of informal rigour, and provided some possible philosophical interpretations of the notion. For the purposes of our arguments in §4 and interpreting our own set-theoretic discourse, it will be useful to set up the idea of *structural relativity*.

Structural relativity is the idea that the structure isolated by a particular piece of mathematical discourse is contingent upon the logic used to underwrite it. It is discussed explicitly by [Resnik, 1997]:

In thinking about formulating a theory of structures we must take into account a phenomenon I will call structural relativity, the structures we can discern and describe are a function of the background devices we have available

¹⁹I am grateful to Leon Horsten for suggesting this interpretation, and Daniel Kuby for some additional discussion led me to realise that I also needed to consider the weaker form of Kreiselian Platonism as discussed in the last subsection.

for depicting structures...This relativity arises whether we think of patterns and structures as a kind of mould, format, or stencil for producing instances, or as whatever remains invariant when we apply a certain kind of transformation, or as an equivalence class or type associated with some equivalence relation. The structures we recognize will be relative to our devices for specifying forms, or transformations or equivalence relations. ([Resnik, 1997], p. 250)

The idea then for Resnik, is that the kind of structures we can talk about can vary contingent upon the logical resources we employ. For the same mathematical discourse D , we might pick many different formal theories to underwrite it, and many different kinds of structure might be isolated by these different concepts. For example, he writes:

If we limit ourselves to describing structures as the models of various first-order schemata, then the types of structures we will define will be like the more coarse-grained ones frequently found in abstract algebra. Here one starts by defining a type of structure such as a group, a ring, or a lattice with the intention of allowing for many non-isomorphic examples of the same type. As a result most of our structural descriptions will fail to be categorical. On the other hand, using second-order schemata, we can formulate categorical descriptions of the structures studied by (second-order) number theory, Euclidean geometry and analysis, and categorical extensions of $[ZFC_2]$ that are considered powerful enough for most mathematical needs.

Thus, depending upon our logical resources, we might introduce:

The First-Order Natural Number Structure,

The Second-Order Natural Number Structure,

The First-Order Structure of the Reals,

The Second-Order Structure of the Reals,

and so on.

By going to stronger logics we get more fine grained versions of the various structures. ([Resnik, 1997], p. 252)

So, for example, we can consider our talk about natural numbers as either formalised in first-order Peano Arithmetic (PA), or in second-order Peano Arithmetic (PA_2). The latter axiomatisation corresponds

(on the usual understanding) to the particular structure of the standard model of natural numbers, the former on the other hand is a general structure that is true both on the standard model (where, presumably, $Con(\mathbf{PA})$ holds), but also can be true in non-isomorphic non-standard models (where, for example, $\neg Con(\mathbf{PA})$ can hold).

The above passage is fairly indicative of what seems to be a (false) dichotomy underlying parts of the literature; we are presented with the choice between either using first-order resources (where almost nothing is categorical, only finite structures) or full second-order resources (where an enormous amount of our mathematical talk is fully categorical).²⁰ This dichotomy does not adequately reflect the fact that in mathematical logic we have a wide range of logics intermediate between first-order and second-order. The properties of these logics are well-understood²¹, and it is surprising that they have not been considered in detail in the context of structural relativity. This is not to say that authors (including Resnik) intend this false dichotomy, just that largely speaking in the structuralist literature these are the two options proffered.

Admitting intermediate logics into interpretations of structural relativity opens up a host of possibilities. Once we free ourselves of the binary choice between first- and second-order resources, we have the option of considering many different formal theories for underwriting a discourse. There is a wide variety of options here, including increasing our resources beyond first-order with certain operators (e.g. ancestral logic) or alternatively allowing infinitary conjunctions or quantifier alternations. Since we will be interested here in theories that we can *use* in manifesting informal rigour, we set aside the use of infinitary resources. In the next section, we shall see how versions of set theory incorporating structural relativity given by weak second-order logic and quasi-weak second-order logic correspond to two natural positions about of informal rigour concerning the cumulative hierarchy.

²⁰Isaacson, for example, writes:

As Shapiro and others have long noted, the language in which to articulate our understanding of particular mathematical structures is second-order... ([Isaacson, 2011], p.28)

²¹See, for example, [Shapiro, 1991] (Ch. 9) or [Shapiro, 2001].

4 The concept of set, degrees of informal rigour, and structural relativity

We are now in a position where:

- (1.) Informal rigour in the concepts underlying a discourse is manifested by axiomatisations that are categorical.
- (2.) We have three different ways of interpreting informal rigour, via Isaacson's Kreisel, Weak Kreiselian Platonism, and Strong Kreiselian Platonism.
- (3.) Structural relativity may come in to play, whereby the kinds of structures we isolate are contingent upon the background logic we use.

In this section, I'll consider some examples that show how we might not be fully informally rigorous about our set-theoretic discourse and set concept. I'll then argue that there should be a degree of structural relativity involved in the axiomatisation of our thought concerning sets. Nonetheless, I shall argue that we are (and have been) *partially* informally rigorous, and our discourse about *portions* of the hierarchy can be understood as about particular structures. To do this, I'll look historically at Mirimanoff's thought concerning the Axiom of Foundation, before considering our own axiomatisation of set theory in terms of ZFC and our possible attitudes to CH.

In order to make out my conclusions, it will be useful first to analyse in a little more detail what we might expect from an account of informal rigour. Important for Kreisel's notion is that our concept of set, and the informal rigour we have about it, is a *source* for axioms. He writes:

What one means here is that the intuitive notion of the cumulative type structure provides a coherent *source* of axioms; our understanding is sufficient to avoid an endless string of ambiguities to be resolved by further basic distinctions...²² ([Kreisel, 1967], p. 144)

Isaacson agrees, at least insofar as interpretation of Kreisel goes:

²²Kreisel continues: "...like the distinction above between abstract properties and sets of something.", speaking about the distinction between intensional entities and sets (this intensionality he seems to diagnose as the source of the class-theoretic paradoxes). Since this diagnosis is rather controversial, I'll set it aside here.

In order actually to solve the continuum problem a formalizable derivation from axioms, of the kind which Cohen and Gödel's results show not to exist from the first-order axioms of ZF, must be found. This means that new axioms are required. ([Isaacson, 2011], p. 16)

My point is the following: If we are informally rigorous about a discourse D and the concepts underlying it, and hence have determined a particular structure, we can expect the use of these concepts as a "coherent source of axioms" not to lead us in radically different directions. Of course, it is possible to have beliefs about a structure that turn out to be false (as when I believe an eventually false conjecture), but it should not be the case that radically different concepts, with radically different theories and consequences are legitimate ways of refining our current concepts. We therefore identify the following:

(Modal Definiteness Assumption.) (MDA) If we are informally rigorous about a mathematical discourse D , using a concept C_0 , then there should not be two (or more) legitimate ways of refining C_0 (to some C_1 and C_2) such that C_1 motivates a theory T_1 and C_2 motivates a theory T_2 such that T_1 and T_2 are inconsistent with one another.²³

The Modal Definiteness Assumption is highly plausible. If our discourse and concepts already determine a particular structure (via informal rigour) then there should not be equally legitimate ways of sharpening our concepts that are inconsistent with one another, since the truth value of all claims in the discourse are already set by this structure. Therefore one of the two theories has to be false, and one of the two concept-schemes is not legitimate.²⁴ Of course, what constitutes a 'legitimate' extension is going to be something of debate. I hope the examples I provide from the philosophy of set theory will make it clear that there are such sharpenings, and hence by MDA we are not informally rigorous about our concept of set. However, let us first see how the MDA might play out in a positive case where we *do* take ourselves to have informal rigour.

²³Many thanks to Daniela Schuster for pressing me to become clearer about my formulation of the MDA.

²⁴If you're familiar with debates in the philosophy of set theory, you might already see where I'm going here.

4.1 The Radical Relativist

Suppose we believe that our discourse about the natural numbers, underwritten by our concept of natural number, is informally rigorous and this informal rigour is manifested by PA_2 and the attendant Dedekind-categoricity theorem. Along comes the Radical Relativist who says to us: You cannot be informally rigorous about arithmetic, since there are legitimate consistent extensions $\text{PA}_2 + \text{Con}(\text{PA}_2)$ and $\text{PA}_2 + \neg\text{Con}(\text{PA}_2)$ of PA that are inconsistent with one another. What should be our reaction?

Our response should be the following: Of course these extensions are consistent, but one is clearly legitimate where the other is not. In particular, $\text{PA}_2 + \neg\text{Con}(\text{PA}_2)$ can only be true in models that are non-standard, not only in their interpretation of the second-order variables, but also have consequences that do not accord with our concept of natural number. For example, we can see that such models have numbers n^* , such that for any particular standard natural number n given to me, n^* is greater than n . So it is simply not true that $\text{PA}_2 + \text{Con}(\text{PA}_2)$ and $\text{PA}_2 + \neg\text{Con}(\text{PA}_2)$ are both legitimate extensions of PA_2 , at least insofar as axiomatising our concepts and thought concerning the particular structure of natural numbers is concerned.

Moreover, there is no categoricity theorem for the theory $\text{PA}_2 + \neg\text{Con}(\text{PA}_2)$, and indeed it can have highly non-isomorphic models. In fact, there are continuum-many distinct consistent completions of PA_2 , and hence continuum-many countable models of PA_2 .²⁵ So there can be no categoricity theorem for this theory, and hence no informal rigour.

This will provide a contrast case for our main two examples; examining the historical situation with respect to Mirimanoff and the Axiom of Foundation, and our contemporary situation with respect to set theory, and CH. First, however, I want to consider a case that is not quite as extreme as the Radical Relativist, on which the natural numbers are determinate, but nonetheless we may have worries about our informal rigour concerning the iterative conception of set.

4.2 The Recursive Iterabilist

We now consider a slightly different situation, one in which we have agents whose thought is best axiomatised by a version of set theory

²⁵This follows just by taking a Henkin interpretation of the second-order quantifiers of PA_2 over a countable model of any consistent completion of PA . See [Kaye, 1991] for an extensive treatment of non-standard models of PA . For the time-pressed reader [Hamkins, 2012a] provides a quick way of seeing the result for PA .

intermediate between first and second-order ZFC. This is not the main focus of our points (we are more concerned with the case of CH), but it is an interesting possibility and serves as a warm-up.

Suppose that one accepts first-order logic and the the iterative conception as a conceptual *idea*, and hence regards ZFC as a (probably) consistent theory worthy of study, but has extreme reservations about informal rigour concerning the notions of arbitrary subset and arbitrary well-order. Instead, one thinks that we can only be informally rigorous about things that are *recursively* defined, and one thinks that it's possible that our thinking might not be informally rigorous and fail to determine particular structures at large infinite ordinals. Call this character the *Recursive Iterabilist*.

To make our points (here and later) we first need to set up some terminology. Two background logics will be of special interest for us:²⁶

Definition. *Weak second-order logic* is the logic in which we allow the same vocabulary as second-order logic \mathcal{L}_K^2 (where K are the non-logical symbols) but with function variables removed.

Its semantics is given by letting the second-order quantifiers range over *finite* relations. Let \mathfrak{M} be a model with domain M . We define a *finite assignment* s on \mathfrak{M} as assignment s that assigns a member of M to each first-order variable, and a finite n -place relation on M to each n -place relation variable. Satisfaction is defined in the usual manner for the first-order connectives and quantifiers, and second-order quantification is handled by the clause:

$$\mathfrak{M}, s \models \forall X \phi \text{ iff for every finite assignment } s' \text{ that agrees with } s \text{ (except possibly at } X), \mathfrak{M}, s' \models \phi.$$

The instances of comprehension $\exists X \forall y (X(y) \leftrightarrow \phi(y))$ which are valid on a structure \mathfrak{M} are those where the extension of ϕ is finite in \mathfrak{M} .

ZFC_{2W} is the theory ZFC₂ with function variables removed from the vocabulary and the underlying semantics given by weak second-order logic.

²⁶The presentations given here are heavily indebted to Stewart Shapiro's [Shapiro, 2001].

Definition. *Quasi-Weak Second-Order Logic* has the same formulas as full second-order logic, but in the semantics each variable assignment assigns countable relations to the variables (this is similar to weak second-order logic, where we allow countable relations instead of finite ones). So $\forall X \phi$ holds iff for all countable X , ϕ holds.

Let \mathbf{ZFC}_{2QW} be set theory formulated in quasi-weak second-order logic with instances of the replacement scheme for each formula of the quasi-weak second-order language.

It is useful to identify some facts off the bat:²⁷

Fact. Both \mathbf{ZFC}_{2QW} and \mathbf{ZFC}_{2W} are able to characterise categorically the natural numbers (i.e. any two models of \mathbf{ZFC}_{2QW} and \mathbf{ZFC}_{2W} always have the standard natural numbers as their standard model of arithmetic, and indeed any two models of \mathbf{PA}_2 with the standard semantics within a model of \mathbf{ZFC}_{2QW} or \mathbf{ZFC}_{2W} are isomorphic).²⁸ The same goes for the rational numbers.²⁹

Fact. \mathbf{ZFC}_{2QW} is able to characterise the theory of real analysis up to isomorphism³⁰, \mathbf{ZFC}_{2W} cannot.³¹

Fact. \mathbf{ZFC}_{2QW} is able to characterise the notion of well-foundedness, that is, all models of \mathbf{ZFC}_{2QW} are well-founded.³²

Fact. \mathbf{ZFC}_{2W} is *not* able to characterise the notion of well-foundedness (i.e. there are models of \mathbf{ZFC}_{2W} with a non-well-founded membership

²⁷See [Shapiro, 2001] for discussion of these results.

²⁸This is because we can characterise the notion of finiteness in both Quasi-Weak and Weak Second-Order Logic. See [Shapiro, 2001], p. 161, and Theorem 16 and Corollary 17 on p. 162.

²⁹This is because we can characterise the notion of *minimal closure* in the two logics, and the rational numbers can be characterised up to isomorphism as an infinite field arising from the minimal closure of $\{1\}$ under the field operations and their inverses. See [Shapiro, 2001], p. 161.

³⁰This is because we can characterise the completeness principle for the reals in \mathbf{ZFC}_{2QW} . See [Shapiro, 1991], pp. 164–165.

³¹Since the Löwenheim number of Weak Second-Order Logic is \aleph_0 . See [Shapiro, 2001], pp. 161–162.

³²Assuming Choice in the meta-theory, the fact that every countable class is a set in a model of \mathbf{ZFC}_{2QW} ensures this. See [Shapiro, 1991], p. 165.

relation).³³

So, we have several logics and versions of **ZFC**-like set theory rendered in them in view. Now, the Recursive Iterabilist will hold that we are informally rigorous about the natural numbers and rational numbers, but have grave worries about our informal rigour concerning the iterative conception in general.³⁴ In this case, we might think that our thought about **ZFC**-based set theory and the concept of cumulative type structure is best axiomatised by \mathbf{ZFC}_{2W} . There we are able to identify the rational and natural numbers up to isomorphism, but the real numbers cannot be so identified, and non-recursive well-orderings (e.g. ω_1^{CK}) can not be characterised up to isomorphism.³⁵

If you are a Recursive Iterabilist, you are thus likely to hold that our talk about sets is only *partially* informally rigorous, and this level of partial informal rigour is manifested in \mathbf{ZFC}_{2W} . We thus have a coherent position on which a level of informal rigour is manifested in a logic stronger than first-order but weaker than second-order.

Of course the natural point to make here is that this is just a case where the Recursive Iterabilist and the believer in the CH-Determinateness assumption have a clash of intuitions. There is nothing in the example as I have presented it that might convince someone who believes in the CH-Determinateness Assumption on the basis of the quasi-categoricity of \mathbf{ZFC}_2 to re-evaluate their position. We will discuss the place of the quasi-categoricity theorem later (§5), for now we just note that the example as presented shows that we can have a coherent position on which our reasoning is axiomatised by a set theory couched in a logic intermediate between first- and second-order.

4.3 Mirmanoff's Informal Rigour

The following example will show that there are plausibly actual examples in which agents were not informally rigorous about set theory (and, at the very least, such examples are possible). However, we might nonetheless think that substantial parts of mathematics were informally rigorous, and as such we had partial informal rigour in the notion of set. We'll see, however, that the example is more analogous to PP than CH (the latter we consider in §4.4).

³³This is because there is a natural equivalence between being a model of \mathbf{ZFC}_{2W} and being an ω -model of **ZFC** (see [Shapiro, 1991], p. 162, Corollary 17) and there are non-well-founded ω -models of **ZFC**.

³⁴Solomon Feferman's views (for example in [Feferman et al., 2000]) are not far from this position.

³⁵See here [Shapiro, 1991], p. 163.

In 1917, Dimitry Mirimanoff wrote a paper entitled ‘Les antinomies de Russell et de Burali-Forti et le problème fondamental de la théorie des ensembles’. In this paper, he considers Russell’s Paradox and the Burali-Forti Paradox, and identifies two kinds of sets; the ‘ordinary’ ones and the ‘extraordinary’ ones. These were to be differentiated by whether or not they contain infinite descending sequences of membership; the ordinary ones do not (in current terminology: they have a well-founded membership relation) and the extraordinary ones do (in current terminology: they have a non-well-founded membership relation):

I will say that a set is *ordinary* just in case it gives rise to finite descents, I will say that it is *extraordinary* when among its descents are some that are infinite. ([Mirimanoff, 1917], p. 42, my translation)³⁶

It is clear that Mirimanoff (in [Mirimanoff, 1917]) was undecided about whether the Axiom of Foundation was a basic principle about sets. It is also fairly clear, we think, that he was *not* fully informally rigorous about set theory. To see this, it suffices to consider what theory might have underwritten his thinking about sets, and show that there are different legitimate extensions that are inconsistent with one another.

Clearly Mirimanoff thought that sets were extensional and he explicitly discusses the axioms of pairing and union, as well as replacement. For the purposes of our discussion, let us assume that he was clear that his notion of set supported at least the first-order axioms of ZF without the Axiom of Foundation. (It doesn’t matter so much whether or not these were *actually* Mirimanoff’s views, as long as this character is at least possible it shows the kinds of situations that are compatible with informal rigour in set theory.)

Can Mirimanoff’s level of informal rigour support more? Is he informally rigorous about the Axiom of Foundation? We answer this negatively using the Modal Definiteness Assumption. We argue that there are legitimate extensions of Mirimanoff’s concept that support inconsistent theories of sets (namely ZF and ZF+“There are non-well-founded sets.”). Clearly the former is a legitimate extension, since it is what we (as a matter of fact) use now on the basis of our concept

³⁶The original French reads:

Je dirai qu’un ensemble est *ordinaire* lorsqu’il ne donne lieu qu’à des descentes finies; je dirai qu’il est *extraordinaire* lorsque parmi ses descentes il y en a qui sont infinies. ([Mirimanoff, 1917], p. 42)

of cumulative type structure . Is the latter a legitimate extension of ZF-Foundation? One might be tempted to answer no: The iterative conception of set clearly prohibits the existence of non-well-founded sets.

The iterative conception is emphatically *not* Mirimanoff’s conception of set, however. Whilst he has the concept of ordinal and rank in play³⁷, it is not really until Zermelo (in [Zermelo, 1930]) that we start to see the idea of cumulative type structure emerge, solidified in Gödel’s work on L (in [Gödel, 1940]), and it was not until the late 1960s and 1970s that the idea of the iterative conception and its relation to ZFC were fully isolated.³⁸ The following situation is then possible: Suppose that instead of the iterative conception becoming the default conception of set, the graph conception of set (on which sets are viewed as kinds of directed graphs) became our the default set-theoretic conception (let’s say this was motivated by considerations about non-well-founded sets emerging in computer science).³⁹ Then, it seems reasonable to accept that Mirimanoff’s intellectual descendants would have accepted that there were non-well-founded sets. By the MDA, he can’t then have been fully informally rigorous, since there are inconsistent ways of extending the concept he was employing about his discourse.

It is then tempting to say that Mirimanoff’s thinking might be best captured by *first-order ZF* without Foundation. We should resist this temptation. For Mirimanoff’s context is plausibly one in which he was informally rigorous about what the natural numbers were, and indeed his work comes after Dedekind’s categoricity proof (in [Dedekind, 1888]). In particular, his definition of well-foundedness depends on the notion of finiteness; he characterises well-founded sets as those which only have finite descending membership chains, rather than using the contemporary first-order statement of the Axiom of Foundation in terms of the claim that every non-empty set A contains a set B such that $A \cap B$ is empty. But, by the Compactness Theorem, finiteness cannot be characterised using first-order logic, nor can the natural numbers.⁴⁰ It is overwhelmingly likely that he would have not accepted non-standard models of arithmetic as legitimate interpretations in the same sense as

³⁷The notion of ordinal recurs throughout his discussion of the Burali-Forti Paradox, and he discusses the notion of rank on p. 51 of [Mirimanoff, 1917].

³⁸In [Boolos, 1971], for example. See [Kanamori, 1996] for a thorough discussion of the history.

³⁹See here [Incurvati, 2014] for a description of the graph conception.

⁴⁰In fact, being able to capture these two notions is roughly equivalent, since “ x is finite” can be parsed in terms of being bijective with a standard natural number, and “ x is standard natural number” can be parsed as being a finite successor-distance away from 0. See [Shapiro, 2001] (p. 155) for the details.

his own.

Since Mirimanoff was also well aware that arithmetic could be coded in set theory, we are at a point where we would like to say that his discourse about parts of set theory such as the natural numbers and finite sets *are* informally rigorous and determined a particular structure. It is also plausible that he was informally rigorous around 1915 about the notion of real number, by this stage he was working on the intellectual foundations that had already been laid by Cauchy, Weirstrass, Cantor, and Dedekind, and the categoricity of the real line had been proved. However, by the MDA, his discourse about set theory in general was not informally rigorous. Thus, if we are to provide an axiomatisation for underwriting his discourse and concept of set, we should use a theory and logic that is not fully categorical, but nonetheless can identify parts of set theory up to isomorphism.

What should we say about Mirimanoff's level of informal rigour? Well, to review:

- (1.) His concept of set did not clearly support the Axiom of Foundation.
- (2.) It is highly plausible that he was informally rigorous about the natural numbers and the real numbers.
- (3.) It is highly plausible that he was informally rigorous about the concept of well-order (being able to distinguish and talk about the extraordinary and ordinary sets).

We can then say that Mirimanoff's level of informal rigour about set theory can be roughly characterised by \mathbf{ZFC}_{2QW} –Foundation (i.e. \mathbf{ZFC}_{2QW} with the Axiom of Foundation removed). There, we can characterise the usual objects of mathematics including the real, rational, and natural numbers (since the categorical characterisations of these theories do not depend on the Axiom of Foundation). Moreover, he can formulate and discuss his worries about well-foundedness in this logic. However, he is not fully informally rigorous, since there are incompatible legitimate expansions of the concept concept he was working with (namely to one supporting the foundation axiom and to one supporting its negation).

We should remark though that Mirimanoff's situation is more like the situation we have with the Axiom of Parallels in geometry, rather than what we have in \mathbf{ZFC}_2 . This is because there is no categoricity proof for \mathbf{ZFC}_2 –Foundation as there are models of \mathbf{ZFC}_2 –Foundation in which Foundation holds and others in which it fails. So whilst our example shows that there might have been a case where we failed to

be informally rigorous about our notion of set, it does not yet show the possibility of a situation where *we* are not, where *we* have the iterative conception of set.

4.4 Informal rigour and the Continuum Hypothesis

So then: What now about our own thought concerning the Continuum Hypothesis? My contention is that, given the Modal Definiteness Assumption, we have good reason to think that we are *not* fully informally rigorous about our concept of set. To see this, it is useful to consider two active programs targeting the resolution of CH in the contemporary foundations of set theory, namely *forcing axioms* and Woodin's *Ultimate-L* programme.

We omit the details here, since they are technically rather tricky, and many questions are still open. Both kinds of programme attempt to incorporate notions of 'maximality'; *Ultimate-L* by incorporating large cardinals in an elegant manner, and forcing axioms by ensuring that various kinds of subset exist. Both represent somewhat different takes on how our concept of set may develop; *Ultimate-L* is trying to get at the idea that the universe should be (in a precise sense) 'orderly'⁴¹, and forcing axioms capture the idea that the universe is saturated under the formation of certain kinds of set⁴².

Crucially, if we take the *Ultimate-L* approach, we can prove CH, and strong forcing axioms (e.g. the Proper Forcing Axiom or PFA) imply \neg CH. They therefore represent inconsistent extensions of our current best theory of sets. They are also both seem legitimate; both correspond to natural ways we might develop our set concept.

Given the MDA, it seems then that we are not fully informally rigorous about our concept of set. It is also plausible, however, that we have a good deal of informal rigour. We seem to have informal rigour about the natural numbers, where the only known independent statements are all equivalent to consistency statements, and the negation of these are illegitimate extensions (assuming that we think the axioms really are consistent). In the case of analysis there are no obvious analogues of CH for second-order arithmetic; in the presence of Projective Determinacy there are no known sentences of ZFC independent from the theory $\text{ZFC} - \text{Powerset} + V = H(\omega_1)$ (other than Gödelian-style diagonal sentences). Both *Ultimate-L* and forcing axioms agree that Projective Determinacy is true. It also seems clear that our concept of cumulative

⁴¹Namely that it is "*L*-like" whilst being able to tolerate consistent large cardinal axioms.

⁴²Namely generics for certain partial orders and families of dense sets.

hierarchy supports the idea that we are informally rigorous about the claim that all sets are well-founded.

Given this, it seems that our level of informal rigour in the cumulative hierarchy of sets might be manifested by \mathbf{ZFC}_{2QW} . In this theory, we are able to define the natural numbers and reals up to isomorphism (and hence have particular set-theoretic structures corresponding to these), and all interpretations of our thought are well-founded. However we can also point out.

Fact. There are models of \mathbf{ZFC}_{2QW} in which CH holds, and models of \mathbf{ZFC}_{2QW} in which CH fails.⁴³

Thus, given the MDA and the Manifestation Thesis, we might think that our current level of informal rigour is manifested by \mathbf{ZFC}_{2QW} ; a logic intermediate between first- and second-order.

5 Objections and replies

In this section I'll consider some objections and replies. These will not only help to shore up my position, but also will help to see some features of the account.

Objection. *What about the Zermelo-Shepherdson Categoricity Theorem?* One question for the arguments I have posed is immediate: What becomes of the the Zermelo-Shepherdson Quasi-Categoricity Theorem? One might think that the theorem shows that our thought about the

⁴³I am grateful to Victoria Gitman for working with me on the following proof:

Proof. Start in a model $\mathfrak{M} \models \mathbf{ZFC} + \neg\text{CH}$ (by preparatory forcing if necessary). Next collapse $|\mathcal{P}^{\mathfrak{M}}(\omega)|$ to ω_1 using the forcing poset $\text{Col}(\omega_1, \mathcal{P}(\omega))$ in \mathfrak{M} . By design, $\mathfrak{M}[G] \models \text{CH}$. But $\mathfrak{M}[G]$ also has the same countable relations as \mathfrak{M} , since it is a standard fact about $\text{Col}(\omega_1, \mathcal{P}(\omega))$ that it is countably closed. (If a countable relation R were added, one can look at the countably many conditions $p_n \in \text{Col}(\omega_1, \mathcal{P}(\omega))$ forcing that $\dot{x} \in \dot{R}$, and (by countable closure) infer that R was already in \mathfrak{M} .) Thus \mathfrak{M} and $\mathfrak{M}[G]$:

- (i) Have the same countable relations on sets in V (for this reason \mathfrak{M} and $\mathfrak{M}[G]$ have the same reals).
- (ii) Differ on the truth value of CH.

Hence $\mathfrak{M}[G]$ thinks that both \mathfrak{M} and $\mathfrak{M}[G]$ satisfy \mathbf{ZFC}_{2QW} (since, according to $\mathfrak{M}[G]$, \mathfrak{M} has all its countable relations) but differ on CH. Hence CH is not fixed by \mathbf{ZFC}_{2QW} . \square

sets is informally rigorous and determines some particular structures (for example those with a specific number of inaccessible cardinals). Earlier, I claimed that it is plausible that there are extensions of our current set concept that support Ultimate- L and others that support forcing axioms (let's take PFA to make things concrete). I then claimed on the basis of the MDA that our set-theoretic discourse and concepts were not informally rigorous. But this is not so (so one might counter-argue) whilst both PFA and Ultimate- L are (we assume) consistent with \mathbf{ZFC}_2 , only one of them can be true under \mathbf{ZFC}_2 with the standard semantics, the other will require a Henkin-style interpretation to make both it and \mathbf{ZFC}_2 true. So it is just not correct to say that both are legitimate; the concept that motivates a theory that is false under the standard semantics requires non-standardness of a certain kind (albeit not as serious as the one required for e.g. $\neg \text{Con}(\mathbf{ZFC}_2)$).

The issue here is that this objection assumes that we have access to the range of the second-order variables in making the criticism. We *already* need to be informal rigorous about the range of second-order variables if we are to hold that \mathbf{ZFC}_2 is a good encoding of our level of informal rigour. Similar points have been repeatedly stressed throughout the literature⁴⁴, but it is particularly relevant to the current context; a categoricity theorem is meant to encode informal rigour that we have about a certain subject matter, not *give* us informal rigour. If there is no informal rigour (which I've argued on the basis of the Modal Definiteness Assumption) it is not necessary for us to accept that the categoricity theorem yields genuine clarity.⁴⁵

It is instructive here to consider our different interpretations of informal rigour. Isaacson's Kreisel should accept (contrary to what Isaacson claims) that there are different legitimate extensions of our concept of set. This is because for Isaacson's Kreisel, informal rigour is dependent upon the degree to which we have understood a mathematical subject matter. If we expand our concept of set C_0 to one C_1 producing

⁴⁴See [Meadows, 2013] for a survey. Hamkins is also explicit about the point when discussing a version of the categoricity argument in [Martin, 2001]:

The multiversist objects to Martin's presumption that we are able to compare the two set concepts in a coherent way. Which set concept are we using when undertaking the comparison? ([Hamkins, 2012b], p. 427)

⁴⁵A different move here would be to shift to *internal* categoricity (an argument of this form is presented by [Button and Walsh, 2018]). If one buys the MDA, one will be forced to accept *some* indeterminacy, for internal categoricity this may be in either the range of the first-order quantifiers or in the use of classical logic. See [Scambler, S] for discussion of this issue.

a consistent axiomatisation (as, let's assume, both Ultimate- L and PFA do) our *understanding* should be cashed out in terms of this new concept C_1 , and this determines (*given* that we are employing C_1) a subject matter that supports either PFA or Ultimate- L , depending on which route we pick. Given then that for Isaacson's Kreisel the subject matter we talk about is determined by the concepts we employ, he should accept that we are able to go in different possible directions with our concept, and thus that we are currently not informally rigorous; our set-theoretic discourse is ambiguous between several different sharpenings of the notion.

For exactly the same reason, the Weak Kreiselian Platonist should accept that we are not fully informally rigorous about our concept of set. Recall that for her, informal rigour should be understood as coming to employ ever more platonistically existing precise concepts of set. But for this reason, it's entirely possible that we select one concept that supports PFA in the future and also possible that we select one that supports Ultimate- L . In this way, our thinking might be currently *ambiguous* between several different sharpenings of the concept.

The only person who can argue that the quasi-categoricity theorem in fact shows that ZFC_2 encodes our level of informal rigour is the Strong Kreiselian Platonist. They hold that there is a *unique* correct concept that we are tending towards using informal rigour. This concept can then serve to interpret the second-order variables, given that ZFC_2 is already quasi-categorical. Therefore (they claim) the case as I've set things up is not possible; *one* of PFA and Ultimate- L (or neither) is correct about this concept, and the process of informal rigour will lead us towards it. Therefore, exactly one or neither of PFA and Ultimate- L is legitimate, and it is just not possible to legitimately expand our concept in incompatible ways. Hence, even given the MDA, we have informal rigour, there are just not incompatible extensions of our concept.

This represents a coherent position, but not one that I find very plausible. The Strong Kreiselian Platonist has to accept that we simply *could not* coherently follow a different intellectual path from the one we have. But this is an enormously strong claim! What about cases where the kinds of modelling requirements we encounter are very different? Suppose, for example, that there are two physically (or even metaphysically) possible worlds at which the modelling requirements for foundations are very different, and one suggests Ultimate- L where the other suggests PFA. Should we insist there that the agents at that world are doing something illegitimate if they select the 'wrong' concept of set to work with? It seems to me that the agents in the two

different cases simply employ different concepts, and use them to talk about different subject matters. But this is not an analysis available to the Strong Kreselian Platonist, she has to either accept that there is a fundamentally 'correct' interpretation for the second-order variables, and the thinking of one of the two communities' thinking is quite simply flawed, or has to deny that such a situation is really possible.

The situation can be made more vivid by a kind of pessimistic probabilistic argument. Assume that we do have a fully determinate interpretation of ZFC_2 . Notice that it might be that in fact *both* Ultimate-L and PFA are false in their full generality, even if one is correct about the status of CH. In fact, there are myriad different ways we might develop our set-theoretic axiomatisation, so why should we expect the one we pick to be right? Our understanding of the *Generalised Continuum Hypothesis* tells us that we can consistently have pretty much whatever pattern we like for the cardinal behaviour of infinite powersets, as well as a whole gamut of set-theoretic principles. So, if we believe that there really is a fully determinate ZFC_2 model below the first inaccessible, it is overwhelmingly unlikely (without further argument) that we pick *exactly* the right axiomatisation, and it is *we* who are saying false things, and can only be interpreted as speaking consistently about non-standard Henkin interpretations.

Perhaps the steadfast believer in full informal rigour concerning the cumulative hierarchy will dig her heels in at this point and say that either we are (a) unlucky and fatally flawed, or (b) either very lucky and/or there is some magic principle that will lead us to the choice of correct axiomatisation. There is certainly no contradiction in taking this stance, however (a) is not particularly useful for developing mathematical axiomatisations (we will likely go off on a fundamentally incorrect road), and (b) immediately raises the question of exactly how we could determine whether or not one of the multitude of seemingly legitimate axiomatisations is correct. Here is what I think is a more plausible take on the matter: Whilst we are not *yet* informally rigorous about our concept of set, we might be in the future. Just look, for example, at the progress that has been made in the hundred years or so since Mirimanoff was writing: our concept of set *clearly* now underwrites the claim that all sets are well-founded. Perhaps in the future we will come to a fully informally rigorous conception of set, however if we do so we should acknowledge that it is not the case that things *had* to be this way.

We can still accept some implications for the quasi-categoricity theorem even given this picture. For, the quasi-categoricity theorem establishes that *given* an interpretation of the second-order variables, a

particular structure is identified by ZFC_2 (with some specific bound on the inaccessibles).⁴⁶ We might think that this fact has philosophical import. Meadows (in [Meadows, 2013]) identifies three roles for a categoricity theorem:

- (1.) to demonstrate that there is a unique structure which corresponds to some mathematical intuition or practice;
- (2.) to demonstrate that a theory picks out a unique structure; and
- (3.) to classify different types of theory. ([Meadows, 2013], p. 526)

He is sceptical about the possibility of (1.) for similar reasons to those I have presented here: The categoricity theorem presupposes the determinacy of the notions it is trying to characterise. However, this is precisely what informal rigour is meant to do; *given* that we have convinced ourselves of informal rigour, the categoricity theorem tells us that our axiomatisation of this notion has been successful. (2.) is thus important; once we believe we have informal rigour, we need to provide a categorical characterisation to manifest this informal rigour (and ensure that the clarity is genuine). I have argued that for set theory, we are not quite there. However, (3.) is important whether or not we actually have informal rigour. The categoricity theorem for ZFC_2 , no matter whether or not we are precise about exactly what structure it is about, *does* tell us that set theory is non-algebraic. It tells us that our thought at least *aims* at specifying a particular structure, and hence is *not* like concepts and theories of general structure (such as that of *group*) that explicitly aim at dealing with many different non-isomorphic structures. *Inside* every model of ZFC_{2QW} (which I've argued is the most natural theory for representing our thought about sets), the Zermelo-Shepherdson Categoricity Theorem holds and ZFC_2 (with a specific bound on the inaccessibles) is a theory for talking about one isomorphic structure. It is just

⁴⁶Multiversists are often explicit on this point. For example Hamkins writes:

If we make explicit the role of the background set-theoretic context, then the argument appears to reduce to the claim that within any fixed set-theoretic background concept, any set concept that has all the sets agrees with that background concept; and hence any two of them agree with each other. But such a claim seems far from categoricity, should one entertain the idea that there can be different incompatible set-theoretic backgrounds. ([Hamkins, 2012b], p. 427)

that this structure can vary across different models of ZFC_{2QW} . Whilst we are not informally rigorous about set theory, the categoricity theorem shows that this situation is *intolerable*, there is *pressure* to become informally rigorous about set theory, even if we currently lack it. This shows that the Difference Thesis (that the case of PP and CH are fundamentally different from one another) can be retained, even in the face of less than full informal rigour in our set concept.

This observation shows that the distinction between *particular* and *general* structures, whilst not incorrect per se, is rather coarse grained. In particular, the idea of *general structure* further subdivides. First, there are those general structures whose concept does not produce a theory for which there is a categoricity proof (e.g. *group*), and thus there is no pressure to hold that informal rigour requires us to determine a particular structure. Call these *intentionally* general structures. There are other concepts (e.g. *set below the first inaccessible*) where we *do* have an axiomatisation with a categoricity proof, even if we *don't* take ourselves to be informally rigorous. We call these *unintentionally* general structures. For set theory, structure we currently talk about is not particular, but given a refinement of the concept we may determine a particular structure at some future point in time. Moreover, there are *portions* of structures corresponding to this concept that are particular (e.g. the representations of countable structures within models of theories corresponding to our set concept), whereas this is different for intentionally general structures such as the one corresponding to the concept *group*.

Challenge. *How do we know when we reach informal rigour?* In responding to the last objection, I suggested that there are certain concepts (and discourse) about which we are not yet informally rigorous, but there is nonetheless pressure to *become* informally rigorous. This immediately raises the following question: How do we know when we are informally rigorous?

My answer here is a little speculative, but it suggests some interesting directions for future research. We begin with the following idea:

Definition. (Informal and Philosophical) We say that a theory T exhibits a *high-degree of theoretical completeness* when there are no known sentences other than meta-theoretic sentences (e.g. Gödelian diagonal sentences) independent from T .

I acknowledge that this definition is somewhat imprecise. In particular I have no technical account on offer of what is meant by 'meta-

theoretic' statements, and I hope that future philosophical research will clarify this notion further. However, it seems that we have *some* handle on the notion though, there seems to be a sense in which $Con(\mathbf{ZFC})$ is a statement of a very different kind from CH.⁴⁷

Given a handle upon the notion, I have the following suggestion; a good indicator⁴⁸ of informal rigour is the existence of a categoricity theorem for the relevant second-order theory *and* a high-degree of theoretical completeness (i.e. the only known sentences independent from our theory are obviously meta-theoretic in some way). If this is the case, then if we take ourselves to be informally rigorous and in fact all known independent statements are meta-theoretic, then we can't construct the kind of simplistic argument from the MDA that I've considered here; any known candidate independent statement does not correspond to a legitimate extension of the concept by design.⁴⁹

This is precisely our current situation in arithmetic. Moreover, as mentioned earlier, if we accept⁵⁰ Projective Determinacy (which is agreed on by both Ultimate- L and PFA, since they both imply $AD^{L(\mathbb{R})}$)⁵¹ then the same situation holds for analysis; the only sentences about $H(\omega_1)$ that are known to be independent from $\mathbf{ZFC} + \text{PD}$ are meta-theoretic in some way.⁵² As we've seen, our current set-theoretic concept lacks this feature. It is this that will enable us to avoid examples of the kind

⁴⁷Not least because the former is absolute for well-founded models of \mathbf{ZFC} , which I've argued our concept of set is sufficient to determine.

⁴⁸I stop short of claiming full sufficiency, simply because I'm not clear that these requirements are sufficient and I don't want to overstate my case. The conjecture that replaces "good indicator" with "sufficient" is still worthy of study.

⁴⁹Walter Dean suggests that this part of my view can be seen as a kind of transcendental refutation of the existence of Orey sentences for a given concept. This seems to be precisely what informal rigour should be aiming at; removing the Orey-phenomenon wherever possible by determining a particular structure.

⁵⁰This is far from controversial. Other scholars (e.g. [Barton and Friedman, 2017], [Barton, 2019], [Antos et al., S]) consider versions of the *Inner Model Hypothesis* (IMH), an axiom candidate relying on extensions of the universe that implies that PD is false. An interesting fact, though one that represents a slight digression (and so I don't include it in the main body of the text) is that (i) this axiom can be coded in strong impredicative class theories (see here [Antos et al., S]) without referring to extensions (other than through coding), and (ii) the IMH implies that there are no inaccessible cardinals in V . A sufficiently strong version of \mathbf{ZFC}_2 with the IMH added would thus be a *fully* (rather than *quasi*) categorical axiomatisation. The IMH unfortunately does not touch CH (and so we could still construct the same MDA-style argument), however there are variations of the IMH (e.g. the *Strong Inner Model Hypothesis* SIMH) that imply that CH fails badly. There is a large community of set theorists that do regard PD as well-justified, however (see here [Koellner, 2014]) and so I set this point aside for the purposes of this paper.

⁵¹See [Woodin, 2017] and [Steel, 2005] respectively.

⁵²See here [Woodin, 2001] and [Welch, 2014].

given earlier where we consider two different legitimate extensions, since our informally rigorous concept should immediately tell us that one or the other extension is illegitimate. Thus, *if* my conjecture that a high-degree of theoretical completeness in combination with a categoricity proof is a good-indication of informal rigour, and *if* we accept PD, and *if* we accept that we do not have a high-degree of theoretical completeness with respect to set theory, *then* this supports the idea that ZFC_{2QW} is a good axiomatisation of our current level of informal rigour, since those concepts for which we have a high-degree of theoretical completeness can be determined up to isomorphism, and those which do not cannot.

Of course, given the claim that theoretical completeness in combination with categoricity likely yields informal rigour, our belief in informal rigour is *defeasible*. It could be, for example, that we *discover* techniques that allow us to find non-meta-theoretic sentences independent from our current theories of arithmetic and analysis. Hamkins entertains this suggestion:

My long-term expectation is that technical developments will eventually arise that provide a forcing analogue for arithmetic, allowing us to modify diverse models of arithmetic in a fundamental and flexible way, just as we now modify models of set theory by forcing, and this development will challenge our confidence in the uniqueness of the natural number structure, just as set-theoretic forcing has challenged our confidence in a unique absolute set-theoretic universe. ([Hamkins, 2012b], p. 428)

Perhaps then one thinks that my account goes too far: Surely we should not allow arithmetic to fail to be informally rigorous in such a situation?

I am quite happy to bite this bullet. If a technique along Hamkins' lines were to be found, I would accept that, after all, our thought concerning arithmetic is not determinate on the basis of the MDA. Given my current evidence however, I find this overwhelmingly unlikely; all such evidence (categoricity, theoretical completeness) seems to indicate that we are informally rigorous, and thus I find it likely that no such technique will be forthcoming.

Objection. *Mathematics is necessary!* It is very natural at this point to make the following objection: I have claimed that our concept of set is currently not informally rigorous and fails to determine a truth-value for CH. However I've also left open the possibility that in the future

we might have an informally rigorous concept of set that determines CH. Moreover, I think that the Axiom of Foundation was not determinate for Mirimanoff’s discourse about sets, whereas it is true given our concept of set. But don’t I think then that mathematical truth can *vary*? Doesn’t this contradict the widely held assumption that mathematical truth is *necessary*? My answer: Yes and no. We can have similar discourses using terms like “set” that are interpreted in very different ways at different times. However once the underlying concept of a discipline is fixed, the truths about that concept at that time are necessary. The only way that truth involving the discourse can vary is by the underlying concepts *changing* somehow.⁵³ So if by “mathematical truth is necessary” we mean “all truths about every mathematical discourse are fixed” then mathematical truth is not necessary, however if we mean “what is true of particular concepts at particular times is fixed” then mathematical truth *is* necessary.

A comparison case is useful here. Sheldon Smith (in [Smith, 2015]) argues (convincingly, in my opinion) that Newton’s thought involving the concept *derivative* could have been sharpened into several precise non-extensionally-equivalent concepts. Two such are the contemporary conception of standard derivative, and the *symmetric* derivative. For the purposes of our discussion it isn’t terribly important how these are defined, but they are not extensionally equivalent (for example, if we consider the absolute value function $f(x) = |x|$, the standard derivative is undefined at the origin, whereas it is the constant 0 function (i.e. the x -axis) for the symmetric derivative). Let us suppose (as Smith argues) that Newton’s concept *derivative*^{Newton} admitted of sharpenings to our concepts *standard derivative* and *symmetric derivative*. Then we should hold that Newton’s discourse about the derivative of functions did not determine a truth value for the sentence “The derivative of the absolute value function at the origin is the constant 0 function”. However, that sentence from our discourse is naturally interpreted (in most contexts) as false, since the concept to be employed (without further specification) for us is *standard derivative*, and the absolute value function has no derivative at the origin for the standard derivative.⁵⁴ But we should not think that such an example seriously

⁵³This idea has much in common with the discussion in [Ferreirós, 2016] of the idea of *invention cum discovery*.

⁵⁴Thanks here to Zeynep Soysal for suggesting that the concept of derivative might be a pertinent comparison case. See [Smith, 2015] for the details. That paper also contains several interesting remarks about how we might think conceptual indeterminacy and optimal theories relate in this context, critically examining [Rey, 1998]’s suggestion that we can implicitly think with a particular concept in virtue of deference to an optimal theory.

threatens the idea that mathematical truth is necessary, since the underlying concepts have changed in some way.⁵⁵

Objection. *First-order schemas and second-order interpretations.* A key part of Kreisel's 1967 paper is the idea that our commitment to first-order schemas is dependent upon the relevant second-order formulations (e.g. Replacement):

A moment's reflection shows that the evidence of the first order axiom schema⁵⁶ derives from the second order schema: the difference is that when one puts down the first order schema one is supposed to have convinced oneself that the specific formulae used (in particular, the logical operations) are well defined in any structure that one considers...([Kreisel, 1967], p. 148.)

His idea is that the informal rigour about the second-order concept is precisely what motivates the first-order schema. Since we are precise about the relevant particular structure, we can see that the first-order schema is always true on this structure, and this is what justifies the principle. Given this claim, and the fact that I have advocated an indeterminacy in the second-order quantifiers in certain contexts, does this undercut the motivation for the first-order schema of Replacement in terms of its second-order formulations?

Kreisel's point is controversial, but even if we accept the idea my response is quick: No. This is because the motivation for the first-order schema could be interpreted as follows: Given *any* particular interpretation of the second-order variables (a notion which I take to be indeterminate) the first-order schema is true. I do not need to be precise about the interpretation of the second-order variables in order to say that however I interpret them, the first-order schemata holds (this is itself a schematic claim). Kreisel seems to be assuming here that an acceptance of meaningful impredicative second-order theories entails a commitment to determinacy in how the quantifiers are interpreted, but this is a mistake, one can perfectly well accept impredicative second-order theories whilst denying that they have determinate interpretation.⁵⁷

⁵⁵Whether or not they are the *same* concept is a question we leave open and will mention in the conclusion.

⁵⁶Here, Kreisel is in fact talking about induction schema in **PA**, but the point transfers to Replacement.

⁵⁷This point has been made increasingly vivid by the recent boom in the study of (sub-)systems of second-order set theory.

Objection. *You've used notions that are dependent upon a definite concept of set in characterising the debate.* A further question is the following: Often I have used phrases like “range of the second-order variables” or “isomorphism” that are naturally interpreted as involving essentially higher-order concepts. But, by my own lights, these notions are indeterminate (for example, I can make two unstructured sets A and B such that $|A| < |B|$ isomorphic by collapsing $|B|$ to $|A|$). How is this legitimate given that I take our talk about sets to be indeterminate?

There are two points to make here:

First, I *do* take myself to be informally rigorous about a good deal of mathematics. I think it is likely that we, as a community, are informally rigorous about the real numbers and natural numbers, and the concept of well-foundedness. Thus, my view does not collapse into an ‘anything goes’ relativism.

Second, we can think of this paper as a modelling exercise concerning what we might be able to say about our current thought in the future. I might begin by saying “Suppose that we were informally rigorous about our concept *set*, what should we then say about our current thought?” I then take myself to have fixed some particular structure \mathfrak{M} about which I am informally rigorous and satisfies \mathbf{ZFC}_2 (possibly with a Henkin interpretation!) and analyse how the debate might be interpreted relative to \mathfrak{M} (e.g. that from the perspective of this hypothetical fixed universe⁵⁸, our current thought would be best axiomatised by \mathbf{ZFC}_{2QW}). This will then resemble how our intellectual descendants who are informally rigorous (should there be any) might think of our thought, much as how we now look at Mirimanoff’s thought as indeterminate.

6 Conclusions and open questions

In this paper, I’ve argued that our level of informal rigour in set theory is insufficient to convince us that our discourse and concepts determine a particular set-theoretic structure. I’ve argued instead that we should admit some structural relativity into our characterisations of structures, and a logic weaker than second-order is appropriate for characterising our current thought about sets (in particular \mathbf{ZFC}_{2QW}). I’ve also argued, however, that there is pressure on us to develop a

⁵⁸There are options here for how we might interpret this reference. It might be interpreted as picking out a specific such \mathfrak{M} (as outlined in [Breckenridge and Magidor, 2012]) or an ‘arbitrary’ such \mathfrak{M} in the style of [Fine and Tennant, 1983]. See [Horsten, 2019] for a recent treatment.

more informally rigorous concept of set, and thereby answer questions like CH. This identifies a fundamental distinction among the general structures; we have structures that are *unintentionally* general (like the structure corresponding to our discourse about sets) and those that are *intentionally* general (like the group structure). This said, there are lots of questions left open by the paper. I take this opportunity to raise some of the main ones.

Question. What is the status of the Modal Definiteness Assumption?

For most of the paper, I was happy to take the MDA as an assumption. I think given the kinds of possibilities described in the paper (Mirimanoff's futures, and our own), it's a very plausible assumption. This said, I am pretty convinced that both Kreisel and Isaacson would be unhappy with it (since it obviously implies their position concerning the determinateness of CH is false), and I haven't subjected it to really intense philosophical scrutiny. This is worth examination.

A second question concerns what kinds of particular structures are determined by our discourse and set-theoretic concepts. Assuming that I am right that ZFC_{2QW} is the right axiomatisation of our current discourse concerning set theory, there is the question of what is determined on this basis. By and large this theory has not (to my knowledge) been studied in detail.⁵⁹ There are some clear candidates for particular structures that can be determined given an acceptance of ZFC_{2QW} , for example any countable structure can be captured (e.g. the Shepherdson-Cohen minimal model), or models with minimality properties (e.g. L_{\aleph_1} is particular, since every countable ordinal is particular and L_{\aleph_1} is the minimal model containing every countable ordinal). We therefore ask:

Question. What other structures (both set-theoretic and non-set-theoretic) are particular, *given* that we accept that our thought is axiomatised by ZFC_{2QW} ?

Closely related is whether or not the only unintentionally general structures we talk about are set-theoretic. For all I've said, it might just be set theory that exhibits this feature. We might then ask:

⁵⁹Much of what I've considered here was gleaned from [Shapiro, 2001] and [Shapiro, 1991]. A recent contribution that briefly considers some other versions of ZFC with different underlying logics is [Kennedy et al., S] (esp. §8: Semantic Extensions of ZFC).

Question. Are there other interesting unintentionally general structures apart from set-theoretic ones?

Throughout the paper, I talked of concepts changing, for example in the shift from Mirimanoff's concept to our own, from Newton's concept of derivative, and from our own concept of set to that of our intellectual descendants. An interesting philosophical question is then in what sense there is a *continuity* of conceptual content and thought between one intellectual generation and the next.⁶⁰ We therefore ask:

Question. When a concept is made more precise, what remains constant, and how should we understand this continuity?⁶¹

We save the toughest question for last. Throughout, I've talked as though we might one day be informally rigorous about our concept of set. However, this might just not be possible. Perhaps any modification of the concept we suggest will be susceptible to decisive objections. Perhaps the different possibilities for extending our concept of set will all seem equally legitimate, and we simply cannot reasonably pick any one concept, whatever the pressure from the quasi-categoricity theorem.⁶² We therefore ask:

Question. Is it possible for us to legitimately develop an informally rigorous concept of set?

Perhaps we can answer this question affirmatively, or perhaps we are doomed to spend our days like a mathematical version of Buridan's Ass, trapped between equally (un)attractive options. Time will tell.

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⁶⁰As far as I know, this hasn't been considered in detail in the philosophy of mathematics, in comparison for example to the huge literature on conceptual continuity in the philosophy of science more generally.

⁶¹I am grateful to Chris Scambler for suggesting this question and some interesting discussion here.

⁶²Considerations along these lines are considered in [Hamkins, 2012b] and [Hamkins, 2015].

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