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Methods, Goals and Metaphysics in Contemporary Set Theory

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Abstract

This thesis confronts Penelope Maddy's Second Philosophical study of set theory with a philosophical analysis of a part of contemporary set-theoretic practice in order to argue for three features we should demand of our philosophical programmes to study mathematics. In chapter 1, I argue that the identification of such features is a pressing philosophical issue. Chapter 2 presents those parts of the discursive reality the set theorists are currently in which are relevant to my philosophical investigation of set-theoretic practice. In chapter 3, I present Maddy's Second Philosophical programme and her analysis of set-theoretic practice. In chapters 4 and 5, I philosophically investigate contemporary set-theoretic practice. I show that some set theorists are having a debate about the metaphysical status of their discipline– the pluralism/non-pluralism debate– and argue that the metaphysical views of some set theorists stand in a reciprocal relationship with the way they practice set theory. As I will show in chapter 6, these two stories are disharmonious with Maddy's Second Philosophical account of set theory. I will use this disharmony to argue for three features that our philosophical programmes to study mathematics should have: they should provide an anthropology of mathematical goals; they should account for the fact that mathematical practices can be metaphysically laden; they should provide us with the means to study contemporary mathematical practices.

Acknowledgements

During my studies for my *Diplom* in mathematics, I learned that set theorists can prove that their currently accepted formal mechanisms are insufficient to solve certain set-theoretic questions. Nonetheless, some set theorists are trying to find answers to these questions. How do they do this? I am thankful to my lecturer in set theory, Ralf Schindler, for teaching me the formalisms involved. These formalisms alone however do not answer the question to my satisfaction. There is something philosophical about the way the set theorists are trying to solve the formally unsolvable problems of set theory. To find out more about this, I embarked on a journey, intellectual and otherwise, a product of which is this thesis.

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Contents

Chapter 1

A Pressing Concern

How to philosophise about mathematics? It is a common view that the question what role mathematical practice should play in our philosophical investigations divides the philosophers of mathematics into two camps: the mainstream philosophers of mathematics pay little attention to mathematical practice, the philosophers of mathematical practice claim that mathematical practice should not or even cannot be ignored in our philosophical investigations. In this chapter, I aim to show that philosophers of mathematics disagree about the relevance of mathematical practice to the philosophical enterprise, the philosophical methods to interrogate mathematical practice and the very question what 'mathematical practice' is supposed to mean. I argue that in light of this heterogeneity of the philosophy of mathematics the question which features we should demand of a philosophical programme to study mathematical practice becomes a pressing question for the philosopher.

I conclude the chapter with a brief outline of the aims and content of this thesis.

1.1 Rebellion

In 1988, William Aspray and Philip Kitcher wrote in their 'Opinionated Introduction': 'prevailing orthodoxy takes the history of the philosophy of mathematics to start with Frege'; [Aspray & Kitcher, 1988], 3. Everything before then is prehistory. Building on work of J. W. Richard Dedekind, Karl T. W. Weierstrass and others, so Aspray and Kitcher tell us, Gottlob Frege undertook a review of the possible ways to set arithmetic on a firm foundation. Having argued that arithmetic is neither a posteriori, nor is there a mathematical a priori intuition, Frege concluded that the foundation of mathematics is logic. Frege tried to develop this foundation. Today, this project bears the name *logicism*. In 1902, Bertrand Russell discovered a logical inconsistency in Frege's foundations, which only underscored the importance of the foundational project, according to Aspray and Kitcher. Around the same time two rival programmes to logicism arose: *formalism*, championed by David Hilbert, and *intuitionism*, championed by Luitzen E. J. Brouwer. The philosophers studying the pros and cons of these three foundational positions form, for Aspray and Kitcher, the orthodox camp in the philosophy of mathematics.

As Aspray and Kitcher tell us, 'the distance between the philosophical mainstream and the practice of mathematics seems to grow throughout the twentieth century'; [Aspray & Kitcher, 1988], 17. Orthodox philosophy of mathematics, which had originated from the works of the above mentioned mathematicians, became increasingly divorced from mathematical practice and turned instead to 'the most general and central issues in philosophy– issues in epistemology, metaphysics, and philosophy of language'; ibid., 17. A minority of philosophers of mathematics drew attention to this divorce of philosophy and mathematical practice and argued for a reunion. Aspray and Kitcher call these philosophers the 'mavericks'.

'If the mainstream began with Frege, then the origin of the maverick tradition is a series of four papers by [Imre] Lakatos, published in 1963-64 and later collected into a book [Lakatos, 1976]'; [Aspray & Kitcher, 1988], 17. Lakatos did not participate directly in the foundational debate. Instead, Lakatos used a detailed historico-philosophical case-study of the development of the concept of a polyhedron to discuss mathematical methodology. A discussion of mathematical methodology was already present in [Pólya, 1973]. However, George Pólya was concerned with heuristics. Lakatos used the same case-study as Polya– the Descartes-Euler formula– to make an argument in epistemology. Lakatos rejected the view that mathematical knowledge is secure because it is proven in the strong sense of the prevailing orthodoxy. For Lakatos, mathematical knowledge is fallible. This argument is not present in Pólya's work.

According to Aspray and Kitcher, the maverick tradition takes the central task of the philosophy of mathematics to be the articulation of the methodology of mathematics. Aspray and Kitcher name Emily Grosholz, Michael Hallett, Mark Steiner and Kitcher himself as followers of this maverick tradition.

20 years after Aspray's and Kitcher's 'Opinionated Introduction', Paolo Mancosu attempted to delineate the maverick tradition in his introduction to [Mancosu, 2008]. Like Aspray and Kitcher, he presents the mavericks in opposition to the orthodox camp. Unlike Aspray and Kitcher, Mancosu credits Paul Benacerraf with setting the research agenda for, as Mancosu calls it, mainstream philosophy of mathematics. In 1965 Benacerraf wrote the paper 'What Numbers could not be' and in 1973 'Mathematical Truth'. These papers pose the questions whether there are mathematical objects and if so how we could have access to them. According to Mancosu, this 'problem of access' has been the problem that philosophers of mathematics have been pursuing for the last fifty years. Aspray and Kitcher had made a similar point; [Aspray & Kitcher, 1988], 14.

Mancosu's view on the mavericks is that they rebel against mainstream philosophy of mathematics. According to him, they have an 'iconoclastic attitude' (ibid., 5) in regards to what had been done in the foundational project of the philosophy of mathematics, which 'throw[s] away the baby with the bathwater' (ibid., 6). Mancosu individuates three claims to which, according to him, the maverick philosophers of mathematics subscribe. In his words:

> 1. anti-foundationalism, i.e. there is no certain foundation for mathematics; mathematics is a fallible activity;
> 2. anti-logicism, i.e. mathematical logic cannot provide the tools for an adequate analysis of mathematics and its development;
> 3. attention to mathematical practice: only detailed analysis and reconstruction of large and significant parts of mathematical practice can provide a philosophy of mathematics worth its name. ([Mancosu, 2008], 5)

Mancosu then distinguishes between those iconoclastic rebels of Aspray's and Kitcher's maverick tradition who subscribe to the three claims above from the not so iconoclastic philosophers of mathematical practice who, like himself, do not commit to these three claims.

Aspray and Kitcher had described the landscape of the philosophy of mathematics as consisting of two traditions: the 'orthodox' and the 'mavericks'. Mancosu claims that, 20 years after [Aspray & Kitcher, 1988], there is a new tradition: the 'philosophy of mathematical practice'. In this chapter, I argue that the non-mainstream philosophy of mathematics is too heterogeneous to be aptly described as consisting of one or two traditions.

A word on terminology is in order. Mancosu has called the tradition he supports the 'philosophy of mathematical practice'. The term 'philosophy of mathematical practice' or 'practice-oriented approach to the philosophy of mathematics' is, however, also used more broadly to refer to all of non-mainstream philosophy of mathematics; mavericks and Mancosu's 'philosophy of mathematical practice' alike. I will use the broader sense of the word in what follows; Mancosu's terminology will play no further part in this thesis.

1.2 A turn to practice?

I am arguing that the philosophy of mathematics is heterogeneous in a way that requires us as philosophers to answer the question which features we should demand of our philosophical programmes to study mathematics. To do this, I use the mainstream/practice-oriented distinction naively, which will allow me to show that this naivety is inapt to describe the philosophical landscape.

What makes a contribution to the philosophy of mathematics a contribution to the philosophy of mathematical practice? We might want to say that said contribution takes mathematical practice seriously. Lakatos studied 'what mathematicians actually do' – an often heard mantra, even today– and used what he had learned from his attention to mathematical practice in his philosophical arguments. However, this mantra will not do as a demarcation criterion between the mainstream philosophy of mathematics and the philosophy of mathematical practice. The major players in the early stages of the foundational debate were mostly mathematicians: Frege, Russell, Brouwer, Hilbert, Gödel and others. Their philosophical views were influenced by their in-depth knowledge of mathematics as practised. Kurt Gödel, a platonist, held that we might have to accept certain mathematical axioms because of their verifiable consequences, implying that mathematical practice and results play a role for the philosophy of mathematics; cf. [Gödel, 1947]. Frege formalised what he considered to be the mathematical reasoning at his time and this formalism was to become Frege's foundation for mathematics in his logicism programme.

Attention to practice in mainstream philosophy of mathematics continues to this day. José Ferreirós and Jeremy Gray tell us that attention to mathematical 'practice is to a greater or lesser extent on the agenda of basically all active philosophers of mathematics'; [Ferreirós & Gray, 2006], 12. Mark Balaguer agrees with this; [Balaguer, 1998]. Like Aspray and Kitcher, Balaguer sees the philosophy of mathematics split into two traditions or 'projects', as Balaguer calls them. One is the 'hermeneutical project' of 'providing an adequate interpretation and account of mathematical theory and practice'; ibid., 3. The other is the 'metaphysical project', which uses 'mathematical theory and practice to solve the metaphysical problem of abstract objects'; ibid., 158. Balaguer pledges allegiance to the metaphysical project, i.e., Balaguer can be considered as a mainstream philosopher of mathematics. Thus, Balaguer is an example of a mainstream philosopher of mathematics who agrees that 'mathematical theory and practice' play a role in the philosophy of mathematics.

Attention to mathematical practice hence cannot serve as a demarcation criterion between mainstream philosophy of mathematics and the practice-oriented approach. At least not if, as I have done above, the term 'mathematical practice' is understood as a primitive term. I will have more to say on the term 'mathematical practice' in section 1.5. For now, I will continue to use 'mathematical practice' as a primitive term.

1.3 Idealisation

Mainstream philosophers of mathematics may reasonably ask why the mathematical activities of mathematicians tell us anything about the ontological and epistemological questions surrounding the foundational debate and Benacerraf's problems. What is the philosophical importance of mathematical practice? There are proposals for answers to this question and I will explore one of them, Penelope Maddy's, in chapter 3. Here I wish to concentrate on the question rather than proposals for an answer. The question implies that the philosophy of mathematics ought to study the ontological and epistemological questions. Aspray and Kitcher write: 'it is pertinent to ask whether there are not also other tasks for the philosophy of mathematics, tasks that arise either from the current practice of mathematics or from the history of the subject', and the proponent of a philosophy of mathematical practice Kitcher answers 'yes'; [Aspray & Kitcher, 1988], 17. The aim is to extend the scope of the philosophy of mathematics. Mancosu gives a list of topics that could do so: 'issues having to do with fruitfulness, evidence, visualization, diagrammatic reasoning, understanding, explanation and other aspects of mathematical epistemology which are orthogonal to the problem of access to 'abstract objects"; [Mancosu, 2008], 1-2. It is understandable that mainstream philosophers of mathematics might regard the new questions as missing the point– they do, they where not designed to hit it– and it is also understandable that philosophers of mathematical practice might resent the dismissal of their questions.

Some philosophers of mathematical practice do more than merely extending the scope of the philosophy of mathematics. They criticise the tools the mainstream philosophers of mathematics use to answer their questions. One of the criticisms is that mainstream philosophy of mathematics is concerned with an idealised conception of mathematics which is philosophically damaging. In what follows, I discuss this in more detail.

Ferreirós and Gray write

> For centuries, the Western tradition tended to conceive of mathematics as an idealized collection of theories, living perhaps in an ideal space and waiting to be pinpointed by human beings. ([Ferreirós & Gray, 2006], 12)

Later they add

> Just as the working masses failed to conform to the theoretical prescriptions of Marxist political parties and real existing socialism didn't look like 'Socialism', real existing proofs often fell far short of 'proof' and can be analysed accordingly. Not to put them right, but to open up the question of how they (and not some idealized, but non-existent object) persuaded and convinced. ([Ferreirós & Gray, 2006], 36)

Ferreirós and Gray have picked a striking example of philosophical idealisation of mathematics: the concept 'proof'. A formal derivation consists of strings of symbols. The symbols have to be arranged in a certain way (they have to form well-formed formulae written down line by line etc.) and there are rules that govern what may be written on each line (premisses; use of modus ponens etc.). As the authors of [Buldt et al, 2008] tell us, formal derivations are usually used to represent mathematical proofs in discussions of mathematical methodology; 310. 'In mathematical practice, proofs are written down in a more condensed, semi-formal style'; ibid., 310. The 'traditional view' (ibid.) is that in such proofs technical details are left out for purely pragmatic reasons and the proofs could be completed to formal derivations. Bernd Buldt, Benedikt Löwe and Thomas Müller make two observations:

> First, the completion of enthymematic, semi-formal proofs to formal derivations almost never happens and hardly plays any rôle in the justification that mathematicians give for their theorems; second, also the production of semi-formal proofs [...] is only the final step of the mathematical research process. This final step, while important for the documentation of results and crucial for the careers of researchers, is not necessary for the acceptance of a proof by the mathematical community. For this, different forms of proof are much more relevant: informal sketches on the blackboard, or scribblings and drawing on napkins. ([Buldt et al, 2008], 311)

Mathematical knowledge holds a special place in philosophical thinking not least because it is proven. With this is mind, the following question is appealingly suggestive.

> Shouldn't [the above] forms of proof replace the unrealistic notion of formal derivation in our epistemology of mathematics? (ibid.)

This view attacks mainstream epistemology of mathematics. The authors argue that the tools of mainstream epistemology of mathematics, idealisations of proof in this case, are inapt for what they are intended to do. The claim is that by considering idealised proofs we idealise away the problem, replacing it with a new and different problem about these idealisations. These philosophers argue that the old understanding of proof should be replaced with their more practice-oriented understanding. This is an argument for a change in the philosophical tools we are using and thus the authors call for a change in how epistemology is done.

This call for change in the epistemology of mathematics is not shared by all philosophers of mathematical practice. For example, Mancosu argues for an extension of the scope of philosophy of mathematics in the sense described in the beginning of this section. He disagrees with the claim that 'the achievements of [the mainstream] tradition should be discarded'; [Mancosu, 2008], 18. For him the 'over-emphasis on ontological questions' and the 'single-minded focus on [Benacerraf's] problem of 'access' has reduced the epistemology of mathematics to a torso'; [Mancosu, 2008], 1. He holds that 'the epistemology of mathematics needs to be extended' (ibid.), not that it needs to be changed, as Buldt, Löwe and Müller argue. In this sense, Mancosu's views, unlike those of Buldt, Löwe and Müller, are continuous with mainstream epistemology of mathematics.

We learn from the above that by 2008, twenty years after Aspray's and Kitcher's 'Opinionated Introduction', there is dispute amongst the non-mainstream philosophers of mathematics. The conservatives argue that the philosophy of mathematical practice should extend the scope of, but be continuous with, mainstream philosophy of mathematics. The radicals hold that mainstream philosophy of mathematics uses the wrong philosophical tools for its tasks and should be discarded in favour of a philosophy which is discontinuous with mainstream philosophy of mathematics. Mancosu leans towards the conservative end of the spectrum, Buldt, Löwe and Müller towards the more radical end.

Aspray's and Kitcher's terms 'mavericks' and 'rebels' for the philosophers of mathematical practice no longer describe the contemporary philosophical landscape. Mancosu points out a list of new questions and areas of research, but he proposes to address them in a manner that is continuous with mainstream philosophy of mathematics. This does not amount to a rebellion.

I lean towards the more radical end of the above mentioned spectrum; I hold that the kinds of idealisations discussed in this section can be harmful to our philosophical understanding of mathematics. To give just one argument to support this view, consider the following quotation: 'Different mathematics gets done in different places'; [Ferreirós & Gray, 2006], 23. This point can hardly be denied. The question is however whether these differences are philosophically important. I argue that they are.

Consider the mathematical reasoning of the ancient Greek masters. On the face of it, there are some very obvious differences in their mathematical reasoning and contemporary (European) mathematical reasoning. Kenneth Manders for example has a piece in [Mancosu, 2008] which explores the diagrammatic reasoning of Euclid; pp. 80-133. If proofs were formal derivations (as explained above), Euclid would not have produced proofs. In fact, he could not have done so. The point is simply that the formalisms needed for such formal derivations began to take the shape they have today only in the

20^{th} century. But of course this is not what the idealisers mean. What they would point out is that if the proof is a correct proof, then it could be transcribed into the relevant formalisms and would thus be seen as a proof. This implies that Euclid and his followers did not produce proofs in the proper sense, but prototypes of such proofs. Transcribing these prototypes of proofs into the formal derivations of the idealisers can be arduous. Hilbert for example spend some effort transcribing Euclid's proofs into formal derivations. Along the way he found various 'missing assumptions' in Euclid's work; cf. [Kline, 1972], chapter 42. For example, Euclid had assumed, without stating so explicitly, that geometric lines do not have gaps. Once these proofs were 'tided up', the additional assumptions stated and so on, the proofs were seen to be correct (in a geometrical space without curvature). But notice that 'proof' refers to two different kinds of proof in the antecedent sentence: the former use of 'proof' refers to Euclid's proof, the latter to the formalism. The idealisers are talking about the formalisms. Setting aside the insulting undertone that the ancient masters did not produce proofs (and, as we have learned from [Buldt et al, 2008], that hardly anyone else does) but only prototypes of proofs, the idealisers are changing the subject matter here. But the subject matter is important. It is what we philosophise about. To replace it by an idealisation is to lose grip on the discursive reality of the given argument and hence on the claim to a philosophically satisfactory account. The idealisers might disagree and claim that a grip on this reality is not lost, that all that is important in a proof is translated into the formalisation as well. Why should this be so? The idealisers owe us an argument here which, so I argue, cannot be given. 'All that is important' in a proof includes why the proof persuaded and convinced. The actual proof did the convincing and persuading, not the idealisation, and we need to account for that.

Some philosophical idealisations of mathematics obscure the social dimensions that affect the mathematicians and the way they practice mathematics. For example, according to Eduard Glas, a common view is that the product of mathematical endeavours is alienated from the mathematician and 'assumes a partially autonomous and timeless status'; [Van Kerkhove & Van Bendegem, 2007], 29. This has led to a concentration on the product rather than its producers (i.e., the mathematicians) in philosophical thinking. Today, there are philosophical works about mathematical practice which show that not only the product but also the process of production of mathematical knowledge is philosophically important. One of these is the contribution to [Löwe & Müller, 2010], 155-178, by Christian Geist, Löwe and Bart Van

Kerkhove. The authors ask whether the certainty of mathematical knowledge is higher than in other scientific disciplines. Mathematicians rely on the work of other mathematicians without always verifying the results. Thus, mathematical knowledge could be closer to knowledge by testimony than commonly assumed. To guard against errors, there is a peer-review process in place. The authors discuss the extent to which proofs are checked in this peer-review process and how much is expected by the community standards. The authors draw our attention here to a social factor that has influence on mathematical knowledge.

In an idealised conception of 'proof', the proofs of mathematicians are mechanically verifiable and could, theoretically, be checked by a computer– in fact, today automatised proof-checkers are powerful indeed. However, we have already seen that proofs as they appear in mathematical practice are different from formal derivations. The peer-review process is not done by (infallible?) proof-checkers but by human mathematicians. Our philosophical accounts of mathematical knowledge have to account for this. And this introduces a social dimension into our thinking about mathematics because at this point the mathematician, the reviewers (and probably others) require our philosophical attention.[1]

Notice that agreeing to the claim that mathematics has a social dimension does not necessarily undermine the objectivity of mathematical knowledge. For example, Glas writes 'humankind has used descriptive and argumentative language to create a body of objective knowledge, stored in libraries and handed down from generation to generation, which enables us to profit from the trials and errors of our ancestors'; [Van Kerkhove & Van Bendegem, 2007], 40. This thought, which Glas traces back to Karl Popper, allows him to argue that whilst 'mathematics is a social practice' (ibid.), mathematical knowledge is nonetheless objective. Of course, philosophers are free to disagree. The point here is that accepting that mathematics has a social dimension does not necessarily undermine the objectivity of mathematical knowledge.

My above argument not only supports the more radical side on the conservative/radical spectrum, it also shows that mathematical practice is relevant

[1]An exemplary study of the social dimension of mathematical knowing is Eva Müller-Hill's PhD thesis [Müller-Hill, 2011], in which she conducts an agent-based analysis of mathematical knowing, focussing explicitly on the question whether a conception of a formalisable proof is necessary for a philosophical understanding of mathematical knowledge and justification. For an English version of her results, see e.g. [Müller-Hill, 2009].

to our philosophical understanding of mathematics. Attention to mathematical practice is necessary to avoid philosophically damaging idealisations of what mathematics is. Thus, I advocate a radical practice-oriented approach to the philosophy of mathematics.

I am arguing that the question which features we should demand of our philosophical programmes to study mathematics is a pressing one. In the last section, I argued that the study of mathematical practice is on the agenda of both the mainstream and the practice-oriented approach. In this section, I showed that the supporters of the practice-oriented approach disagree about the continuity of their philosophy with mainstream philosophy of mathematics along the conservative/radical spectrum. I also positioned myself on the radical side of this spectrum. In the next section, I will show that the practice-oriented philosophers of mathematics also disagree about their methods.

1.4 Method

Matthew Inglis and Andrew Aberdein claim that 'a common methodological move made by philosophers of mathematics' is to offer an example of a piece of mathematics, assert that this piece of mathematics has a certain property and 'appeal to the reader's intuitions for agreement'; [Inglis & Aberdein, to appear], 2. Inglis and Aberdein call such philosophers *exemplar philosophers* (ibid.). According to Inglis and Aberdein, exemplar philosophers are committed to the assumption that their intuitions about the example are widely shared.

In [Inglis & Aberdein, to appear], the authors claim that the exemplar approach to a practice-oriented philosophy of mathematics is unsatisfying. Their test-case is the debate about explanatory mathematical proofs. As Inglis and Aberdein tell us, Solomon Feferman suggests that those proofs are more explanatory that are more general; [Feferman, 1969]. Mark Steiner presents a proof which is, according to him, more general yet not explanatory, relying on his readers to share his intuitions on the explanatorieness of the proof; [Steiner, 1978]. Thus, Steiner is an exemplar philosopher in Inglis' and Aberdein's sense. Steiner gives his own characterisation of explanatoriness: an 'explanatory proof makes reference to a characterizing property of an entity or structure mentioned in the theorem, such that from the proof it is evident that the result depends on the property'; ibid. 143. Steiner supports his understanding of explanitoriness by two examples: a proof which is, according to him, explanatory and which fits his characterisation and a proof which is, again according to him, not explanatory and which does not fit his characterisation. Again Steiner relies on his readers to share his intuitions on the explanatoriness of the presented proofs. As mentioned, exemplar philosophers assume that their intuitions about the example are widely shared. As Inglis and Aberdein argue, it is indicative of a problem of the exemplar approach to philosophy of mathematical practice that Michael Resnik and David Kusher disagree with Steiner's intuitions about the proofs Steiner presented; [Resnik & Kusher, 1987].

Inglis and Aberdein avoid the exemplar approach and rely on statistical methods in their work instead. The plea for the use of statistical methods in the philosophy of mathematical practice, or a plea for an 'empirical philosophy of mathematics' as the authors call it, is also present in [Löwe et al, 2010]. The authors argue that some claims about mathematical practice are empirical claims. For instance, as the authors tell us, it is often claimed that mathematicians believe in the actual existence of mathematical objects and 'that these mathematicians interpret their own work as mental manipulation of abstract objects or of mental representations of abstract objects'; ibid., 2. The authors note that this is an empirical claim and can thus be empirically tested.

Löwe, Müller and Eva Müller-Hill hold that

> Philosophy of mathematics, like other areas of philosophy, relates phenomena (in this case, mathematics) to a philosophical theory. Whether the philosophical theory is correct/adequate or not is not independent of the phenomena. In analytic philosophy and in particular in philosophical logic, the analysis of phenomena is often done by a technique that one could call conceptual modelling, philosophical modelling, or logical modelling, in analogy to the well-known applied mathematics technique of mathematical modelling. ([Löwe et al, 2010], 2)

The authors argue that these philosophical models, just like scientific-models, can be tested against the phenomena. The study of these is, according to Löwe, Müller and Müller-Hill, underdeveloped. There is little available data against which to test philosophical models. According to the authors, this shows a poverty of our current understanding of mathematical practice. They 'propose to consider collecting data that allow[s] to identify stable philosophical phenomena in mathematical practice'; ibid., 3. They lead by example. They consider the philosophical model of what they view as the 'traditional view on mathematical knowledge', collect and present some relevant data, argue that there is no good fit between the model and the data and end their paper with the suggestion that 'we should now develop a new understanding of knowledge that replaces the one that [the authors] called the 'traditional view''; ibid., 17.

Whilst the statistical methods of empirical data-collection produce results that can reasonably be called general features of the studied practice, it can push philosophical work from a qualitative engagement with mathematical thought to the quantitative statistical results about these thoughts. It might hence be unsurprising that not all philosophers of mathematical practice rely on statistical methods in their writings.

One philosopher whose methods for the philosophical investigation of mathematical practice rely less on the quantitative statistical results the above authors stress is Brendan Larvor's 'dialectical philosopher of mathematics'. In [Larvor, 2001], Larvor works out what Lakatos might have meant by 'dialectical philosophy of mathematics'. In a dialectical argument the terms employed may develop and change throughout an argument; the meaning of 'justice' develops in the course of Plato's *Republic*. In contrast, formal logic requires the meanings of the terms of an argument to remain fixed throughout the argument. Lakatos has called the former 'language dynamics' and the latter 'language statics'; [Lakatos, 1976], 93. Larvor's dialectical philosopher of mathematics pays due respect to the language statics of formal logic. However, his focus is the dialectical logic of mathematical development. 'The dialectical philosopher of mathematics seeks rationality and integrity in the development of mathematics'; [Larvor, 2001], 216. Thus, the dialectical philosopher of mathematics shares with the historian an interest in development and change.

In contrast to the historians, the dialectical philosopher insists that 'the general direction of a historical development [in mathematics] is best explained by an analysis of the concepts governing that development'; ibid., 215. For this analysis, the dialectical philosopher of mathematics adopts what Larvor calls an 'inside-phenomenological stance'; ibid., 214. As Larvor stresses, this is neither a study of what it feels like to do mathematics, nor is it concerned with the individual mathematician. Rather, it is a conscious rejection of such idealisations of mathematics as I discussed in 1.3 whilst simultaneously insisting that rephrasing the philosophy of mathematics in psychological and sociological terms is to give up philosophy in favour of the outside-observer position of a (social) scientist. Instead, the dialectical philosopher of mathematics is 'interested in the "point of view" belonging to mathematics itself'; ibid, 214. This philosopher assumes the rationality and integrity of mathematical enquiry. Changes in mathematics, so the dialectical philosopher assumes, are normally brought about for mathematical reasons. Outside factors, such as environmental or historical factors, are not ignored, but as a 'default position' (215) it is assumed that mathematical rationality gives reasons that guide the change.

To learn about the mathematical reasons that govern change, the dialectical philosopher of mathematics is concerned with the 'evaluative discourse of mathematicians'; ibid., 218. However, Larvor offers no methodology which would order this discourse. The dialectical philosopher of mathematics has no philosophical model through which he studies mathematical practice; notice that this point marks a clear difference with the empirical approach proposed by Löwe, Müller and Müller-Hill mentioned above. 'The ambition [of the dialectical philosopher of mathematics] is to describe the rationale of mathematical research as we find it, rather than to press it into some pre-formed mould'; ibid., 218. In this sense, Larvor sees the dialectical philosopher of mathematics as a 'methodological anarchist'; ibid, 218.

The results of the methodological anarchist are influenced by his choices which cases to study. This problem is not discussed in Larvor's paper. In his book, [Larvor, 1998], Larvor mentions this problem for the Lakatos of *Proofs and Refutations*, whom Larvor calls a methodological anarchist as well; cf. [Larvor, 1998], 106. In Lakatos' work, so Larvor argues, this problem remains unresolved.

The problem might not be as serious as it at first appears. Once the dialectical philosopher of mathematics has obtained his results about the cases he has studied, he has illuminated a part of mathematical practice. All he needs is an argument that his study of this part of mathematical practice is in some way philosophically valuable. However, the methodological anarchist loses any claim to have told us something about the 'point of view' of some *general* mathematics.[2] All he has on offer at this point is a philosophically valuable story about a part of mathematical practice. Perhaps that is enough.

[2] As Larvor argues, Lakatos could not accept such a loss of generality in his programme due to his political motivations, cf. [Larvor, 1998], 105.

I have presented three approaches to philosophically investigate mathematical practice; exemplar philosophy, empirical philosophy and dialectical philosophy. There are more. The detailed presentation of one of these, Maddy's 'Second Philosophy', is the aim of chapter 3.

We have seen in this subsection some of the many methods philosophers interested in mathematical practice use. These methods are not necessarily mutually exclusive; e.g. the dialectical philosopher of mathematics might learn something from the statistical results of the empirical philosopher of mathematics. Nonetheless, these methods are noticeably distinct and some, such as the method of the exemplar philosopher, are criticised by proponents of other methods; in this case by Inglis and Aberdein. This shows that what I have naively been calling the philosophy of mathematical practice is not homogeneous in regards to its methods. This point is further strengthened by the fact that some conservative philosophers of mathematical practice, such as Mancosu, deem certain mainstream methods acceptable, e.g. idealisation of 'proof', which more radical philosophers of mathematical practice, such as myself, reject.

In my ongoing argument that the individuation of features which we should demand from our philosophical programmes to study mathematics is a pressing philosophical issue, we now know that the practice-oriented philosophers disagree about their methods and whether they are continuous with the mainstream approach. Furthermore, both mainstream and practice-oriented philosophers connect their philosophy in some fashion with mathematical practice, at least when a naive understanding of 'mathematical practice' is at play. It is now time to discuss the term 'practice'.

1.5 The Term 'Practice'

Thus far, I have used the term 'mathematical practice' as a primitive term. This is not only due to ease of exposition. There has been little attention of the philosophers of mathematical practice to investigate in more detail what 'mathematical practice' is supposed to mean. There is little material to fall back on, let alone a consensus on this topic. In this section, I present three accounts in order to draw attention to some of the dimensions of 'mathematical practice'. This will not amount to a definition of the concept. It will however indicate what I take 'mathematical practice' to mean.

The mantra mentioned in 1.1 is that philosophers of mathematical practice study what mathematicians actually do. However, not all they do classifies as belonging to mathematical practice; mathematicians ride buses, drink water and do many of the other things that are part of ordinary life. What we mean by 'mathematical practice' only connects to the mathematical work of the mathematicians. However, as pointed out in [Van Kerkhove & Van Bendegem, 2004], what connects to mathematical work and what does not is not so easily differentiable. For example, as Van Kerkhove and Jean Paul Van Bendegem point out, we may reasonably hold that the chit-chat between mathematicians before a conference does not belong to mathematical practice. But what if this chit-chat ultimately leads to the appointment of a mathematician to a university position? What if this appointment leads to the formation of a relevant and respected research group? The importance of networking for mathematicians might be a worthy subject of study for a philosopher of mathematical practice and I am reluctant to exclude conference chit-chat from philosophical consideration due to mere intuition, even though I would agree that generally this chit-chat does not belong to mathematical practice.

In [Kitcher, 1984], Kitcher defines a mathematical practice as a quintuple consisting of a language, a set of accepted statements, a set of accepted reasonings, a list of important questions and a collection of philosophical or metamathematical views. Whilst this definition of 'mathematical practice' is sometimes cited in introductory texts to works on practice-oriented philosophy of mathematics, e.g. [Mancosu, 2008], 4, n.2, it has found little application in the wider works of the philosophers of mathematical practice. As Mancosu reminds us, 'Kitcher's aim was to account for the rationality of the growth of mathematics in terms of transitions between mathematical practices'; ibid., 4. Today, philosophers of mathematical practice also engage in other questions about mathematics; recall here Mancosu's list: 'epistemological issues having to do with fruitfulness, evidence, visualization, diagrammatic reasoning, understanding, explanation'; ibid., 1. This list can be extended by considerations about the social dimension of mathematical practice, historical considerations and so on. Kitcher's definition of 'mathematical practice' may have been suitable for the job he wanted it to do, but as a general understanding for the term 'mathematical practice' it is philosophically unsatisfactory because questions about mathematical explanation and understanding or the various aspects of the social dimension of mathematical practice do not fit well into Kitcher's concept.

Kitcher's thoughts on 'mathematical practice' are taken up and elaborated further in [Van Kerkhove & Van Bendegem, 2004]. The authors of this paper notice the difficulties in giving a closed definition of the concept of 'mathematical practice' as Kitcher did and remark:

> when wanting to study mathematical practice in its entirety, the vast structure one would have to look at consists *at least* of the following seven elements: $\mathbf{MathPract} = \langle M, P, F, PM, C, AM, PS, ... \rangle$. ([Van Kerkhove & Van Bendegem, 2004], 11, emphasis in original)

Hereby M is the community of mathematicians, P stands for research programme, F is a formal language, PM are the proof methods, C the concepts, AM the argumentative methods and PS the proof strategies. But in what sense did Euclid have a formal language? And what about mathematical practices that did not rely on proofs, such as ancient Chinese mathematical practices? Arguably, Van Kerkhove's and Van Bendegem's open-ended definition does not fit well either.

As Van Kerkhove and Van Bendegem mention, the primary scope of their understanding of 'mathematical practice' is an institutional and social one; ibid. 13. This shows something about the development of the philosophy of mathematical practice. Where the early work of Kitcher remained close to a more traditional understanding of mathematical activity, the more recent work of Van Kerkhove and Van Bendegem incorporates other aspects of mathematics, such as social, institutional, individual and cognitive dimensions, into the understanding of mathematical practice as well.

According to Manders, 'at its most basic, a mathematical practice is a structure for cooperative effort in *control* of self and life'; [Mancosu, 2008], 82, emphasis in original. This allows for mathematical rigour: mathematical activity is closely controlled to avoid error. Manders gives a detailed study of such rigour in Euclidean geometric practice.

Manders' account of 'mathematical practice' is promising, but underdeveloped; note that Manders did not intend to philosophically delineate the term 'mathematical practice' in his contribution to [Mancosu, 2008]. It is promising because Manders manages to fruitfully apply his conception of mathematical practice to his study of diagrammatic reasoning in Euclidean geometric practice. His account of 'mathematical practice' cuts across the various elements of the concept individuated by Kitcher, Van Kerkhove and Van Bendegem in a way which seems to allow him to account for the difficulties the other authors face; it accounts for social dimensions as well as mathematical practices without a proving-tradition for example. However, we lack a clear story why, for example, the conference chit-chat mentioned at the beginning of this section sometimes is and sometimes is not part of mathematical practice. This does not mean that such a story cannot be given, just that Manders' account is thus far underdeveloped.

I argue that 'mathematical practice' has a philosophically relevant human dimension. Mathematics is an intellectual activity and thus part of our human intellectual life. Inquiring into mathematics as part of our human intellectual life connects the philosophical investigation of mathematics to disciplines outside of philosophy proper. There are points of fruitful connection with the history of mathematics and mathematics education, see [Ferreirós & Gray, 2006] and [Van Kerkhove & Van Bendegem, 2007] respectively. Furthermore, understanding the human dimensions of mathematics may lead to further beneficial interaction between philosophy and the way mathematics is practised. Mathematical practice is not in the business of studying itself and thus practitioners are in general ignorant to some aspects of their practice. Perhaps this may be in the conception of what counts as a proof, an ignorance of an implicit use of a method by some practitioners, what role diagrams play in mathematical reasoning, ignorance about certain social factors, locational differences or else. Obviously, these will vary from mathematician to mathematician. Making these aspects more clearly visible through philosophical analysis allows mathematicians to consider them more clearly and account for them in their work. This has already happened. Maddy, a philosopher, has made two methodological principles explicit that (some) set theorists implicitly use in their work on theory development in set theory; cf. 4.1. Today, these principles are (explicitly) discussed by some set theorists; e.g. [Löwe, 2001], [Steel, 2010] and [Magidor, 2012]. I say more on this in chapter 4.

It is hard to deny that 'mathematical practice' has human dimensions. It is less difficult to argue that not all the human dimensions are relevant to the philosophical investigation of mathematics. For example, the social dimensions of 'mathematical practice' I have been stressing in this section are missing from Kitcher's definition of 'mathematical practice'. There is disagreement about which aspects of 'mathematical practice' are relevant to the philosophical investigation of mathematics. Thus, philosophers of mathematical practice are not united in questions about their subject matter.

1.6 Aims of this thesis

In this chapter, I have argued that most philosophers of mathematics take mathematical practice seriously, at least as long as 'mathematical practice' is understood as a primitive term. I have then focussed on the practice-oriented approach to the philosophy of mathematics. The term 'practice-oriented approach' remained naive because I have given no clean demarcation criterion to distinguish between the practice-oriented and the mainstream approach. I have shown that the community of practice-oriented philosophers of mathematics is heterogeneous. They are divided on the question of the continuity of their approach with the mainstream approach along the conservative/radical spectrum; they use different methods to interrogate mathematical practice philosophically; they are not united in their understanding of the term 'mathematical practice'. In short, their philosophical programmes to study mathematics have different features.

The descriptive part of the story of this chapter is that the landscape of the philosophy of mathematics is heterogeneous. To learn that a piece of philosophy belongs to the practice-oriented philosophy of mathematics neither positions it on the conservative/radical spectrum, nor does it tell us about what 'mathematical practice' means to the author of the piece of philosophy. The mainstream/practice-oriented distinction is too coarse to account for the fine-grained differences found in the works on the philosophy of mathematics. These differences however are philosophically important: they lead to different, and at times conflicting, philosophical accounts of mathematics, as I have shown in this chapter.

The story of this chapter also has a normative part. I have shown that the landscape of the philosophy of mathematics is heterogeneous. Put differently, I have shown that the philosophical programmes to study mathematics on offer have different features. Indeed, some features of such programmes stand in conflict with features of other such programmes; e.g. Mancosu accepts the idealisations discussed in 1.3, the dialectical philosopher of mathematics does not. Which features are philosophically damaging and which are philosophically desirable? At this point, the normative question arises which features we should demand of our programmes to philosophically study mathematics. This question gets at the fine-grained distinctions in the landscape of the philosophy of mathematics in a way that the coarse question whether we should be mainstream or practice-oriented philosophers does not. This shows that the fine-grained question about features is a pressing philosophical concern.

The distinction between mainstream and practice-oriented approach may be a helpful first classification of a philosophical work. As a leading question of this thesis however it is too coarse.

The development of a philosophical programme to study mathematics is a philosophical endeavour which lies far beyond the scope of a PhD thesis. However, I can point out some features that such a programme should have. To do this, I first present one particularly well developed programme to philosophically analyse mathematics: Penelope Maddy's Second Philosophy. Maddy concentrates her work on set theory. I follow her lead and present two stories from set-theoretic practice: the pluralism/non-pluralism debate and the existence of instances of reciprocal relationship between mathematics and metaphysics. My two stories about set-theoretic practice are disharmonious with Maddy's Second Philosophical account of the practice. Because the Second Philosophical programme is so well developed, my two stories amount to more than mere criticism of Maddy's philosophical presentation of set-theoretic practice; in the context of the Second Philosophical programme, the stories reveal programmatic features that, so I will argue, we should demand of our philosophical programmes to study mathematics.

Notice here that it is not an aim of this thesis to refute Maddy's work. As I discuss in more detail in chapter 6, my philosophical analysis of set-theoretic practice does not necessarily stand in conflict with Maddy's Second Philosophy. As argued there, Maddy might account for the disharmony of my two stories with her philosophical analysis of set-theoretic practice by maintaining that in these two cases the set theorists discussed do not use proper set-theoretic methods. This raises two sorts of questions. Firstly, do such normative claims fit into the Second Philosophical programme as presented by Maddy? Secondly, can philosophy meaningfully offer normative guidance to mathematical practice? Both questions are beyond the scope of this thesis. I study Maddy's Second Philosophical programme as a means to access the debate about the kind of features we should demand of our philosophical programmes to study mathematics; my critique on Maddy is method rather than goal.

My thesis has two goals. First, I philosophically analyse set-theoretic practice, thereby contributing to the ongoing investigation of mathematical practices. I then confront Maddy's Second Philosophical programme with my philosophical analysis. Out of the misfit of my stories about set-theoretic practice with Maddy's story about this practice I will develop three features we should demand of our programmes to philosophically study mathematics.

The identification of these three features is the second goal of my thesis.

The above suggests an outline of this thesis. In this chapter, I have argued that the individuation of features our philosophical programmes to study mathematics should have is a pressing question. Maddy has presented a particularly well developed such programme, which she has used to analyse set-theoretic practice. In particular, Maddy has concentrated on what I will call the foundational debate. As we will see in the course of this thesis, the set theorists participating in this debate use mathematical formal results in their arguments; these results form the discursive reality of the set-theoretic debate. These arguments serve as evidence for the philosophical claims Maddy and I make about set-theoretic practice. Thus, it is important to get a grip on this discursive reality. The aim of chapter 2 is to present the foundational debate and the discursive reality it is embedded in. With the groundwork done, I present Maddy's Second Philosophical programme in chapter 3. As mentioned, Maddy's programme is philosophically rich. I concentrate my presentation on those parts that are relevant to the debate which features philosophical programmes to study mathematics should have. In chapter 4, I present the contributions of the set theorist Joel David Hamkins to the foundational debate and show that his multiverse-view stands in conflict with what Maddy has identified as proper conduct in set theory. In chapter 5, I show that this tension is part of an ongoing debate the set theorists are having: the pluralism/non-pluralism debate. Hamkins is a pluralist. I present the contribution of a non-pluralist, Hugh Woodin, and argue that the metaphysical position these set theorists adopt in the pluralism/non-pluralism debate influences and is influenced by their set-theoretic practice. In my conclusion in chapter 6, I return to the question about programmes to philosophically investigate mathematics and argue for three features these programmes should have: they should provide us with an anthropology of mathematical goals; they need to be sensitive to the influences the metaphysical views of the mathematicians can have on mathematical practices; they should provide us with the means to philosophically investigate contemporary mathematical practices.

Chapter 2

The Foundational Debate

Arguments in a debate are not merely isolated chains of premisses, logical steps and a conclusion. There are reasons why the argument is given, goals the argument is trying to achieve and debates in which the argument is presented. Arguments are given from the perspective of certain positions, and the proponents of these positions are trying to bring together into a coherent view their convictions and the facts. Arguments are embedded in the discursive reality of a debate.

It is part of the aim of this thesis to philosophically investigate what role metaphysics can play in set-theoretic practice. By the end of chapter 5 I will have argued that we need to consider the metaphysical views of some mathematicians to fully appreciate certain mathematical arguments. Furthermore, to appreciate why these mathematicians hold these metaphysical views, we need to understand the mathematical arguments that have led the mathematicians to their views. Thus, to make my philosophical point it is necessary to have a grip on some parts of certain mathematical arguments and debates. Mathematical arguments are part of the evidence for my philosophical claims, which requires us to study these arguments. We can learn about these arguments by studying mathematical publications.

The mathematical publications studied in this thesis rely heavily on mathematical facts. This is in particular the case for the metaphysical arguments the set theorists present in these publications. Thus, to understand the discursive reality in which the primary evidence for my philosophical claims about set-theoretic practice is embedded in, we need to get a philosophical grip on the relevant mathematical facts. We need to learn about mathematics because mathematics is part of the evidence for the philosophical claims

I make.

The part of mathematics we need to know about in this thesis is the foundational debate. In this chapter, I give a presentation of this debate, which will be incomplete in some regards. For example, I will not present mathematical formalisms. This is because such elements add little to the philosophical understanding of those parts of the foundational debate which are relevant for my argument. Instead, I aim to convey a feeling for the forces at play in this debate. What matters for this thesis is that we understand the struggle of the set theorists studied here. And this struggle can be understood without formal definitions and proofs.

In what follows, I assume that my reader knows about the basics of set theory, such as ordinal and cardinal numbers. The reader interested in technical details is referred to [Enderton, 1977] for an introduction to set theory, [Kunen, 2006] for an introduction to independence proofs, [Jech, 2006] for more advanced technical results and [Kanamori, 2009] for historically motivated results in the higher reaches of set theory.

2.1 The Independence Issue

David Hilbert famously wrote 'in der Mathematik gibt es keinen ignorabimus'; [Hilbert, 1900].[1] If mathematics were some (recursively enumerable) formal theory that contains arithmetic, whose underlying logic were first order logic, whose proofs would be given in a certain, specified and formal way and so on, then Hilbert would have been proven wrong by Kurt Gödel. In 1931, Gödel presented his proofs of the two incompleteness theorems. Because the currently accepted axiom system for set theory satisfies the assumptions in Gödel's theorems, the theorems can be used to prove the formal incompleteness of the currently accepted axioms of Zermelo-Fraenkel set theory with choice, ZFC. That is, it follows directly from Gödel's theorems that there is a statement S in the language of set theory such that ZFC proves neither S nor $\neg S$; in this case S is said to be independent from the ZFC axioms. Hence the question 'Does S hold?' cannot be formally answered from ZFC. As we will see in this thesis, this fact about formal contemporary set theory bothers some, but not all, set theorists.

[1] Translated in 'Mathematical Problems', *Bulletin of the American Mathematical Society* 8 (1902), pp. 437-479, as 'in mathematics there is no [thing that cannot be known]'.

In the proof of the second incompleteness theorem, Gödel constructs a statement for a theory T (whose precise nature need not bother us here) by formalising (in the given formal language) the statement 'theory T is consistent'. He then shows that if T proves this statement, then T is inconsistent (recall that inconsistent theories formally prove every statement). This means that if theory T is consistent, then the constructed statement cannot be proved from T; no theory that satisfies the conditions of Gödel's theorems can prove its own consistency.

Gödel holds that sets exist independently of us and that statements about sets are either true or false.[2] One common view before Gödel proved his theorems was that all truths about mathematical entities can be found by proof; cf. the quote from Hilbert above. Gödel's theorems show that for formal set theory there are statements about sets which are neither provable nor disprovable. This divorces 'truth' from 'proof'; there are true statements which are not provable. In this sense, Gödel's theorems are important for the philosophical aspects of mathematics.

Whilst Gödel's proof of his theorem gives us a constructed and hence directly accessible independent statement, the statement is also non-natural in the sense that the formalised statement has never appeared in any piece of mathematics prior to Gödel's proof and it is doubtful whether mathematicians would have ever considered it in their questioning about mathematics without Gödel's proof. To be sure, Gödel's statement plays an important role for the proof of the second incompleteness theorem, but beyond its technical implications it bears little connection to ordinary mathematical thought. We may hence reasonably wonder why the set theorists should be worried about the incompleteness of ZFC. All that Gödel showed was that one may construct non-natural independent statements. However, why should the set theorists be interested in those? If we wish to understand sets, then we wish to find answers to our questions about sets. However, the non-natural statements Gödel constructed do not qualify; they are not 'our questions'.

In [Cohen, 1963/4], Paul Cohen showed that there is a long-standing question of set theory that cannot be answered from ZFC. Cohen proved that there is a natural statement that is independent from ZFC. To do this, Cohen relied on Gödel's proof that the Continuum Hypothesis holds true in the constructible universe. Cohen then introduced the method of forcing to construct an extension of the constructible universe in which the Continuum

[2] See for example [Gödel, 1947]. See [Maddy, 1997], 89-94, 172-176 for a short exposition.

Hypothesis fails. All of the mentioned concepts are of importance for what follows because they set the stage for the two contemporary arguments in the foundational debate I will discuss in chapter 4 and 5. Thus, to get a philosophical grip on these arguments it is important to get a philosophical grip on these concepts.

In 1873, Georg Cantor proved that the cardinality of the natural numbers is strictly less than the cardinality of the the real numbers (henceforth: the reals); cf. [Ferreirós, 2007], 180. This raises the so-called Continuum Problem: is there a set whose cardinality is strictly between the cardinality of the natural numbers and the cardinality of the reals? There were times in which Cantor thought this question has a positive answer and times in which he tried to prove a negative answer. None of his proof-ideas amounted to a final proof. Instead, today we know the negative answer as Cantor's Continuum Hypothesis, CH: 'There is no set whose cardinality is strictly greater than the cardinality of the natural numbers and strictly less than the cardinality of the reals'.

There is still no consensus about the truth of the CH amongst set theorists, and in this thesis two mutually exclusive arguments will be presented; in particular 4.2.1 and 5.2. This disagreement hinges on the understanding of the word 'truth' by the set theorists. Truth certainly does not mean provability for them, because what Cohen showed was that the CH is neither provable nor disprovable from ZFC; the CH is independent from ZFC.

As mentioned, Cohen's proof relies on results by Gödel. In [Gödel, 1938], Gödel showed that if ZF is consistent, i.e. if ZFC minus the Axiom of Choice is consistent, then $ZFC + CH$ is consistent. The proof-idea is that, given the consistency of ZF, one can construct a set-theoretic model in which $ZFC + CH$ holds. The concept of a 'model' has become fundamental in set-theoretic practice, so it will be useful to elaborate on it here.

A model for a formal language $\mathcal{L}$ consists of a collection things, called the *domain*, plus an interpretation for the non-logical symbols in $\mathcal{L}$. The model assigns the relations, predicates and functions as given by the non-logical symbols of $\mathcal{L}$ to the elements (or sets thereof) of the domain. That is, in a domain of a model the elements stand in certain relations to one another, have certain predicates and so on. A model can hence be seen as a world full of the entities of the domain. In this thesis, I am only interested in models for two-valued logics; from now on by 'model' I mean 'model for a two-valued logic'. In such models, every statement (of the relevant formal language) is either true or false. Moreover, if all the statements of some theory T are true in the model M, then we call M a model for T. Thus, M is a model for ZFC, or simply a ZFC-model, if all the axioms of ZFC are true in M. It has become common practice to ascribe some metaphorical agency to set-theoretic models and say that 'M thinks that ZFC holds' or 'M believes ZFC' in case M is a ZFC-model; cf. [Kunen, 2006], [Jech, 2006]. I adopt this use of language in this thesis because it overcomes the clumsiness of some more formal descriptions.

Using models we can say that statement S is independent of theory T if and only if there are (at least) two models of T such that one is a model of S and the other a model of $\neg S$. Are there always two such models? It follows from Gödel's theorems that if the theory is consistent (and meets the formal requirements of Gödel's theorems), then there is an independent statement. Hence, there must be two such models for any consistent theory (that meets the requirements). In addition, once we realise that, e.g., $T + S$ also satisfies the assumptions of Gödel's theorems and that hence there are independent statements even for this theory, we see that independence carries us far and guarantees that there are not only two but infinitely many T-models. However, it is merely a logical result that these models exist (in some sense of the word); these considerations tell us nothing about what these models look like. That is, if S is a set theoretic statement and M is a ZFC-model, we are in general unable to check whether S holds in M. This incapability is tamed in Gödel's constructible universe.

Gödel carefully constructed a model, called the constructible universe L, for ZF in such a way that he was then able prove that in this model the Axiom of Choice and the Continuum Hypothesis also hold. That is, Gödel's model is a $ZFC+CH$-model. These results are due to Gödel; [Gödel, 1938]. Today we know much more about the constructible universe L. It lends itself nicely to mathematical study and, contrary to the general case mentioned in the last paragraph, for the model L set theorists can check for most natural set theoretic statements S whether it holds in L or not.

In conjunction with Gödel's completeness theorem, what has been said about L thus far is a powerful meta-mathematical tool. The completeness theorem states that a theory is consistent if and only if it has a model. Hence, if L is the model for some theory T, then this theory is consistent. However, L was built assuming the consistency of ZF. Thus, theory T is consistent assuming that ZF is consistent. In jargon: T is consistent relative to ZF. Thus, what Gödel showed is that $ZFC + CH$ is consistent relative to ZF.

For the sake of readability, I will assume that ZFC is consistent and suppress explicit mention of this assumption from now on. Wherever this assumption plays a crucial role I will point this out.

Gödel showed that $ZFC+CH$ is consistent. To show the independence of CH from ZFC Cohen hence needed to show the consistency of $ZFC+\neg CH$. To do this, Cohen built a model for this theory and the way he did this was to influence set theoretic practice profoundly: Cohen introduced the method of forcing.

Forcing is done over a so-called *ground model*. Cohen started with Gödel's constructible universe L. He now had a given model, i.e. a domain and an interpretation of the non-logical symbols. To this domain[3] he carefully added new entities whilst simultaneously ensuring that the resulting model would still be a ZFC-model. The latter part of that sentence is highly non-trivial. In general, adding elements to the domain of a ZFC-model results in a model that is no longer a ZFC-model. However, Cohen skillfully relied on so-called generic filters to ensure that the new model he built was still a ZFC-model. The resulting model is called a *generic extension* of the ground model. However, Cohen did even more. Not only did he ensure that ZFC would still hold in the generic extension, he carefully arranged it so that he could show that $\neg CH$ holds in the generic extension.

Cohen has constructed a model for $ZFC + \neg CH$. Gödel's L is a model for $ZFC + CH$. There are hence two ZFC-models, one in which the CH holds and one in which it fails. Thus, CH is independent from ZFC.

In this section we saw that there are natural set-theoretic questions, such as CH, that our current formal theory of sets cannot answer. In this thesis, I call this issue the *independence issue*. Some set theorists are having a discussion about the independence issue. However, calling this discussion the independence debate is too limiting; the set theorists are discussing more than just the independence issue, as will become clear in this thesis. I will use the term *foundational debate* to refer to the debate these set theorists are currently having about the fundamentals of their field.

[3]More precisely: Cohen used the Löwenheim-Skolem theorem to obtain a countable model. This helps with the technical details of forcing; in particular, in countable models the existence of a generic filter for a partial order, i.e. a set which intersects all dense subsets of that order, is guaranteed; cf. [Kunen, 2006], 186. Today, other methods of forcing are known, such as the Boolean valued method and the naturalised method, which show that countability of the domain of a model is not necessary; [Hamkins, 2011], see 5.3.2 for a brief discussion.

2.2 Fragility

In the last section, we saw that independence is an issue in set theory. Not only are the somewhat unnatural Gödel sentences independent from our current formal theory of sets, ZFC, but there are natural questions, such as the Continuum Problem, which cannot be answered on the basis of the ZFC axioms alone. In this section, I look more closely at the method of forcing and 'paint a picture' as it were.

The first question we need to ask is why set theorists need forcing at all. Here an example will serve to give an idea towards an answer to the formal side of this question. Let us suppose that we wish to show that the collection of all sets exceeds the constructible sets that inherit Gödel's constructible universe. [Kunen, 2006], 184, asks us to suppose that working in ZFC we could define some collection of sets (a transitive proper class) and prove that in a model with this collection as its domain, all axioms of ZFC plus the axiom stating that there is a non-constructible set holds. Now, Gödel's constructible universe L is such that any model of ZFC that contains all the ordinals also contains L as a submodel. Then our supposed model and L must be different, as ensured by the fact that L thinks that all sets are constructible whereas our model believes the negation of this sentence. Hence, working in ZFC, we would have proven that there is a proper extension of L. That is, from ZFC we would have proven that not all sets are in L and hence that not all sets are constructible. This however contradicts the fact that ZFC+'every set is constructible' is consistent, as witnessed by Gödel's construction of L. Hence, we cannot find a class such as Kunen has asked us to do by purely working in ZFC. To show the consistency of ZFC+'not every set is constructible' we need another method. The current method to do so is the method of forcing.

The method of forcing always starts from a given model of set theory. This is the so-called *ground model*. One then formally defines a *forcing* as a partial order in the ground model.[4] Furthermore, one needs a *generic filter*. A filter can be seen as an 'inverted ideal'. Ideals are downwards-closed collections of sets: if a set is in the ideal, then all its subsets are also in the ideal. Filters are upwards-closed collections of sets. A generic filter is a special kind of filter which has non-empty intersection with all dense

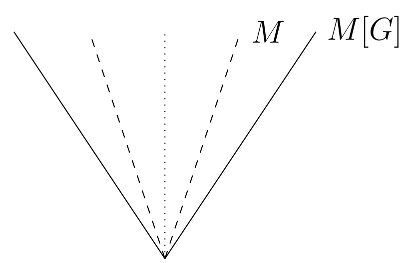
[4] The Boolean-valued approach to forcing uses Boolean-valued algebras instead of partial orders. I will not discuss the Boolean-valued approach here; cf. 5.3.2 for more.

subsets of the forcing. With a generic filter given, one can define the *generic extension* or *forcing extension* of the ground model via the given forcing by the given generic filter. This leads to the following heuristic: choose a statement that you wish to force over your chosen ground model. Then find a partial order which has suitable dense subsets such that the elements of the intersection of these dense subsets with the generic filter guarantee that the desired statement holds in the forcing extension.

That forcing is a widely used method in set-theoretic practice also becomes clear when considering the great variety of different forcings set theorists have studied. Examples include Cohen forcing, Easton forcing, Kunen-Paris forcing, Silver's forcing and Priky forcing. There is also 'the' trivial forcing, which are all those partial orders such that the generic filter is an element of the ground model. In these 'trivial' cases, the mathematics works out that the forcing extension is just the ground model. Nothing happens, hence the name 'trivial forcing'.

Notice that there are two uses of the word 'forcing'. One is the method, the other is the formal understanding of the word as a partial order. I will refer mostly to the method. When the formal understanding as a partial order is intended, I will point this out.

A forcing extension is 'fatter' than its ground model. Every set that belongs to the ground model also belongs to the forcing extension. However, the forcing extension contains some sets that are not part of the ground model. For example, the generic filter, often called G, of a non-trivial forcing is an element in the forcing extension but not in the ground model. However, the forcing extension and the ground model agree on the height of the ordinals. That is, the forcing extension is as 'tall' as the ground model. Pictures help. Here, M is the ground model, $M[G]$ the forcing extension and the dotted line represents the ordinals.



Because $M[G]$ is a ZFC-model, there are forcing extensions of $M[G]$. Of these forcing extensions there are further and further forcing extensions.

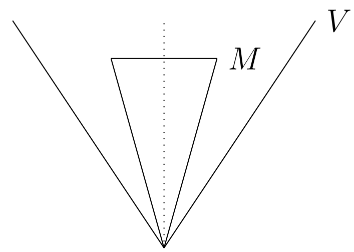
In fact, we never run out of statements such that we can construct a forcing extension of whatever model we are currently working with such that the statement holds. The jargon for this is that we never run out of forceable statements or that there are always statements that can be forced. In this sense, the process of constructing forcing extensions never stops. It is illuminating to see why this is so.

As pointed out in [Hamkins & Löwe, 2008], there are three types of set-theoretic statements in relation to forcing.[5] The ones connected to the endlessness of the forcing process are statements that can be made true or false at will via forcing; they are like light-switches which can be switched on and off at will. Then there are statements that can be made true by forcing in such a way that they remain true in all further forcing extensions; these statements are like buttons which, once pressed, remain pressed. The last type of statements consists of the negations of the statements of the second type. Whatever set-theoretic statement we consider, forcing can act on it (in perhaps a trivial way), and this shows how very fragile and context dependent some of our current formal conception of set-hood is. It is no wonder then that the members of a discipline which, as we will see in the next chapter, is regarded by some as the foundation of mathematics itself, want to rid set theory of this fragility. And thus it might be unsurprising that 'forcing stability' is a desired feature of theories; see next chapter.

Let me recapitulate where we are at the moment in my presentation. We have learned from Gödel's theorems that there are formally unsolvable questions about sets. Cohen introduced the method of forcing to show that a very natural question about sets, the question for the size of the Continuum, is formally unsolvable from our currently accepted axioms of set theory. This method has proven to be a fruitful and powerful tool and is nowadays widely used amongst set theorists. The results they were able to prove show a certain type of fragility of the set concept.

Forcing affects the girth of a model; the forcing extension is at least as 'fat' as its ground model. However, as I mentioned when I introduced forcing a few pages back, the ground model and the forcing extension agree on the height of the ordinals. This is not the case for all ZFC-models. For example, there are so-called cut-off universes which are, nonetheless, models of ZFC. Here is a picture.

[5] For a detailed proof of this, see [Rittberg, 2010].



Hereby, M is the cut-off universe and the dotted line represents the ordinals. As can be seen from the picture, there are some ordinals which are part of V but not of M. However, M is a ZFC-model and hence believes that it contains all the ordinals. But then, who has it right, V or M? With forcing we have considered the girths of different models. What the cut-off universes show is that we might have to think about tallness of models as well.

These are some of the facts. If we assume that there is one 'true' conception of set, then there must be one 'true' model of set theory, one model which captures correctly the true conception of set-hood. But then the mathematical facts discussed in this chapter are problematic because they reveal that our current theory of sets is not sufficient to fully describe this true correction of set-hood. We will encounter this position throughout this thesis, first in Maddy's presentation of the practice of set theory in chapter 3, then under the name 'universist view' in chapter 4 and finally under the banner of 'non-pluralism' in chapter 5, where we will also meet Hugh Woodin, a set theorist who holds such a non-pluralist view. Woodin is searching for this 'true' model of set theory or, how it is more commonly known, the 'true universe of sets'. What we have learned from forcing is that there are many different models of set theory that could be the true universe of sets. But then: which one is it? How to find out? How to convince others? These are the questions that arise from the facts about forcing and the belief that there is one true universe of sets, and these are some of the questions that I will be tracking throughout this thesis.

Non-pluralism is a nice position for the exposition of the problem I am describing here because it makes visible the struggle some set theorists face. However, there are other set theorists who are less troubled by the mathematical results described thus far. In chapter 4 I will present some thoughts of the pluralist Joel David Hamkins on the matter, who argues that the independence issue is not a problem for but rather a feature of set theory. For now, however, let us be non-pluralists and 'feel the struggle' as it were.

2.3 Large Cardinals

Questions and problems have started to pile up. Do the formally unsolvable questions have answers? How fat is the universe of sets? How tall is it? There are no easy answers. The aim of this thesis is also to present the answers some contemporary set theorists have given on these matters. There is, however, a surprising order to the independent questions, which I aim to illuminate in this section: the large cardinal hierarchy.

In his seminal book on large cardinals, *The Higher Infinite*, Akihiro Kanamori writes about large cardinal axioms:

> These hypotheses posit cardinals that prescribe their own transcendence over smaller cardinals and provide a superstructure for the analysis of strong propositions [i.e. propositions not provable from ZFC alone]. As such, they are the rightful heirs to the two main legacies of Georg Cantor, founder of set theory: the extension of number into the infinite and the investigation of definable sets of reals. The investigation of large cardinal hypotheses is indeed a mainstream of modern set theory. ([Kanamori, 2009], p. XI)

A large cardinal axiom then is a statement, which affirms the existence of a set, the large cardinal. These large cardinals are such that they 'prescribe their own transcendence over smaller cardinals', which is why they are also called 'strong axioms of infinity'. For example, one can consider the Axiom of Infinity as a large cardinal axiom because it affirms the existence of a set (namely ω) which is transcendent over all those cardinals which can be proven to exist without the Axiom of Infinity (in this case: over all finite cardinals). Similarly with the Axiom of Replacement. Without this axiom, no sets of cardinality $\aleph_\omega$ or bigger can be proven to exist. These two axioms however have already been accepted into our contemporary axiomatic system, whereas other large cardinal axioms do not (yet?) hold this place of honour. The Axiom of Infinity and the Axiom of Replacement are usually not seen as large cardinal axioms in a formal setting. For example, Kanamori, in his 'chart of cardinals', lists 28 different types of large cardinal axioms, and neither the Axiom of Infinity nor the Axiom of Replacement is on this list; [Kanamori, 2009], p. 472. This, however, is convention. There are no generally accepted and precise criteria that an axiom could meet to be classified as a large cardinal axiom.

When added to ZFC, large cardinal axioms increase the strength of the axiom system. This means that some statements which are formally undecidable from the axioms of ZFC become formally decidable in the large cardinal extension. Perhaps the easiest example of this is the case of inaccessible cardinals.[6] Call the axiom stating 'there is an inaccessible cardinal' IC. $ZFC+IC$ proves that there is a model of ZFC. Thus, by Gödel's completeness theorem, $ZFC + IC$ proves the consistency of ZFC, $Con(ZFC)$. However, by Gödel's second incompleteness theorem, ZFC does not prove $Con(ZFC)$. Thus, $ZFC+IC$ is stronger than ZFC: not only does it prove all that ZFC proves, it also proves $Con(ZFC)$ (and other statements not provable from ZFC). Notice that this implies in particular that ZFC does not prove the consistency of $ZFC+IC$ (because otherwise ZFC could prove its own consistency).

The large cardinal axioms have been individuated from a variety of different fields in mathematics, amongst them measure theory, descriptive set theory and model theory; cf. [Kanamori, 2009]. Thus, prima facie, the different axioms have little in common. This makes it all the more surprising that the large cardinals can be linearly ordered by their consistency strength. This needs explaining.

The consistency strength of theory T is higher than that of theory S if the consistency of T implies the consistency of S. The surprise about large cardinals is that for any two large cardinal axioms ϕ and ψ it holds that either $ZFC + \phi$ has a consistency strength at least as high as $ZFC + \psi$ or vice versa. Thus, the consistency strength order is a linear order of the large cardinal axioms, which means there is a *large cardinal hierarchy*. As it turns out, the axiom stating that there is an inaccessible cardinal is the weakest large cardinal axiom in this hierarchy.

We just saw that adding large cardinal axioms to ZFC results in an axiom system which is stronger than ZFC. Furthermore, the large cardinal axioms can be linearly ordered. This leads to a calibration of the complexity of the statements formally undecidable from ZFC through the large cardinal hierarchy: the *degrees of unsolvability*. Say the set-theoretic statements ϕ and ψ are both formally undecidable from the axioms of ZFC. Furthermore, assume that the weakest large cardinal extension in which ϕ becomes formally decided is $ZFC+LCA_1$ and the weakest large cardinal extension in which ψ

[6]For ease of exposition, I suppress the difference between weakly and strongly inaccessible here and in what follows.

becomes formally decided is $ZFC + LCA_2$. One can then compare the two large cardinal axioms LCA_1 and LCA_2 in the large cardinal hierarchy. Say that LCA_1 is stronger than LCA_2. Thus, to formally decide ϕ one needs to add a stronger large cardinal axiom to ZFC than to decide ψ. In jargon: ϕ has a higher degree of unsolvability than ψ. Thus, by passing through the large cardinal hierarchy, seemingly unrelated set-theoretic questions can be meaningfully compared.

Large cardinal extensions solve problems formally unsolvable from ZFC and the higher the large cardinal axiom in the consistency strength order, the more former formally unsolvable questions become formally solvable. Thus, there is good reason to study large cardinals: they resolve some of the problems of set theory I have been tracing in this chapter. As we have learned from the Kanamori quotation above, the study of large cardinals has indeed become a mainstream topic in contemporary set theory.

There are limitations to the power of large cardinals. Not all formally unsolvable problems can be solved by large cardinal axioms. The well-known example is the Continuum Hypothesis. [Lévy & Solovay, 1967] shows that no addition of a large cardinal axiom to ZFC can prove or disprove the CH. This shows that even if we assume large cardinal axioms, there remain formally unsolvable problems in set theory. The degree of unsolvability of these problems exceeds the large cardinal hierarchy.

Large cardinal axioms can resolve some of the undecidability issues the set theorists face in contemporary set theory, but not all of them. Thus, large cardinal extensions of ZFC might be helpful stepping stones towards a solution, rather than the solution itself. The exploration of these stepping stones faces the consistency question: are the large cardinal extensions of ZFC consistent? As a negative result, we learned from the discussion of inaccessible cardinals above that the consistency of ZFC does not imply the consistency of ZFC plus the axiom stating that there is an inaccessible cardinal. Inaccessible cardinals are the weakest of the large cardinals, which means that since ZFC cannot prove the consistency of 'ZFC+ 'there is an inaccessible cardinal', ZFC can also not prove the consistency of any other large cardinal extension of ZFC. But then, how to ensure, or at least have an argument for, the consistency of the large cardinal extensions of ZFC? This is an aim of the inner model programme.

2.4 The Inner Model Programme

Recall that in a given model of set theory, every set-theoretic statement either holds or fails to hold. Thus, a model of set theory decides every set-theoretic statement. There is hence a difference in studying what can be proven from ZFC, where there are undecidable statements, and studying what holds in a given model, in which every statement is decided.

It is, in general, difficult to discern the truths of a given model of set theory. When the model is an arbitrary model of set theory, the only truths of this model we can determine are the statements which can be proven from ZFC. If a definition of the model is given, then one can generally discern the truths of the model in a bit more detail. In this sense, the currently best understood model of set theory is Gödel's L.

In [Scott, 1961], Dana Scott showed that L cannot accommodate any large cardinals on the level of measurable cardinals or above. Measurable cardinals are in the lower third of the large cardinal hierarchy presented in [Kanamori, 2009] and can therefore be considered as fairly weak large cardinal axioms. Thus, L cannot be used to provide arguments for the consistency of (most) large cardinal extensions of ZFC.

What makes L interesting is that it is so well understood. However, L cannot accommodate large cardinals. The *inner model programme* seeks L-like structures which can accommodate large cardinals.

Gödel constructed L from the assumption that ZF is consistent. This established the consistency of ZFC relative to ZF. We already saw that large cardinal extensions cannot be proven to be relative consistent to ZFC. The heuristic of the inner model programme is hence slightly different from Gödel's: the methodology to construct an inner model which can accommodate a certain large cardinal is to assume the consistency of ZFC plus the targeted large cardinal axiom. Thus, unlike Gödel's case, the constructed model does not serve as a proof of relative consistency for the large cardinal extension because consistency features as a premiss.

The inner model programme has successfully constructed models for some large cardinal extensions of ZFC. However, each such model comes with a limiting result similar to Scott's result mentioned above: no large cardinal higher up in the hierarchy than the targeted large cardinal can be accommodated in the model.[7] Thus, the set theorists are trying to construct models

[7] However, compare 5.2.

which can accommodate large cardinals higher and higher up in the large cardinal hierarchy. As it turns out, the higher up in this hierarchy one climbs, the more complicated the construction of the inner model becomes.

The construction of an inner model which can accommodate a certain large cardinal axiom serves as an argument for (but not a proof of) the consistency of ZFC plus the relevant large cardinal axiom. The point is that the construction of a model for the theory makes it unlikely that the theory is inconsistent.

State of the presentation: problematic are the statements that are formally undecidable from ZFC. Large cardinals can tame this problem by deciding more and more statements the higher we climb in the hierarchy. The inner model programme generates arguments that the given large cardinal extensions are consistent. However, the problem is only tamed, not banished. The inner model programme cannot yet generate arguments for all large cardinal extensions (but see 5.2) and the large cardinal extensions cannot formally decide all statements. Thus, the problem is still with us, but we now have some tools to handle it. These tools will feature prominently in the arguments of the set theorists presented in this thesis.

Chapter 3

Maddy

In this chapter, I present Maddy's programme to investigate mathematics philosophically: Second Philosophy. The chapter starts with a presentation of Maddy's account of why mathematical practice is relevant to the philosophical study of mathematics in 3.1. I then move to a discussion of the details of the Second Philosophical programme, its methods and aims, in 3.2. In subsection 3.2.1, I present the case studies of set-theoretic practice Maddy has given in her books and the set-theoretic methods she has distilled from them. Subsection 3.2.2 elaborates on Maddy's account of the properness of these methods.

3.1 Maddy's attention to practice

In 1997, Penelope Maddy published *Naturalism in Mathematics*. In this book, Maddy introduces 'a position on the proper relations between the philosophy of mathematics and the practice of mathematics'; [Maddy, 1997], 161. She presents an approach to philosophy that regards the mathematics as seen in mathematical practice as a given background for philosophical study. This means that practice comes first and philosophy grows out of it. Below, I will call this Maddy's principle.

Maddy calls her position a naturalism 'because it owes so much to Quine'; [Maddy, 1997], 161. But by 2007, she realised that the term 'naturalism' is used too broadly to describe her position. In her next book she coined a new term: *Second Philosophy*. In the book, [Maddy, 2007], Maddy develops her naturalistic approach into a philosophical programme. She seems to have encountered the worry that if mathematical practice comes first, there may not be much left to do for the philosopher. In her book she hopes to show that there is 'plenty to do'; ibid., 411.

In 2011 Maddy rounds out her philosophical work with a Second Philosophical investigation of the ontological status of mathematics in *Defending the Axioms*.[1]

In an early publication, Maddy writes

> The central problem in the philosophy of natural science is when and why the sorts of facts the scientists cite as evidence really are evidence. The same is true in the case of mathematics. ([Maddy, 1988], 481)

This problem has stayed with her throughout her philosophical career. 23 years later, in *Defending the Axioms*, Maddy phrases her question thus

> what are the proper methods of set theory, and why? ([Maddy, 2011], 37)

Maddy's proposal to answer these questions is her Second Philosophical programme. This programme got its name only in Maddy's 2007 book. However, the continuity of her thoughts in the three books [Maddy, 1997], [Maddy, 2007] and [Maddy, 2011] suggests that all three books contribute to the presentation of this programme. This is not to say that Maddy's work has not matured. In her later works she has made some small adjustments to her earlier contributions. Where appropriate, I will point these out in what follows. However, these adjustments are minor; there is a continuity in the three above mentioned books. Therefore, I will treat these three books as one body of work which presents Maddy's Second Philosophical programme in what follows.

We can learn about Maddy's appreciation of the practice of mathematics from her discussion of Willard V. O. Quine. Discussing Quine's admiration for and critique of Rudolf Carnap, Maddy writes

[1] Since then, Maddy has published the book *The Logical Must: Wittgenstein on Logic* (2014). In this book, Maddy draws on and challenges Wittgenstein's analysis of logical necessity with a Second Philosophical analysis of her own. The nature of the logical must has no direct bearing on my set theory focused thesis; I will not discuss Maddy's newest book any further.

> If it weren't so pretentious, I might say, with similar admiration and gratitude, that Second Philosophy [i.e. Maddy's position] was largely determined by problems that I felt Quine's naturalism presented. ([Maddy, 2007], 67, n. 6)

One of these problems stems from Quine's well-known argument for realism in mathematics: the indispensability argument.

(P1) We ought to have ontological commitment to all and only the entities that are indispensable to our best scientific theories.

(P2) Mathematical entities are indispensable to our best scientific theories.

In conclusion

(C) We ought to have ontological commitment to mathematical entities.[2]

(P1) speaks of 'best scientific theories'; what are these? Quine's answer relies on 'a convergence of evidence' ([Quine, 1976], 246), which Quine divides into five virtues:

> One is simplicity: empirical laws concerning seemingly dissimilar phenomena are integrated into a compact and unitary theory. Another is familiarity of principle: the already familiar laws of motion are made to serve where independent laws would otherwise have been needed. A third is scope: the resulting unitary theory implies a wider array of testable consequences than any likely accumulation of separate laws would have implied. A fourth is fecundity: successful further extensions of theory are expedited. The fifth goes without saying: such testable consequences of the theory as have been tested have turned out well, aside from such spare exceptions as may in good conscience be chalked up to unexplained interferences. ([Quine, 1976], 247)

When we take these virtues seriously, so Maddy argues, then we see that atomic theory satisfied these virtues at around 1900. However, and this is Maddy's point, at this time physicists did not actually believe in the existence of atoms. Maddy cites Wilhelm Ostwald's textbook of 1904:

[2] This formulation of the indispensability argument is taken from [Colyvan, 2015].

> the atomic hypothesis has proved to be an exceedingly useful aid to instruction and investigation... One must not, however, be led astray by this agreement between picture and reality and combine the two. (as cited in [Maddy, 1997], 138)

This attitude changed, according to Maddy, through the work of Jean Perrin, published between 1908 and 1911. This leaves us with a time-span in which scientists used atoms in our best scientific theory, in fact, atoms were indispensable to these theories, yet not even physicists believed in their actual existence. There must hence be something wrong with (P1) in the indispensability argument. What is missing, so Maddy argues, is an attention to scientific practice. (P1) simply does not capture the actual theorising of the scientific community. '[T]he indispensable appearance of an entity in our best scientific theory is not generally enough to convince scientists that it is real'; [Maddy, 1997], 143.

Maddy argues that we need to pay closer attention to actual scientific reasoning in the indispensability argument. Then, if we wish to draw any conclusions about the ontological status of mathematical entities from this argument, we need to better understand how scientific reasoning regards mathematics. We need to know just how mathematics features in scientific theories and practice.

The first point Maddy notes is that mathematics is often usefully applied to assumptions that we know are literally false. For example, in the theory of water waves it is usually assumed that the ocean is infinitely deep. Maddy points out that 'on the face of it, an indispensability argument based on such an application of mathematics in science would be laughable'; [Maddy, 1997], 143. Instead, if we wished to keep an argument based on the Quinean argument, we would need a more responsible indispensability argument, which accounts for the kinds of subtleties of the application of mathematics in science that infinitely deep oceans provide. Maddy concentrates on the question whether science responsibly supports the assumption that there is a continuum and concludes that this question is currently unsettled. In fact, according to Maddy, responsible indispensability arguments support only the existence of 'a few (if any) mathematical entities'; [Maddy, 1997], 153. Nevertheless, science does support the existence of scientific entities such as atoms (after Perrin). This shows that the scientific enterprise does not treat mathematical entities on epistemic par with scientific entities. Hence, science does not seem to be in the business of assessing mathematical ontology, as

Quine's argument would have us believe.

The above argument is a serious attack on Quinean realism. However, even if we allow for a more responsible form of indispensability argument, one that pays closer attention to scientific practice, the fact that the 'what is' of mathematical entities is answered by science does not sit well with Maddy. Consider the example of the continuum again. Maddy discusses the questions of the continuity of space-time to 'cast serious doubt on the existence of any physical phenomena that are literally continuous'; [Maddy, 1997], 152. According to Maddy, these kinds of questions are currently unsettled by science. Now, if there were a continuum then the set theorist's question about its size is legitimate, even though this question cannot be settled from the axioms of set theory, ZFC, alone.[3] But if it should turn out that there is no continuum, then the question for its size is nonsensical. Then, so Maddy claims, every extension of ZFC by new axioms would be as good as any other.[4] This however does not square well with set theoretic practice. Some set theorists who pursue the search for new axioms certainly do not regard them all as equally good, as we will see in what follows. But should these set theorists then not be very interested in the natural sciences, waiting as it were on a decision of the natural sciences on the matter of the existence of the continuum? Maddy's point is that set theorists do not pay this kind of close attention to the natural sciences, at least not more than any other neutral bystander. This could show that set theorists do not know what they should be interested in, but Maddy dismisses this point. Instead, she concludes that mathematical methodology does not depend on application and hence not on science.

Quine has presented science as an arbiter of mathematical ontology. Maddy points out that if this were so, then set theoretic practice should account for this and hence be different than it currently is. She writes

> a philosopher wedded to realism, or unshakeably convinced by the original Quinean considerations, could conclude [...] that mathematicians and scientists are in error, that they should correct their methods and procedures in light of these various philosoph-

[3] Recall here the Continuum Problem discussed in chapter 2.

[4] This is a subtle point. Maddy presupposes here (in [Maddy, 1997]) that we need to have ontological commitment to an objective mathematical reality to regard one axiom as more suitable than another; a point Maddy later (in [Maddy, 2011]) rejects (cf. Arealism below).

> ical insights. My own inclination– and here I follow Quine himself [...]– is to reject such moves. This simple inclination lies at the heart of naturalism. ([Maddy, 1997], 160)

Maddy phrases this inclination as a principle:

> if our philosophical account of mathematics comes into conflict with successful mathematical practice, it is the philosophy that must give. (ibid., 161)

Maddy's principle highlights a key difference between Quinean naturalism and Maddy's philosophy. Quine evaluates mathematical methods on grounds of their connection to the natural sciences; Maddy proposes to evaluate mathematical methods on mathematical grounds. The principle also presents Maddy with a reason to study mathematical practice: faithfulness to mathematical practice is a criterion of adequacy for philosophical accounts of mathematics. As Maddy has told us, the inclination to reject those philosophical results that are not aligned with mathematical practice lies at the heart of her naturalism. This requires a study of the practice. Maddy proposes a scientifically minded approach to philosophy, which tests its results against a 'reality' of mathematical practice. This is such a fundamental belief for her that she names her position after this insight: 'Philosophy undertaken in [...] complete isolation from science and common sense is often called "first philosophy", so I call [my position] Second Philosoph[y]'; [Maddy, 2011], 40.

Maddy's reasons to study mathematical practice hence stem from her naturalistic position. Her naturalism is embedded into a larger 'scientific' project. Maddy presents us with a literary character.

> Imagine a simple inquirer who sets out to discover what the world is like, the range of what there is and its various properties and behaviours. She begins with her ordinary perceptual beliefs, gradually develops more sophisticated methods of observation and experimentation, of theory construction and testing, and so on; she's idealized to the extent that she's equally at home in all the various empirical investigations, from physics, chemistry, and astronomy to botany, psychology, and anthropology. She believes that ordinary physical objects are made up of atoms, that plants live and grow by photosynthesis, that humans use language to describe the world to one another, that social groups tend to behave in certain ways, and so on. She also believes that she and

> her fellow inquirers are engaged in a highly fallible, but partly and potentially successful exploration of the world, and like anything else, she looks into the matter of how and why the methods she and others use in their inquiries work when they do and don't work when they don't; in these ways, she gradually improves her methods as she goes. ([Maddy, 2011], 38)

This inquirer is Maddy's Second Philosopher. The quote displays Maddy's profound respect for the scientific method of 'theory construction and testing'. She accepts and embraces the fallibility of this method, in part because she believes that it leads to a 'partly and potentially successful exploration of the world', and acknowledges the need for constant revision and improvement of the methods used. This kind of scientific-mindedness permeates through Maddy's Second Philosophy, as I will present in more detail in the next section.

3.2 Second Philosophy

We have seen Maddy's argument that mathematical practice deserves philosophical attention. What does this mean for her? Maddy presents her *Second Philosophy*.

> There is no hard and fast specification of what 'science' must be, no determinate criterion of the form 'x is science iff...' It follows that there can be no straightforward definition of Second Philosophy along the lines 'trust only the methods of science'. Thus, Second Philosophy, as I understand it, isn't a set of beliefs, a set of propositions to be affirmed; it has no theory. ([Maddy, 2007], 1)

My task then is not an easy one: describe that which has no theory. However, there is precedent: Maddy has already done this. She introduces a character, the Second Philosophical inquirer we met in the long quotation at the end of the last section. 'Second Philosophy is then to be understood as the product of her inquiries' (ibid.). It seems prudent to adopt Maddy's method.

Before I continue with the presentation of Second Philosophy, let me draw attention to the possible difference between the Second Philosopher's position and Maddy's presentation thereof. Unlike the contemporary philosopher

Maddy, the Second Philosopher is idealised; the Second Philosopher is equally at home in all scientific disciplines whereas Maddy is not. Thus, we might agree to Second Philosophy but disagree with Maddy's presentation of some of the particulars of the cases she has studied. In Maddy's words

> one might sign on as a Second Philosopher while thinking I've gone astray in my pursuit of the particulars. ([Maddy, 2007], 3)

I will pick up this question again in chapter 6 and argue that because we learn about the methodology of the Second Philosophical programme by following Maddy's lead by example, stripping away the idiosyncrasies of Maddy's investigation from the Second Philosophical programme leaves us without answers to the pressing questions I highlighted in chapter 1.

In the long quotation given at the end of the last section, Maddy presents us with an interested inquirer who 'set out to discover what the world is like'. Her, the inquirer's, methods are those of observation and experimentation, and her at first every-day inquiries soon lead her onto the terrain of more sophisticated science. At this point, Maddy's inquirer learns about scientific theories and scientific methodology. It becomes clear that mathematics plays an important role in these sciences, and so the inquirer develops an interest in mathematics, especially applied mathematics. Her 'narrowly applied sense of the subject gradually gives way to the full pursuit of pure mathematics' ([Maddy, 2011], 41) after realising what Maddy presents as three strands in the rise of pure mathematics: the legitimacy of mathematical goals; the desire for a stocked warehouse of mathematical objects and structures; the realisation that mathematical accounts of physical phenomena are not literal truths. I discuss these in turn.

For Maddy's argument of the first of these strands, consider the following well-known quote from Cantor:

> Die Mathematik ist in ihrer Entwicklung völlig frei [...] das *Wesen* der Mathematik liegt grade in ihrer *Freiheit*. ([Cantor, 1883], 563-564, emphasis in original)[5]

Maddy tells us that a systematic analysis of the historical developments that led up to this view of mathematics would illustrate that the motivation for some mathematical activities is based on mathematical rather than scientific

[5]Translated in [Hallett, 1984], 16, as 'Mathematics is entirely free in its development. [...] The essence of mathematics lies in its freedom.'

goals and values. Maddy does not actually give this story (probably due to space constraints) but makes her point by reminding us of parts of the history of group theory. She tells us that Galois, in his well-known 1830 work on what we today call substitution groups, did not identify the concept of a group; Cayley presented the concept only about 20 years later. It took yet another 20 years for the concept to flourish because before the 1870s 'there weren't enough examples of groups to make the notion useful'; [Maddy, 2011], 7. But once enough examples were present, it became clear that the concept of a group calls attention to similarities for a broad range of otherwise dissimilar structures. The concept of a group became an object of mathematical study. However, groups did not feature in the theories of the natural sciences until the 1920s. Maddy's Second Philosophical inquirer concludes that groups were studied in pursuit of mathematical goals, not scientific ones.

The second realisation of Maddy's inquirer is that mathematics should provide a 'stocked warehouse' of mathematical objects and structures. The idea is that mathematics is 'peeled away' from the natural sciences.

> Descartes, Newton, Euler and many others believed mathematics to be an accurate description of real phenomena [...] they regarded their work as the uncovering of the mathematical design of the universe. ([Kline, 1972], 1028)

Maddy gives Gauss as an example of someone who held that Euclidean geometry is true of physical space. However, the use of non-Euclidean geometry in the theory of General Relativity and, so Maddy tells us, the confirmation of General Relativity made this view untenable. However, mathematicians were reluctant to give up Euclidean geometry as straightforwardly false. Instead, mathematicians introduced a distinction between physical space and mathematical space (or spaces). That is, here is a mathematical theory that is 'protected from empirical falsification by positing a realm of abstracta about which they remain true'; [Maddy, 2011]. 9. Maddy has called this a warehouse, stocked with mathematical objects and structures from which the natural scientist can freely choose. The conclusion Maddy's inquirer draws from this is that not all mathematical objects and structures need to be used in the sciences. It is desirable to create a stock of as of yet un-applied mathematics so that when the time comes, the scientists can help themselves in the stocked warehouse to whatever they need – this is what happened in Einstein's case: he faced a problem in his theory of general relativity, and the mathematician Marcel Grossman could point him towards non-Euclidean geometry. This is the Second Philosophical inquirer's second reason to study pure mathematics.

The above mentioned second realisation shows that mathematics can be disconnected from empirical investigation. Nevertheless, this does not show that Galileo's 'book of nature' is not written in the language of mathematics. Here is the relevant passage from Galileo:

> Philosophy [nature] is written in that great book which ever lies before our eyes– I mean the universe– but we cannot understand it if we do not first learn the language and grasp the symbols, in which it is written. This book is written in the mathematical language, and the symbols are triangles, circles and other geometrical figures, without whose help it is impossible to comprehend a single word of it; without which one wanders in vain through a dark labyrinth. ([Galileo, 1623], 4)

Maddy's inquirer's third realisation is that scientists today no longer agree with Galileo's view. One of Maddy's case studies is the derivation of the Ideal Gas Law. The argument for this law begins with an abstract description of the gas as consisting of point masses that do not interact with each other and whose collusions with the container walls are perfectly elastic. We can now try to make this a more literal description of physical space by replacing point masses by three-dimensional objects and account for their interaction with each other. The result is van der Waal's equation. Yet, van der Waal's model remains an abstract model. We can refine the model further, but only up to a point. 'Unfortunately, despite the stunning success of quantum mechanics as a predictive device, we still have no firm grasp of what worldly features underlie its various mathematical constructs'; [Maddy, 2011], 23.

One moral the Second Philosophical inquirer draws from these realisations is that today's mathematics in application is not in the business of discovering some hidden real world structures. Rather, 'we're constructing abstract mathematical models and trying our best to make true assertions about the ways in which they do and don't correspond to the physical facts'; ibid., 27. It is certainly interesting how this correspondence between mathematics and the world of physical phenomena works, but in this thesis, I focus, with Maddy, on the 'constructing abstract models' side of mathematics; that is, on pure mathematics. Maddy's questions are: what principles guide and govern the pure mathematician. 'What', Maddy asks, 'constrains our methodological choices?'; ibid., 31.

Faced with this question, Maddy's inquirer turns to the history of mathematics once again. Maddy tells the story of mathematics at the end of the 19^{th} century, how, after natural science as a pragmatic and empirical basis for mathematics had been called into question, mathematics had broken up into a variety of branches. Each branch seemed to rest on its own axiom system, so the question for Maddy's inquirer becomes which axiom systems are acceptable. She, the inquirer, notes that the mathematicians of the time did not accept just any such system. The worry at the time was, according to Maddy, that the axioms might fail to describe genuine structures. '[F]or example, the coherence of the axioms for non-Euclidean geometries had been demonstrated by modelling them in Euclidean geometries, and Euclidean geometry itself could be modelled using the real numbers, but the buck has to stop somewhere'; ibid. 32. Gradually it became clear, according to Maddy, that all of mathematics could be expressed using only the means of set theory. For Maddy, this means that the inquirer who is interested in the question what constrains our methodological choices in mathematics can focus on set theory.

> Questions of the form– is there a structure or a mathematical object like this? – are answered by finding an instance or a surrogate within the set-theoretic hierarchy. Questions of the form– can such-and-such be proved or disproved? – are answered by investigating what follows or doesn't follow from the axioms of set theory. ([Maddy, 2011], 33-34)

Maddy refers here to set theory as a foundational theory. However, Maddy understands the foundational character of set theory in a modest sense.

> for all mathematical objects and structures, there are set theoretic surrogates and instantiations, and the set theoretic versions of all classical mathematical theorems can be proved from the standard axioms for the theory of sets (ZFC). This includes no claim about the real identity of mathematical objects, no claim to have reduced ontology, no claim to have founded mathematics on something provably free from contradiction or more certain, and no claim that all mathematical methods can be replaced by set theoretic methods. ([Maddy, 1997], 34)

Maddy asked 'What constrains our methodological choices in mathematics?'. From the modest sense of the foundational character of set theory it does not follow that we can answer Maddy's question by a study of set theory. The point is that Maddy's modest sense of the foundational character of set theory allows for cases in which there are constrains on methodological choices on some part of mathematics which are not the constrains of set theory. Conversely, there might also be methodological constrains on set theory which do not constrain other mathematical disciplines. Maddy does not discuss this, perhaps due to the fact that she introduced us to the inquirer in *Second Philosophy*, 10 years after her clarification of the 'modest sense' of the foundational goal. In any case, Maddy's argument why the Second Philosophical inquirer is interested in set theory remains underdeveloped. Nonetheless, set theory is Maddy's (and my) focus.

Let me recapitulate what we have seen thus far. The Second Philosophical inquirer uses the methods of hypothesising and testing to learn about the world. Soon she is interested in science and, because of the role mathematics plays in science, the inquirer becomes interested in mathematics. Mathematics turns out to be a discipline in its own right and set theory is a particularly interesting practice. Two types of questions arise: what is the methodology of set theory, what methods are used to introduce sets, justify axioms and so on? The other question 'is more traditionally philosophical' ([Maddy, 2011], 41): what are sets? how do we come to know about sets? what sort of activity is set theory? The idea is that the Second Philosopher first arms herself with knowledge about set-theoretic practice by answering the first question and then uses this knowledge to engage in the second question. I continue to follow her in her steps.

3.2.1 Naturalising Set Theoretic Practice

In this subsection, I discuss how the Second Philosopher learns about the practice of set theory. In the next chapter, I will point out that the way Maddy learns about set-theoretic practice might not be strong enough to support her general claims about proper conduct in set-theoretic practice. For now, however, I will mostly refrain from criticism in an effort not to muddy the waters here. What follows is hence largely an exposition of Maddy's position.

In order to find out about the methodology of set theory, Maddy constructs a naturalised model of the practice. Her idea is this. Turn to the practice of set theory to give a description of a variety of cases where methodological discussions have been resolved. This generates a picture of the practice that Maddy calls a *naturalised model*. This model is 'purified' if no contributions that turned out to be irrelevant to the resolution of the methodological discussion are included in the naturalised model. Maddy is only interested in purified naturalised models.

> The claim is that this purified and amplified model provides an accurate picture of the actual justificatory structure of contemporary set theory and that this justificatory structure is fully rational. ([Maddy, 1997], 193-194)

Maddy never explicitly constructs a naturalised model of set theoretic practice. Throughout her books one finds descriptions of episodes of historical and contemporary set-theoretic practice, but none of these is ever bundled and called a naturalised model. This makes sense. Maddy's approach to philosophy is ever improving and open-ended; her Second Philosopher is constantly in the process of improving her methods. Any attempt to fully present a naturalised model would run counter to the Second Philosophical approach, as it would fix the model and introduce a staticness where no staticness is wanted. This of course raises the question why we need the concept of a naturalised model in the first place. The answer, I believe, lies in its explanatory power. By presenting this concept, Maddy makes clear what she is after– a rational reconstruction of some relevant parts of the practice, purged from the irrelevant methodological views – even if the Second Philosopher can never actually give a full description of this model. This explanation however is mine; Maddy is silent on this point.

What Maddy is after are stories that reveal some important insights about set-theoretic practice. We find such stories scattered throughout her books. I present five such stories here. The first is taken from *Naturalism in Mathematics* and tells us about the foundational goal of set theory. The other four stem from her more recent *Defending the Axioms* and deal with questions about set introduction and axiom choice.

In [Maddy, 1997], 22, Maddy starts the discussion of the foundational goal with the views of some set theorists on their discipline.

> All branches of mathematics are developed, consciously or unconsciously, in set theory. ([Lévy, 1979], 3)

> Set theory is the foundation of mathematics. All mathematical concepts are defined in terms of primitive notions of set and mem-

> bership. [...] From [the] axioms, all known mathematics can be derived. ([Kunen, 2006], xi)

> Mathematical objects (such as numbers and differentiable functions) can be defined to be certain sets. And the theorems of mathematics (such as the fundamental theorem of calculus) then can be viewed as statements about sets. Furthermore, these theorems will be provable from our axioms. Hence, our axioms provide a sufficient collection of assumptions for the development of the whole of mathematics– a remarkable fact. ([Enderton, 1977], 10-11)

According to Maddy, these set theorists express that they subscribe to the foundational goal: 'the job of set theoretic foundations is to isolate the mathematically relevant features of a mathematical object and to find a set theoretic surrogate with those features'; [Maddy, 1997], 26. The idea is that set theory functions as one arena in which all mathematical objects can be studied 'side-by-side'; [Maddy, 2007], 35. This arena serves, according to Maddy, as the ultimate arbiter for questions of mathematical existence and proof: 'if you want to know if there is a mathematical object of a certain sort, you ask (ultimately) if there is a set theoretic surrogate of that sort; if you want to know if a given statement is provable or disprovable, you mean (ultimately), from the axioms of the theory of sets'; [Maddy, 1997], 26.

Maddy reminds us that the foundational goal was one of the goals of set theory from its Cantorian beginnings and from the above quotes she learns that some set theorists are still trying to provide a foundation for mathematics. *Naturalism in Mathematics* is also an extensive study of this goal of set theory. Maddy draws two morals from it, which she presents as principles: UNIFY and MAXIMISE. If set theory is to serve as a foundation of mathematics, then we should be looking for a single, unified theory of sets; we should UNIFY. As mentioned in the last section, Maddy argues that the pursuit of pure mathematics should not be encumbered in any way and hence set theory should not encumber mathematics: 'the set theoretic arena in which mathematics is to be modelled should be as generous as possible'; ibid., 210-211. This is the principle MAXIMISE. I discuss these two principles in more detail in 4.1.

Maddy's Second Philosopher learns from this that set theorists subscribe to the foundational goal. I cast doubt on this in the next chapter. For my presentation of Maddy's Second Philosophical programme however, we note that Maddy's Second Philosopher takes the foundational goal in the form outlined above to be a goal of set theory.

The second case-study is Cantor's introduction of sets. Maddy gives the well-known story of Cantor's attempt to generalise a theorem on representing functions by trigonometric series. Cantor's investigation of exceptional points then leads to an investigation of collections of these points and the collection of their limit points, the so-called derivative. Maddy cites Ferreirós

> What is really original in this contribution is that Cantor does not consider limit points in isolation, so to say, as Weierstrass had done, but makes a step toward a set-theoretical perspective. As a result, 'set derivation' is conceived as an operation on sets. ([Ferreirós, 2007], 143)

Maddy concludes

> From a methodological point of view, what's happened is that a new type of entity– a set– has been introduced as an effective means toward an explicit and concrete mathematical goal: extending our understanding of trigonometric representations. ([Maddy, 2011], 42)

The title of the third case study is 'Dedekind's introduction of sets'; [Maddy, 2011]., 43. Maddy is well aware that Richard Dedekind's is a 'methodologically rich story' (ibid.) but decides to concentrate on 'one central strand' (ibid.) of this story: Dedekind's insistence to treat sets of numbers as mathematical objects in their own rights. Ernst Kummer had introduced the concept of an 'ideal number', defined as a divisor of certain numbers. For Dedekind, this is not a definition in its own right. He proposes to replace Kummer's ideal number 'by a *noun* for something which actually exists'; Dedekind writing 1877, translation by Avigad, [Ferreirós, 2007], 172. This something is, for Dedekind, the set of numbers Kummer took his ideal number to divide. Whereas Kummer had to represent each ideal number by the numbers it would divide, Dedekind's 'insistence on treating [sets] of numbers [...] as objects in their own right [has] important methodological consequences: it encourages one to speak of arbitrary systems, and allows one to define operations on them in terms of their behavious as sets or predicates, in a manner that is independent of the way in which they are represented.

For example, in 1871, Dedekind defines the least common multiple of two modules to be their intersection, without worrying about how a basis for this intersection can be computed from bases for the initial modules'; Avigad, [Ferreirós, 2007], 173.

As Maddy tells us

> Among Dedekind's goals were general arguments in representation-free terms that would then "'explain" why calculations with and properties of the objects do not depend on these choices of representations'. ([Maddy, 2011], 43)

Important for the Second Philosophical naturalised model of the practice of set theory is here that, just as it was the case with Cantor, Dedekind introduces sets in service to particular mathematical desiderata: representation-free definitions. Maddy then argues that Dedekind's introduction of sets also serves other mathematical goals: 'non-constructive abstract algebra; a rigorous characterisation of continuity to serve as a foundation for analysis and a more general study of continuous structures; a rigorous characterisation of the natural numbers and resulting foundation for arithmetic'; ibid. 45.

The fourth case study turns from the reasons to introduce sets to questions about the adoption of axioms about them. Under the heading 'Zermelo's defence of his axiomatisation', Maddy presents an abridged history of the discussion about Ernst Zermelo's axioms, which focuses exclusively on Zermelo's arguments. According to Maddy, Zermelo had two sorts of evidence for his axioms. The first relies on the intuitive evidence of the axioms. Zermelo's focus is the Axiom of Choice, AC, and part of his argument for this axiom is that it has been implicitly used in the works of such prominent figures as Cantor, Dedekind, Bernstein and others. This shows, according to Zermelo, that there is some intuitive pull towards AC; that this axiom catches something in the informal understanding of the term 'set'. However, so Maddy tells us, 'Zermelo despairs of defining this concept with a precision adequate to the development of set theory'; ibid. 46. Instead, Zermelo appeals to a second kind of evidence, one that can be 'objectively decided': the necessity of the axiom for science.

> no one has the right to prevent the representatives of productive science from continuing to use this 'hypothesis' [i.e. AC]– as one may call it for all I care– and developing its consequences to the greatest extent. ([Zermelo, 1908], 189)

Maddy notes

> This mode of defence goes beyond the observation that his axioms allow the derivation of set theory as it currently exists and the foundational benefits thereof; Zermelo here counts the mathematical fruitfulness of his axioms, their effectiveness and promise, as points in their favour. ([Maddy, 2011], 47)

Maddy notes that Zermelo's two sorts of argument mirror Gödel's distinction between the intrinsic evidence for axioms– their self-evidence, intuitive appeal or being 'part of the concept of set'– and the extrinsic evidence– the effectiveness, fruitfulness and productiveness of the axiom. The Second Philosopher notes that set theorists rely on extrinsic evidence.[6]

The fifth and last story is the case of determinacy. The Axiom of Determinacy, AD, states that every set of reals is determined.[7] The axiom was first presented by Jan Mycielski and Hugo Steinhaus in [Mycielski & Steinhaus, 1962], in which the authors showed that the axiom contradicts the Axiom of Choice. According to Kanamori, '$ZF + AD$ was never widely entertained as a serious alternative to ZFC'; [Kanamori, 2009], 378. Nonetheless, as will become clear below, variations of the axiom have received considerable attention from the set-theoretic community. Two variations are of particular interest, both of which restrict the range of considered sets of reals. In the original formulation, the axiom is a statement about 'all sets of reals'. One variation, the Axiom of Projective Determinacy, PD, is a statement about all projective sets, whereby projective sets are special types of sets of reals.[8] Another variation of AD is the statement that AD holds in a certain generalisation of Gödel's L called $L(\mathbb{R})$. This axiom is denoted by $AD^{L(\mathbb{R})}$. Since $L(\mathbb{R})$ includes all the projective sets, $AD^{L(\mathbb{R})}$ implies PD. In her presentation, Maddy focusses on arguments for $AD^{L(\mathbb{R})}$.

Maddy gives a 'telegraphic summary' ([Maddy, 2011], 49) of the current evidence for $AD^{L(\mathbb{R})}$, which she divides into four classes. The first is that $AD^{L(\mathbb{R})}$ generates a theory which answers 'all the questions about projective

[6] Maddy ignores intrinsic evidence for the better part of [Maddy, 2011]. When she discusses the concept in the later pages of her book, she 'float[s] the heretical suggestion that in fact intrinsic justifications are secondary to the extrinsic' (134). Compare also chapter 5.

[7] 'Determined' is a technical term. See [Jech, 2006], section 33, for details.

[8] For a definition of 'projective set' and a discussion in relation to AD, see [Jech, 2006], section 33.

sets from classical descriptive set theory'; [Steel, 2010], 428. Maddy mentions Peter Koellner's remarks that $AD^{L(\mathbb{R})}$ is implied by the consequences of this theory, which shows that $AD^{L(\mathbb{R})}$ is necessary for this theory; [Koellner, 2006], 170-174. Maddy refers to Steel to point out that 'the theory of projective sets one gets in this way extends in a natural way the theory of low-level projective sets developed by the classical descriptive set theorists using only ZFC'; [Steel, 2010], 428. According to Maddy, there is hence some clear extrinsic evidence for $AD^{L(\mathbb{R})}$: it is powerful and resolves some of the problems the set theorists are having. This quality is also shared by the axiom expressing that every set is constructible: $V = L$. I will discuss this axiom in more detail in the next chapter. Note here that, according to Maddy, $AD^{L(\mathbb{R})}$ resolves the problems the set theorists are having in a natural and useful way whereas $V = L$ does not.

Let me briefly interrupt my presentation of Maddy's argument here to ask what 'natural' means in the setting above. Maddy relies on Steel's and Koellner's judgement, but she does not give their arguments. Maddy is relying on authority here. Steel and Koellner are experts in their field and thus there is reason to rely on their judgement. However, it means that Maddy endorses the position of two set theorists without discussing the views of other set theorists who might disagree. I consider Maddy's neglect of the positions and arguments of those set theorists who do not agree with her philosophical account of set-theoretic practice a weakness of Maddy's philosophical work. I take this theme up again in the next chapter and then again in chapter 6.

I now continue the presentation of the four cases for $AD^{L(\mathbb{R})}$ Maddy gives with the second case. Maddy claims that the equivalence of $AD^{L(\mathbb{R})}$ to the existence of inner models which can accommodate large cardinals counts as evidence in favour of the axiom.[9] According to her,

> $AD^{L(\mathbb{R})}$ inherits the intrinsic and extrinsic evidence for large cardinals, and large cardinals, in turn, gain extrinsic support by implying the determinacy-based account of projective sets. ([Maddy, 2011], 50)

Thirdly, Maddy argues that $AD^{L(\mathbb{R})}$ is also supported by the fact that 'virtually *every* natural theory of sufficiently strong consistency strength actually implies $AD^{L(\mathbb{R})}$'; [Koellner, 2006], 173. Koellner gives two examples:

[9] The inner models can accommodate infinitely many Woodin cardinals; cf. [Jech, 2006], theorems 33.16 and 33.26.

the theory that states that there is an ω_1-dense ideal on ω_1 and the Proper Forcing Axiom, PFA. Both these theories are incompatible,[10] yet both imply $AD^{L(\mathbb{R})}$. Moreover, so Koellner mentions, there are many more examples like these. For him 'definable determinacy is inevitable in that it lies in the overlapping consensus of all sufficiently strong natural mathematical theories'; ibid., 173. Maddy draws the following conclusion

> Given the long-standing foundational goal of set theory and the open-endedness of contemporary pure mathematics, we have good grounds to seek theories of ever-higher consistency strength. If all reasonable theories past a certain point imply $AD^{L(\mathbb{R})}$, this constitutes a strong argument in its favour. ([Maddy, 2011], 51)

Notice that what counts as a 'reasonable theory' here is crucial to the argument because not all theories of high consistency strength imply $AD^{L(\mathbb{R})}$. This connects to the point about natural theories I made above, and I will not discuss this further here.

The last supporting case for $AD^{L(\mathbb{R})}$ Maddy mentions is that it generates a form of generic completeness. Recall that the best contemporary tool for showing independence is forcing. However, in the presence of a proper class of certain large cardinals called Woodin cardinals, the $L(\mathbb{R})$ in the ground model is elementary equivalent to the $L(\mathbb{R})$ in all its forcing extensions. That is, in the presence of $AD^{L(\mathbb{R})}$ (and a proper class of Woodin cardinals) forcing cannot succeed in showing that some questions about $L(\mathbb{R})$ are unsolvable. Conversely, if there is a proper class of inaccessibles (the weakest of the large cardinals) and the $L(\mathbb{R})$ in the ground model is elementary equivalent to the $L(\mathbb{R})$ in all forcing extensions, then $AD^{L(\mathbb{R})}$ holds.[11] That is, $AD^{L(\mathbb{R})}$ implies and is implied by this kind of generic completeness. Maddy states:

> Given that we want our theory of sets to be as decisive as possible, within the limitations of Gödel's theorems, generic completeness would appear a welcome feature of determinacy theory. ([Maddy, 2011], 51)

This conclues the partial construction of Maddy's naturalised model of the practice of set theory. It is time to take stock of what we have learned.

[10] *For connoisseurs*: PFA implies Martin's Axiom, MA, as well as $2^{\aleph_0} = \aleph_2$. $MA + \neg CH$ in turn implies that there is no ω_1-dense ideal on ω_1.

[11] For a discussion of these results, see [Koellner, 2006], 171-173.

We have seen that, according to Maddy's Second Philosopher, set theorists subscribe to the foundational goal. Furthermore, we saw Maddy's argument that sets were originally posited in service of mathematical goals (extending our understanding of trigonometric representations; representation-free definitions; non-constructive abstract algebra; a rigorous characterisation of continuity to serve as a foundation for analysis and a more general study of continuous structures; a rigorous characterisation of the natural numbers and resulting foundation for arithmetic). That is: according to Maddy set theorists have a goal and the discussed cases provide effective means to achieve this goal. Similarly for the case of axiom candidates. Maddy counted the fruitfulness of the Axiom of Choice in its favour because it leads to 'productive science'; [Zermelo, 1908], 189. In the case of determinacy, we have seen Maddy's argument that the relevant axiom produces solutions to old problems that are desirable. And so on. Maddy concludes

> Given what set theory is intended to do, relying on considerations of these sorts is a perfectly rational way to proceed: embrace effective means toward desired ends. ([Maddy, 2011], 52)

Thus, Maddy attributes means-ends reasoning to set-theoretic practice. However, what is set theory intended to do? Is it not in the business of describing, say, an objective mathematical realm, some true universe of sets? Turning again to the practice, Maddy tells us that many practitioners do in fact surround their arguments with metaphysical claims. The example of Dedekind is particularly striking: for him, the natural numbers are 'free creations of the human mind'; [Dedekind, 1888], 791. Thus, for Dedekind, we are not discovering the natural numbers, we are creating them and this has consequences for the methods we use to do so. However, as Maddy notes, other mathematicians hold other metaphysical beliefs. Hence, Maddy follows, there should be considerable disagreement on the proper methods of mathematics. For example, the history of the Axiom of Choice shows such disagreement. However, this methodological debate has been settled according to Maddy, as well as many other methodological debates in mathematics. Maddy puts it thus:

> My point is simply that the methodological debates have been settled, but the philosophical debates have not, from which it follows that the methodological debates have not been settled on the basis of the philosophical considerations. ([Maddy, 1997], 191)

Instead, so Maddy continues, to answer questions about mathematical methodology, do not turn to philosophy but turn instead 'to the needs and goals of mathematics itself'; ibid..

Notice that Maddy's Second Philosopher has studied mainly those goals which have already been achieved by the set-theoretic community. The exception is the foundational goal.[12] In the next chapter, I will show that not all set theorists agree with Maddy's Second Philosophical argument for her version of this goal. In my conclusion in chapter 6, I pick up this train of thought to argue that our philosophical programmes to study mathematics ought to provide us with an anthropology of the contemporary and unachieved goals of the mathematical practices we are studying.

Because the Second Philosopher claims that means-ends reasoning is rational set-theoretic reasoning, goals play an important role in the Second Philosophical programme. According to Maddy, the resolution of the methodological debates rests on the goals of set theory, not some philosophical view.

> Given the wide range of views mathematicians tend to hold on [philosophical] matters, it seems unlikely that the many analysts, algebraists and set theorists ultimately led to embrace sets would all agree on any single conception of the nature of mathematical objects in general, or of sets in particular; the Second Philosopher concludes that such remarks should be treated as colourful asides or heuristic aides, but not as part of the evidential structure of the subject. What matters for her methodological purposes is that all concerned do feel the force of the kinds of considerations we've been focusing on here; these are the shared convictions that actually drive the practice. ([Maddy, 2011], 53)

Maddy denies here the primacy of philosophy over mathematical goals. I take up this point in chapter 5 and argue that there are instances of a reciprocal relationship between the metaphysical views of the set theorists and the way they practice set theory.

I now come back to my presentation of the Second Philosophical programme. Maddy argues that the kind of reasoning that is convincing in set-theoretic practice when discussing foundational issues is means-ends reasoning: if the means help to satisfy our goals, then it is rational to accept

[12] The case for determinacy is special in this regard. I discuss this in more detail in chapter 6.

the means. The next subsection explores what makes our mathematical goals proper.

3.2.2 Mathematical Depth

The Second Philosophical inquirer set out on her open-ended quest to learn about what the world is like by a broadly speaking scientific method: hypothesising, testing, confirming, rejecting old beliefs and so on. When she encountered the use of mathematics in the scientific theories she has been using, her interest in mathematics stirs. Realising that the applied mathematics is only a part of the larger realm of the practice of mathematics, the inquirer investigates this practice. In our case, the practice of interest is set theory. This practice however has norms quite different from the norms the inquirer has encountered thus far.

> For example, [the Second Philosophical inquirer] isn't accustomed to embracing new entities to increase her expressive power (as in Cantor) or to encourage definitions of a certain desirable kind (as in Dedekind), or to rejecting a theory because it produces less interesting consequences (as with the alternative to determinacy's theory of projective sets that results from $V = L$). ([Maddy, 2011], 53)

Might not the inquirer now think that her usual methods can criticise the methods of mathematics? Not according to Maddy. Remember Maddy's principle that whenever philosophy and mathematics clash, it is philosophy that must give. Therefore, no matter how reasonable and philosophically well-founded the norms of our inquirer are before she studies mathematics, once she realises that mathematics is fruitfully used in science and that it has other norms than science, she readily accepts these new norms. Mathematics then has its own norms and goals and its own criteria for the appropriateness of its methods and is in no need of external, philosophical justification.

The Second Philosophical inquirer is tracking two questions: 1) what are the proper methods of set theory? 2) why are they reliable? Having given the five examples presented above and a brief discussion to the extent that the inquirer accepts mathematical norms and goals as in no need of philosophical criticism or backing, Maddy somewhat surprisingly writes

> If all [the inquirer] ultimately cared about were answering questions of the first type– what are the proper set-theoretic methods?– she'd now be done ([Maddy, 2011], 54)

Surely, Maddy does not intend this to be taken literally. After all, she has told us that her Second Philosophical approach is an open-ended one, up for constant improvement. One would hence suppose that this holds also true for her answer to questions of the first type. The most favourable reading of the above quote seems to be that Maddy simply intends here that the Second Philosopher has answered the first question to her satisfaction at this point and can now press on to the second type of question. Let me skip over this lightly here– I discuss these issues in detail in the following chapters– and follow Maddy as she turns her attention to the second type of question: what makes the proper methods of set theory, those we have learned about from the naturalised model, reliable? 'Do they successfully track the existence of sets and their properties and relations?'; [Maddy, 2011], 54. Thus, Maddy is interested in the kinds of ontological and epistemological questions of what Mancosu calls 'mainstream philosophy of mathematics'; cf. chapter 1.

To answer her questions, Maddy spends the best part of her [Maddy, 2011] to develop three metaphysical positions that, according to her, can account for the reliability of the set-theoretic methods. In this thesis, I am not going to challenge Maddy on these metaphysical views; I study Maddy out of the motivation to investigate set-theoretic practice and the lessons we may learn from this investigation about the features we should demand of our philosophical programmes to study mathematics; cf. 1.6. Thus, I will spend little time on Maddy's metaphysical proposals. Nonetheless, they need to be mentioned because they are intimately connected with Maddy's conclusions about set-theoretic practice and they lead us to a concept that will receive some attention in this thesis: mathematical depth.

Before the Second Philosopher can answer whether set-theoretic practice successfully tracks the existence of sets, she needs to answer the question whether there are sets at all. Maddy's approach to this is to assume, for now, that sets exist and go from there. The result is what she calls 'Thin Realism'. This helps to individuate the concept of mathematical depth. From there Maddy can then construct a story which does without the existence of sets; this is her 'Arealism'. Maddy's third metaphysical proposal is 'Objectivism', the position that ultimately it does not matter whether we side with Thin Realism or Arealism. I will roughly follow Maddy's argumentative structure and explain her three metaphysical proposals as I go along.

For now, we are assuming that there are sets. Are the set-theoretic methods reliable in tracking them in the above sense? According to Maddy, this is not a question that can be answered 'within set theory or pure mathematics proper'; ibid. 54. 'We are, in effect, standing within empirical science, asking a question about a particular human practice'; ibid. However, according to Maddy, this external view does not supply means for criticism of the set-theoretic methods. 'Though [the Second Philosophical inquirer] is viewing the practice from her external, scientific perspective, as a human activity, she sees no opening for the familiar tools of that perspective to provide supports, correctives, or supplements to the actual justificatory practices of set theory. She has no grounds to question the very procedures that do such a good job of delivering truths'; ibid. 55. Maddy concludes that the methods of set theory are reliable guides to the facts about sets. The question that remains to be answered is why these methods are reliable; what is the metaphysical nature of sets such that the set-theoretic methods reliably track it?

Maddy develops her answer to the above why-question from an opposition to what she calls Robust Realism. For the Robust Realist, there is some objective realm of abstract objects (or structures or else), which mathematical axioms describe. In this realm, all statements of the relevant kind (set-theoretic statements in our case) are either true or false. One prominent proponent of Robust Realism is Gödel: 'the set-theoretic concepts and theorems describe some well-determined reality, in which Cantor's conjecture must be either true or false'; [Gödel, 1947], 260.

[Benacerraf, 1973] asked how we could gain reliable information about this causally isolated realm. John Burgess and Gideon Rosen talk about a 'great gulf' ([Burgess & Rosen, 1997], 29) between us and this realm of abstracta. They remind us that the Robust Realist owes us 'a detailed explanation how anything we do here can provide us with knowledge of what is going on over there, on the other side of the great gulf' (ibid.).

Maddy's Second Philosophical inquirer objects to Robust Realism even before Benacerraf's epistemological challenge can get off the ground. From the naturalistic model given above, she has learned that mathematical reasoning relies on means-ends reasoning: if, say, you want to formulate and prove a stronger theorem on trigonometric functions (as Cantor did) and the introduction of sets allows you to do so, then it is reasonable to introduce sets. Similarly, if you want a certain type of theory (say a generically complete theory) and a certain axiom allows you to form such a theory, then it might be reasonable to assume the axiom (given that it does not conflict with other desiderata). Nevertheless, means-ends reasoning does not suffice as justification for the introduction of new entities for the Robust Realist. For all he knows, the other side of the great gulf could be unpleasantly uncooperative. Think of it this way: just because some assumptions lead to an intellectually pleasing theory about the physical world does not mean that these assumptions are true. So why should this be the case for set theory? Just because certain sets (e.g. large cardinals) solve a problem for us here does not mean that they actually exist on the other side of that great gulf, and just because we fancy a certain type of nice theory does not mean that this nice theory actually describes the reality on the other side of the great gulf. The Robust Realist hence needs a further argument of a different kind; something that ensures him that what the mathematician is doing actually tracks what is going on over there on the other side of the great gulf.

> To the Second Philosopher, this hesitation seems misplaced: why should perfectly sound mathematical reasoning require supplementation? Hasn't something gone wrong when rational mathematical methods are called into question in this way? ([Maddy, 2011], 58)

Benacerraf's question suggests that the Robust Realist needs to supplement mathematical arguments with an epistemic account of why these arguments track what is going on on the other side of the gulf. Maddy's point is that it seems odd to demand that mathematicians do more than they currently do. True to her principle, Maddy finds fault with the philosophy rather than the mathematics here.

In conclusion, Maddy regards Robust Realism as unsuited to answer the question why set-theoretic methods are reliable. But then, what sort of things are sets? 'Under the circumstances, the Second Philosopher is naturally inclined to entertain the simplest hypothesis that accounts for the data: sets are just the sort of thing set theory describes; this is all there is to them; for questions about sets, set theory is the only relevant authority'; [Maddy, 2011], 62. According to Maddy, Robust Realism tells a story which goes 'well beyond' (ibid., 62) what set theory tells us; cf. the point about the uncooperative other side of the great gulf above. Her proposal seems, in contrast, rather thin. Thus, she names her form of realism, which regards set theory as the 'only relevant authority', Thin Realism.

Maddy spends a whole chapter on fleshing out her Thin Realism. However, as mentioned above, these details will not play a role in this thesis. Thus, I skip over them here and move directly to a worry that might be raised in connection to her position.

According to Maddy, Thin Realism 'not only squares with the Second Philosopher's austere and hard-nosed scientism, it actually seems to arise naturally from it'; ibid. 77. Nonetheless, she acknowledges the worry that her position might be 'all too easy, that it rests on some sleight of hand', that connecting the metaphysical nature of sets and set-theoretic methods in the way she does 'invite[s] the suspicion that sets aren't fully real, that they're a kind of shadow play thrown up by our ways of doing things, by our mathematical decisions'; ibid., 77. According to her, 'the position would be considerably more compelling if it offered some explanation of why sets are this way'; ibid., 77.

This brings us back to Maddy's Second Philosophical inquirer. She wants to find out about how the world is. Her hard-nosed scientism leads her to believe that there is a world to find out about and that this world is made in a certain way. There are hence some facts that are true about this world. Some of these facts are ontological: Maddy's inquirer holds that there are trees in this world, there are humans, animals and chocolate cake. These are the facts about what the world is made of. Then there are the facts about how the world is. For example, that the ball falls to the ground once I let go of it is a fact about how the world is. I might describe this phenomenon in different ways ('gravity!' or 'Godly intervention' or whathaveyou), but the fact remains. Similarly for mathematics: the world is in such a way that the large cardinal axioms can be ordered in a linear hierarchy; the world is in such a way that appealing to transfinite sets allows for generalisations of theorems about trigonometric series. It is not 'up to us', to use Maddy's expression, that the concept of a group is 'getting at the important similarities between structures in widely different areas of mathematics'; [Maddy, 2011], 79-80. It is not up to us because there simply is a fact of the matter how the world is. And this fact remains regardless of what we think the world is made of.

According to Maddy, these facts about how the world is in regards to mathematics extend beyond the merely logical connections. The definition of a group stands out from all the slight variations of the concept of group, even though logic does not differentiate between all these variations in definition (assuming their internal consistency). What makes the definition of a group special is that it is getting at similarities of particular structures that share otherwise no connection. That it does is a fact about how the world is, and what it allows the mathematician to do is to develop a rich and fruitful theory. That is: the world is in a certain way and because the world is this way, the concept of a group is useful.

Because these facts about how the world is are not up to us, they are not subjective. 'I might be fond of a certain type of mathematical theorem, but my idiosyncratic preference doesn't make some conceptual or axiomatic means toward that goal into deep or fruitful or effective mathematics'; ibid. 81. If the world is such that the definition you have come up with is fruitless, then, according to Maddy, no amount of hard work will make it fruitful. Moreover, no matter how much you might dislike a fruitful idea, your emotional position towards it does not make it any less fruitful.

Thus, for Maddy, there is an objective reality and some facts about it are visible through mathematics. Nevertheless, notice that this is not an objective reality on the other side of some great gulf. It is, quite simply, the objective reality the Second Philosophical inquirer has been learning about all along. She also set out to find out about the way the world is. It turns out that the world is in such a way that our definition of a group gets at similarities which can be fruitfully put to use in a way that other definitions do not. Thus far, we are operating under the assumption that sets exist in the sense of Thin Realism. Notice however that this ontological assumption plays no part in the above. I come back to this below.

Some bits of mathematics can get at these facts how the world is. The world is in such a way that allowing for transfinite numbers allows to generate an expanded theorem on trigonometric series. The world is in such a way that the introduction of sets allows for representation-free definitions. The world is in such a way that the Axiom of Choice has a vast array of important implications not shared by other axioms. And the world is in such a way that allowing for $AD^{L(\mathbb{R})}$ answers 'all the questions about projective sets from classical descriptive set theory'; [Steel, 2010], 428. I have discussed these four cases in the naturalised model (in order: Cantor, Dedekind, Zermelo and the case for determinacy). In all cases, the mathematics involved has a special virtue: it tracks some fact about how the world is. What we are getting at here is what Mady calls *mathematical depth*.

Maddy uses the terms 'mathematical depth, mathematical fruitfulness, mathematical effectiveness, mathematical importance, mathematical productivity and so on [...] interchangeably' and uses them as 'a catch-all for the various kinds of special virtues we clearly perceive in our illustrative examples of concept-formation and axiom-choice' ([Maddy, 2011], 81).[13] Mathematical depth (and its relatives) then is the virtue of a piece of mathematics of tracking some fact about how the world is, of 'track[ing] deep mathematical strains that the [other pieces of mathematics] miss'; [Maddy, 2011], 79.

We have just seen that according to Maddy the works of Cantor, Dedekind, Zermelo and the work on determinacy all have this 'special virtue' of getting at the facts about how the world is; they are all mathematically deep. This means that the methods Maddy has told us about in the discussion of the naturalised model above lead to deep mathematics. However, deep mathematics is that kind of mathematics that reveals how the world is, i.e. it does exactly what the Second Philosophical inquirer set out to do. Therefore, the proper methods of set theory do the job the Second Philosopher wants done: they allow her to find out something about how the world is. And this answers her question why the proper methods of set theory are reliable: they track the strains of mathematical depth.

Maddy introduces us to this idea of mathematical depth by presenting us with Immanuel Kant. For Kant, the three-sidedness of a triangle is an analytic truth about the triangle, i.e. the three-sidedness is (covertly) contained in the concept of 'triangle'. But that the inner angles add up to two right angles is a synthetic truth, i.e. the inner angle sum of a triangle lies outside the concept of the triangle itself. Hence, to learn about the inner angle sum of triangles, we need to bring intuition to the concept: either empirical intuition in drawing an actual diagram or the kind of pure intuition that is involved when we construct diagrams in our visual imagination. This means that learning about the inner angle sum of triangles we do not only learn about the concept but we are also constrained by the nature of space itself. Moreover, space, so Kant thought, is Euclidean and this is why the inner angle sum is equal to two right angles. 'Of course, this picture of geometric knowledge hasn't survived subsequent progress in logic, mathematics, and natural science'; [Maddy, 2011], 78. The reason Maddy presents it is that Kant is tracking something beyond mere logical connections. According to Maddy, Kant is tracking the nature of space. As we know by now, Maddy replaces what is tracked with the facts about how the world is. So why tell the story about Kant? Because what is tracked for Kant is intuition, something traceable to ourselves. Not so for the kind of facts Maddy is after: 'the facts that constrain our set-theoretic methods [...] are not traceable to ourselves

[13] In most citations, I change all of Maddy's variations to mathematical depth.

as subjects' (ibid. 81).

Maddy 'anchors' (ibid. 82) mathematical goals in the objectivity of what the mathematics traces. The examples we have been rehearsing are not mathematically deep because they fulfilled the goals of the relevant mathematicians. 'Cantor may have wished to expand his theorem on the uniqueness of trigonometric representations, but if this theorem hadn't formed part of a larger enterprise of real mathematical importance, his one isolated result wouldn't have constituted such compelling evidence for the existence of sets'; ibid., 82. Maddy draws an important conclusion:

> our mathematical goals are only proper insofar as satisfying them furthers our grasp of the underlying strains of mathematical [depth]. (ibid. 82)

Of course, there are many such proper goals and we have a choice amongst them. However, our choice is not just amongst any goal. 'The goals are answerable to the facts of mathematical depth'; ibid., 82.

> So there is a well-documented objective reality underlying Thin Realism, what I've been loosely calling the facts of mathematical depth. The fundamental nature of sets (and perhaps all mathematical objects) is to serve as means for tapping into that well; this is simply what they are. And since set-theoretic methods are themselves tuned to detecting these same contours, they're perfectly suited to telling us about sets. ([Maddy, 2011], 83)

We now have a reason why set-theoretic methods are proper. We started off by assuming that sets exist. Discarding Robust Realism led Maddy to embrace Thin Realism, a form of realism that accepts set theory as the only authority to tell us about sets. And the discussion about mathematical depth has shown that the set-theoretic methods track the kind of objective reality that is necessary for a realist account of mathematics.

I now return to the assumption that sets exist. Maddy's Second Philosophical inquirer accepts that the methods of set theory she has identified are proper for the set-theoretic enterprise. However, they are also quite different from the methods she is used to; 'claims aren't supported by her familiar observation, experimentation, theory-formation, and so on'; [Maddy, 2011], 88. Rather than concluding that set theory is nonetheless a body of truths, could the Second Philosopher not rest content with a description along the lines that 'just as the concept of group is tailored to the mathematical tasks set for it, the development of set theory is constrained by its own particular range of mathematical goals'; ibid. 89? Maddy thinks we can. 'Such a Second Philosopher would see no reason to think that sets exist or that set-theoretic claims are true– her well-developed methods of confirming existence and truth aren't even in play here– but she does regard set theory, and pure mathematics with it, as a spectacularly successful enterprise, unlike any other'; ibid. This is Maddy's Arealism.

As with Thin Realism, Maddy considerably expands her elaborations on Arealism and just as with Thin Realism I point out that these elaborations play no role in this thesis and I hence do not give them here. I am interested here in the fact that Maddy presents Arealism as a serious metaphysical position. From this we can learn something about mathematical depth.

> The Thin Realist holds that sets exist and set theory is a body of truths, and the Arealist denies both. But despite their disagreements over truth and existence, the Thin Realist and the Arealist are indistinguishable at the level of method. [...] This methodological agreement reflects a deeper metaphysical bond: the objective facts that underlie these two positions are exactly the same, namely, the topography of mathematical depth [...]. For the Thin Realist, sets are the things that mark these contours; set-theoretic methods are designed to track them. For the Arealist, these same contours are what motivate and guide her elaboration of the theory of sets; she can go wrong as easily as the Thin Realist if she fails to detect the genuine mathematical virtues in play. For both positions, the development of set theory responds to an objective reality– and indeed to the vary same objective reality. ([Maddy, 2011], 100)

What we learn from this is that, according to Maddy, mathematical depth is not dependent on the existence of sets. What really matters is depth, not existence.

If existence is not the issue, then our Second Philosophical inquirer might be free either to take the set-theoretic statements at face value and assume that set-theoretic existence statements talk about something that exists and accept that set theory has introduced some new methods for finding out about what there is in this world– this is Thin Realism. Or the Second Philosophical inquirer could view the set-theoretic methods as too different from her usual methods of observation, experimentation and so on and conclude that whilst being an extremely fruitful enterprise, set theory is not in the business of telling us about what there is– this is Arealism. According to Maddy, both choices are possible; the Second Philosopher is free to accept either Thin Realism or Arealism. And this insight, that the Second Philosopher is free to choose at this point, is a position Maddy calls Objectivism. For the Objectivist 'Thin Realism and Arealism are equally accurate, second-philosophical descriptions of the nature of pure mathematics. They are alternative ways of expressing the very same account of objective facts that underlie mathematical practice'; [Maddy, 2011], 112.

Chapter 4

The Hamkins Story

So far in this thesis I have argued as follows. The question which features we should demand of our philosophical programmes to study mathematics is a pressing question for philosophers of mathematics. In order to bring out three of these features, I have presented Maddy's Second Philosophical programme. The aim of this chapter and the next is to present two stories about set-theoretic practice which are disharmonious with Maddy's presentation thereof. In this chapter, I show that there is a relevant set theorist who does not agree that the foundational goal of set theory implies the search for a unique theory of sets. In the next chapter, chapter 5, I show that there are instances of reciprocal relationship between mathematics and metaphysics. I will show that these stories do not easily align with Maddy's Second Philosophical programme. In chapter 6, I will refer to these two stories about set-theoretic practice in order to bring out the three features we should demand of our programmes to philosophically investigate mathematics.

Maddy's Second Philosopher studies the methodological debates of set-theoretic practice that are regarded as resolved by 'the set theorists'. Such general claims about a practice face sample-size problems: how many set theorists need to regard a debate as resolved for the philosopher to make the general claim that the debate is resolved? Maddy does not discuss this point. What she does is cite, quote and refer to some of the leading set theorists of our time when it comes to these matters. Maddy refers to the views of Kenneth Kunen, Herbert Enderton, Azriel Lévy and Yiannis Moschovakis in regards to the foundational goal ([Maddy, 1997], 22, 25-26); the case for determinacy relies on quotes from John Steel, Moschovakis, Peter Koellner and Hugh Woodin ([Maddy, 2011], 47-51). Interestingly, the cases of set-introduction (Cantor, Dedekind) and axiom defence (Zermelo) rely not on the views of contemporary set theorists in Maddy's presentation. Instead, she falls back to respected historians of mathematics, such as Ferreirós and Jeremy Avigad, for set-introduction ([Maddy, 2011], 41-45). Regarding the axiom of choice, Maddy presents Zermelo's reasons for accepting it; that the set theorists today accept it and why they accept it is not discussed ([Maddy, 2011], 45-47).

In this thesis, I do not dispute Maddy's presentation of the historical cases. I concentrate on those cases in which Maddy relies on the views expressed by contemporary set theorists: the foundational goal and the case for determinacy. According to Maddy, it is 'fairly uncontroversial' that 'set theory hopes to provide a foundation for classical mathematics'; [Maddy, 2007], 354. And with regards to determinacy she writes 'the current case for determinacy has blossomed so impressively that many would agree with Woodin's assessment: 'Projective determinacy is the *correct* axiom for the projective sets"; [Maddy, 2011], 51.[1]

In the next two sections, I argue that Maddy does not describe the practice of set theory as a whole but rather the practice of a particular group of set theorists. To do this, I first discuss in more detail Maddy's story about the foundational goal of set theory in section 4.1. I then give a presentation of Joel David Hamkins' multiverse view in section 4.2. The aim here is twofold. Firstly, Hamkins serves as an example of a noteworthy set theorist whose views are neither captured nor discussed by Maddy. Secondly, I will draw on the Hamkins story in the next chapter in order to tease out some instances of the reciprocal relationship between mathematics and metaphysics. The current chapter ends with a possible answer the Second Philosopher might give in regards to the Hamkins story in section 4.4.

4.1 The foundational goal

Recall that Maddy is a proponent of means-ends reasoning. She gives us 'simple counsel': 'identify the goals [of a practice] and evaluate the methods by their relations to those goals'; [Maddy, 1997], 194. This means that we need a way to identify the goals of a mathematical practice. The Second Philosophical programme gives us no explicit story how to do so. Nonetheless, Maddy has identified one such goal: the foundational goal of set theory. As

[1] The quote from Woodin is taken from [Woodin, 2001a], 575.

we saw in 3.2.1, the Second Philosopher refers to the views of the respected set theorists Lévy, Enderton and Kunen. These set theorists hold that the foundational goal is, in fact, a goal of set theory. There is, however, a question what this means.

In [Maddy, 1997], Maddy discusses various philosophical understandings of the foundational goal, such as the ontological reduction of mathematical entities to sets for example. According to Maddy however, the foundational goal is not a goal of set theory because of philosophical benefits. Rather, the mathematical benefits of having a foundational theory are the reason why set theory has the foundational goal. The story about these mathematical benefits is developed in more detail in [Maddy, 2011]. There, Maddy tells the story how mathematics and science became divorced. This led to a renewed emphasis on rigour, the central tool of which was axiomatisation. Maddy continues that simply laying down a list of axioms is not enough to establish that these axioms describe a genuine structure. It gradually emerged that 'set theory provides a natural arena in which to interpret the myriad structural descriptions of mathematics'; ibid., 32. Today, so Maddy tells us, set theory has solidified its role as a foundation of set theory. She claims

> Questions of the form– is there a structure or a mathematical object like this?– are answered by finding an instance or a surrogate within the set-theoretic hierarchy. Questions of the form– can such-and-such be proved or disproved?– are answered by investigating what follows or doesn't follow from the axioms of set theory. [Maddy, 2011], 33-34.[2]

With this goal given, the question is what the effective means to reach this goal are; what are the methodological consequences for a mathematical practice with such a goal? Maddy answers:

> if your aim is to provide a single system in which all objects and structures of mathematics can be modelled or instantiated, then you must aim for a single, fundamental theory of sets. ([Maddy, 1997], 209)

Notice what has happened here. Maddy, in the role of the Second Philosopher, learns from three set theorists that set theory has a foundational goal. Two questions arise: why does set theory have this goal and how is it to be

[2]Compare here also Maddy's 'modest sense' of the foundational goal explained in 3.2.1.

understood? Maddy answers these questions by pointing out what is mathematically beneficial. That is, Maddy answers to these questions here. She does not tell us about the reasons the set theorists give why set theory has the foundational goal. Also, the argument how the goal is to be understood is hers, not that of any set theorists. This point will become important in chapter 6.

In this chapter, I will present a set theorist, Hamkins, who agrees that set theory has a foundational goal but disagrees with Maddy about what this means for set theory. In the next chapter, I will present another set theorist, Woodin, who agrees with Maddy that set theory should aim for a single, fundamental theory of sets, but whose argument for this position does not rely on the foundational goal. These points will become relevant in my argument in chapter 6 that a philosophical programme to study mathematics needs an anthropology of goals.

I now return to the presentation of Maddy's thoughts on the foundational goal. According to her, two 'methodological morals' or 'methodological maxim[s]' (ibid., 209) follow from the foundational goal of set theory. One is that set theorists should aim for one unified theory of sets. Recall that the set theorists currently know of many different theories of sets: $ZFC + CH$, $ZFC + \neg CH$, $ZFC+$'there is a measurable cardinal' and so on. What Maddy tells us here is that because of the foundational goal, the set theorist should aim to identify a single such theory as *the* theory of sets. This is her maxim UNIFY.

The second maxim relies on Maddy's view that 'contemporary pure mathematics is pursued on the assumption that mathematicians should be free to investigate any and all objects, structures, and theories that capture their mathematical interest'; ibid., 210. We have seen the argument for this in the last chapter: it is useful to have a 'stocked warehouse' full of mathematics which is potentially useful to the natural sciences.

> If mathematics is to be allowed to expand freely in this way, and if set theory is to play the hoped-for foundational role, then set theory should not impose any limitations of its own: the set theoretic arena in which mathematics is to be modelled should be as generous as possible, the set theoretic axioms from which mathematical theorems are to be proved should be as powerful and fruitful as possible. ([Maddy, 1997], 210-211)

This is Maddy's maxim MAXIMISE.

In a sense, adding any axiom A to our axioms of set theory imposes a limitation: $\neg A$ is now no longer a player in the resulting set theoretic arena– 'CONSISTENCY is an overriding maxim'; ibid., 216.[3] However, this is not what Maddy means by 'should not impose any limitations'. Today, Maddy would probably try to phrase this in terms of mathematical depth, but when she wrote *Naturalism in Mathematics* in 1997 she did not have this terminology at her disposal (Maddy introduced mathematical depth as a technical term in [Maddy, 2011]). Instead, she has Steel to help her try to formalise the idea in the language of set theory.

Maddy starts with a crude definition of what it means for theory T to maximise over theory T' in terms of (a non-standard understanding of) inner models. She then introduces (some of) the (sometimes very artificial) counter-examples that Steel invented. The idea of these counter-examples is that some theory should intuitively maximise over the other but does not according to the formalism, or that some theory should intuitively not maximise over the other but does according to the formalism. This helps Maddy to improve upon her notion of 'maximise'. In the end, Maddy presents us with a formal criterion, but even here

> Steel's ingenious examples show that the formal criterion for restrictiveness [a term central to Maddy's formalisation of MAXIMISE] is not enough by itself; it must be supplemented by useful but nevertheless imprecise notions of the 'optimality' of inner model interpretations and 'attractiveness' of alternative theories. ([Maddy, 1997], 231)

The reason why I skip over all of this so lightly is because in her later writing Maddy discards her formalisation of the principle.

> The differences between my version [of a formalisation of MAXIMISE] and Steel's [as presented in ([Steel, 2010], 423)] are largely due to my not entirely successful effort to spell out what counts as a 'natural' extension of ZFC and a 'natural' interpretation. See [Löwe, 2001] and [Löwe, 2003] for more. ([Maddy, 2007], 359, n.1)

[3]The given quotation is the only instance where Maddy mentions the CONSISTENCY maxim. There is no discussion whether set theorists hold it and why. Perhaps the point is too obvious to be discussed.

The formalisation might not have been fully successful, but Maddy's normative claim still stands: because of the foundational goal of set theory, the set theorists ought to look for theories that are as mathematically fruitful and as mathematically powerful as possible. Now recall that Maddy uses 'mathematical depth, mathematical fruitfulness, mathematical effectiveness, mathematical importance, mathematical productivity and so on [...] interchangeably' ([Maddy, 2011], 81) to see that MAXIMISE is trying to get at the facts about mathematical depth. Maybe we could phrase it like this: theory T' MAXIMISES over theory T if T' is better at tracking the facts of mathematical depth. However, this is my formulation, not Maddy's.

There is a tension between MAXIMISE and UNIFY. The theories $ZFC + V = L$ and $ZFC + MC$ (where MC stands for 'there is a measurable cardinal') are mutually exclusive (MC implies $V \neq L$). How is the set theorist to choose between these theories? MAXIMISE seems to counsel to accept both theories in an effort not to block any fruitful route of mathematics. This however conflicts with UNIFY, which asks us to look for a single theory. 'The subtlety of applying MAXIMISE and UNIFY will come in the effort to satisfy both admonitions at once'; [Maddy, 1997], 211. Indeed, Maddy uses her formal criterion for MAXIMISE to show that in the case of the two above mentioned theories, it is possible to satisfy both admonitions simultaneously. The idea is this. For any model M of $ZFC + MC$ there is an inner model of M (namely: L) which is a model of $ZFC + V = L$. Thus, we can recover $ZFC + V = L$ in $ZFC + MC$ by restricting every formula to L. That is, working within $ZFC + MC$, instead of regarding the formula ϕ, we consider ϕ^L, i.e. the formula ϕ with all its quantifiers restricted to L. It now holds that $ZFC + V = L$ proves ϕ if and only if $ZFC + MC$ proves ϕ^L.[4] In this sense, $ZFC + MC$ can emulate $ZFC + V = L$. Hence, there is a sense in which accepting $ZFC + MC$ does not lose anything we could get from $ZFC + V = L$: we can simply emulate the latter theory in the former. Maddy now claims that the converse direction does not work: '$ZFC + V = L$ cannot recapture [$ZFC + MC$] in a similar way'; [Maddy, 2007], 359. This requires some illumination.

What does Maddy mean by 'similar way'? The idea is that there is a natural interpretation for one theory in the other but not vice versa. But then, what is a 'natural interpretation'? As mentioned above, Maddy had

[4] As Hamkins points out, this only holds under the assumption of an absolute background concept of the ordinals; see also below.

given a formal definition of the concept in her 1997 book. But ten years later, in her [Maddy, 2007] she concedes that her attempt was not entirely successful; see above quotation. Instead, she now relies on John Steel, who makes an argument very similar to the above about the recoverability of the theories $ZFC + V = L$ and $ZFC + MC$ in one another; [Steel, 2010]. However, Steel does not define natural interpretation. Indeed, what a natural interpretation is is an open problem, and it is not my intention here to engage in this debate. I mention this point mainly because later on in this chapter I will discuss Hamkins' argument that, conversely to Maddy and Steel, $ZFC + MC$ can in fact be recovered in $ZFC + V = L$. More on this below.

Maddy argues that once we accept $ZFC + V = L$ we lose the mathematical fruitfulness of $ZFC + MC$. This means that $ZFC + V = L$ is restrictive. For Maddy, $ZFC + MC$ as not restrictive in the same way. She concludes: $ZFC + MC$ properly MAXIMISES over $ZFC + V = L$.[5] This is Maddy's argument against $V = L$. Steel defends an argument very similar to Maddy's; [Steel, 2004], [Steel, 2010].

4.2 Hamkins' Multiverse View

In the last section, we saw that according to Maddy there is 'one fairly uncontroversial motivation: set theory hopes to provide a foundation for classical mathematics'; [Maddy, 2007], 354. According to Maddy, the principles MAXIMISE and UNIFY follow from the foundational goal of set theory. In this section, I present a set theorist who disagrees, in part, with Maddy's picture: Joel David Hamkins. Hamkins has recently published two papers, in which he defends a view which strongly disagrees with UNIFY; [Hamkins, 2011], [Hamkins, 2012]. Notice that these papers were published in the same year as Maddy published her *Defending the Axioms*; [Maddy, 2011]. Perhaps we cannot fault Maddy for not taking Hamkins' views into account in her books. However, Hamkins' position that set theory is not (or: no longer) in the business of searching for a single unified theory of sets has been argued by other set theorists before him, such as Paul Cohen and Thoralf Skolem. Maddy might hence not have known about Hamkins' argument, the general

[5]Since MAXIMISE is a formal criterion in *Naturalism in Mathematics*, one needs to do further work to show this conclusion formally. Maddy shows that this can be done; [Maddy, 1997], III.6.

thrust of his position however is well-known amongst those interested in the philosophy of set theory. I use Hamkins' rather than Cohen's or Skolem's arguments in this thesis because Hamkins' arguments are contemporary and well-documented, which make them particularly philosophically interesting; see also my argument for philosophical analysis of contemporary mathematical practice in chapter 6. Maddy might respond here by pointing out that she is making normative claims. I discuss this briefly at the end of this chapter and pick the thought back up again in my conclusion in chapter 6.

To argue against a general claim about the practice of mathematics it is not enough to present a practitioner who practices mathematics differently. The point is that mathematical practices have a social dimension (cf. chapter 1) and the views and actions of a single quirky practitioner do not necessarily affect the features of the practice as a whole. I hence have to ensure that the presented practitioner, Hamkins in my case, leaves a footprint in the practice of set theory; that is, I have to argue that Hamkins' case is representative enough to show us something about the practice.

Perhaps the most telling point about the resonance Hamkins' views have in the set-theoretic community is that he is discussed by his rivals. Magidor engages with Hamkins' views in [Magidor, 2012], Antos, Friedman, Honzik and Ternullo do the same in [Antos et al., 2015]. Steel mentions Hamkins in [Steel, to appear]. This list could be extended. However, Hamkins has not only made rivals. His work on the modal logic of forcing and the set-theoretic geology project– two projects which will be discussed in more detail in the next chapter– have attracted collaborators such as Löwe (e.g. [Hamkins & Löwe, 2008]), Leibman ([Hamkins et al., 2015]), Gitman ([Gitman & Hamkins, 2010]) and many more.[6] That Hamkins' views have the relevant kind of impact on the set theory community is also seen in that fact that Hamkins presented at the perhaps most important series of talks on the incompleteness issue in recent history: the *Exploring the Frontiers of Incompleteness* project, led by Peter Koellner.[7] There can be no doubt that Hamkins' views are influential. That is not to say that all set theorists agree with him. But it does show that he is not some quirk with a weird opinion. He is a relevant figure in the practice of contemporary set theory, and this makes his position suitable for my purposes.

[6]See the entries on Hamkins' web page at http://jdh.hamkins.org/the-set-theoretic-multiverse/.

[7]http://logic.harvard.edu/efi.php

4.2.1 The dream solution to the CH

Recall from chapter 2 that the Continuum Problem deals with the question about the size of the continuum. Cantor's Continuum Hypothesis, CH, is that the continuum is as small as possible. This hypothesis is independent from the currently accepted axioms of set theory, ZFC. Maddy claims that the set theorists are looking for a maximally strong (in her sense) and unique theory of sets. If found, such a theory would decide the truth-value of the CH. Hamkins calls set theorists who hold such a view 'universists' and their position the *universe view*; [Hamkins, 2011].[8]

According to Hamkins, most universists try to solve the continuum problem by what Hamkins calls the 'dream solution template to the CH':

> **Step 1** Produce a set-theoretic assertion ϕ expressing a natural and 'obviously true' set-theoretic principle.
>
> **Step 2** Prove that ϕ determines CH. That is, prove that $\phi \to CH$, or prove that $\phi \to \neg CH$. ([Hamkins, 2011], 16)

What does 'obviously true' mean? Hamkins elaborates as follows:

> the assertion ϕ should be obviously true in the same sense that many set-theorists find the axiom of choice and other set-theoretic axioms, such as the axiom of replacement, to be obviously true, namely, the statement should express a set-theoretic principle that we agree should be true in the intended interpretation, the pre-reflective set theory of our imagination. (ibid.)

Hamkins presents us with a possible contender for such a ϕ: Freiling's Axiom of Symmetry. Imagine you throw a dart onto the real line, hitting the point p. There it 'scatters' and (at most) countably many points on the real line are associated with the entry-point of the dart. Let us call the set of all these (at most) countably many points $f(p)$. Now imagine you throw another dart at the same line, hitting the point q. Then, so Freiling argues, one would expect q to be outside of $f(p)$. The argument is that there are 'so many more' points on the real line than points in the countable set $f(p)$, that it seems very unlikely to hit any point belonging to $f(p)$. Now let the second

[8] In Hamkins' presentation, the universe view is a realist view. As we have seen in chapter 3, Maddy's Second Philosopher does not need to subscribe to realism. This difference plays no role for the argument at hand.

dart scatter to an (at most) countable set of points on the real line as well, call it $f(q)$. Since the order in which we have thrown the darts should not matter, the same reasoning as above applies to argue that it is very unlikely that p is in $f(q)$.

Now take a step back and look at what I have done. I have been talking about a scattering-function f, which scatters the point $p \in \mathbb{R}$ onto the at most countable set of reals $f(p)$, i.e. f is a function mapping reals to at most countable sets of reals. Furthermore, Freiling argues that it is highly likely that the couple of reals (p, q) satisfies the statement '$p \notin f(q)$ and $q \notin f(p)$'. So it seems reasonable to assume that even if a specific pair p and q does not satisfy the statement, there will be some x and y such that $x \notin f(y)$ and $y \notin f(x)$. And since we made no assumptions on the precise nature of f, we can conclude:

> For all functions f mapping real numbers to at most countable subsets of real numbers there is a couple (x, y) of real numbers such that $x \notin f(y)$ and $y \notin f(x)$.

This statement is the *Axiom of Symmetry*, AS. By the above dart-throwing argument, AS is 'obviously true' in Hamkins' sense. Hence, step 1 of Hamkins' dream solution template is satisfied.

Interestingly, the AS also satisfies step 2 of the dream solution template: AS is equivalent to $\neg CH$. Quite in accordance with the dream solution template, Freiling holds that the dart-throwing argument, together with the proof of the equivalence $AS \leftrightarrow \neg CH$, is 'a simple philosophical 'proof' of the negation of Cantor's continuum hypothesis'; [Freiling, 1986], 190. However, as we know, the set theory community did not accept Freiling's proof, even though it fits the dream solution template. The question is why.

Hamkins tells us that many mathematicians pointed out that Freiling makes various non-trivial assumptions in his dart-throwing argument. For example, the argument assumes that the set $\{(x, y) \mid y \in f(x)\}$ is measurable. This is no trivial matter because measure-theory has shown us that measurability of a set is not always a given. However, Hamkins reminds us that

> Freiling clearly anticipated this objection, making the counterargument in his paper that he was justifying his axioms prior to any mathematical development of measure, on the same philosophical or pre-reflective ideas that are used to justify our mathematical

> requirements for measure in the first place. ([Hamkins, 2011], p. 17)

Hamkins does not venture to defend Freiling's argument. Rather, Hamkins draws attention to the point that

> mathematicians objected to Freiling's argument largely from a perspective of deep experience and familiarity with non-measurable sets and functions, including extreme violations of the Fubini property, and for mathematicians with this experience and familiarity, the pre-reflective arguments simply fell flat. ([Hamkins, 2011], 17)

It is this kind of experience that is important for Hamkins' thought. When the mathematicians of the above quote (sadly, not given by name) point to their experience with 'badly behaved functions and sets of reals [...] in terms of their measure-theoretic properties' (ibid.), then what they are pointing out is that they have experience with some of the mathematical facts about measure theory. These mathematical facts entail that certain functions and sets can be badly behaved. Freiling's philosophical and pre-reflective ideas imply that these same functions and sets are not badly behaved. Thus, Freiling makes an additional assumption here. Hamkins' point is that the mathematicians had too much experience with the contrary of Freiling's assumption to consider the assumption as 'obviously true' (in the sense of point 1 of the dream solution). And this robs AS of its intuitive appeal.

In the discussions Maddy mentions, the set theorists point to certain mathematical facts to support their arguments. Maddy has called these facts the 'facts of mathematical depth', cf. 3.2.2. Hamkins tells a similar story, in which the reasoning of some mathematicians about a proposed axiom is guided by some facts about certain pieces of mathematics. Hamkins has another example of the kind of experience he is after, a discussion of which will be helpful at this point.

Hamkins presents us with the results of a experiment of his own. On the well-known website www.mathoverflow.net, he has published some mathematical statements and asked the mathematicians visiting this site about the intuitive appeal of these statements.[9] In [Hamkins, 2011], Hamkins discuses an example.

[9] http://mathoverflow.net/questions/6594

> If a set X is smaller in cardinality than another set Y, then X has fewer subsets than Y.

According to Hamkins, amongst mathematicians who are not set theorists (henceforth: non-set-theorists) this statement is largely accepted. ‘An enormous number of mathematicians, including many very good ones, view the [above statement] as extremely natural or even obviously true in the same way that various formulations of the axiom of choice or the other basic principles of set theory are obviously true’; ibid., 18. Most of these mathematicians are surprised to hear that, in fact, this statement is independent of the current theory of sets.

Set theorists, so Hamkins tells us, react differently. To understand why many set theorists deny the intuitive appeal of the axiom, we first need to understand why the statement is independent of ZFC.

Recall that the powerset of X, $\mathscr{P}(X)$, is the set of all subsets of X. We can restate the above as follows.

> For all sets X, Y: if $|X| < |Y|$, then $|\mathscr{P}(X)| < |\mathscr{P}(Y)|$

Hamkins calls this the *Powerset Size Axiom*, PSA. To see the independence of PSA from ZFC, notice first that PSA is implied by the Generalised Continuum Hypothesis, GCH. Because GCH holds in Gödel’s constructible universe L, we know that $ZFC + GCH$ is consistent. It follows that $ZFC + PSA$ is consistent. Conversely, $ZFC + \neg PSA$ is also consistent. To see this, consider Cohen’s model of $\neg CH$. We know that $\omega < \omega_1$. But in Cohen’s model one can also prove that $|\mathscr{P}(\omega)| = |\mathscr{P}(\omega_1)|$. That is, $\neg PSA$ holds in Cohen’s model, which proves the consistency of $ZFC + \neg PSA$.

The set theorists have experience with both kinds of models involved in the above proof sketch in a way that non-set-theorists do not. In fact, there are many more models that would do the job in the above proof, and most set theorists have ‘met’ them in their undergraduate studies. According to Hamkins, the intuitive appeal of PSA is limited for the set theorists by their experiences with the facts about a theory of sets which implies that ‘PSA?’ is not so easily answerable.

We are again in a situation in which mathematicians deny the intuitive appeal of a certain statement or thought on the basis of their experiences with certain mathematical facts. Given sufficient experience with these facts, according to Hamkins, a mathematician would not accept a statement or thought based on pure intuition if it is at odds with these facts. That is, both Hamkins and Maddy stress the importance of certain mathematical facts in discussions about axioms. I will discuss mathematical facts in more detail below. I now return to the discussion of the dream solution template.

According to Hamkins, set theorists have a 'deep understanding how the CH can hold and fail [and] a rich experience in the resulting models'; [Hamkins, 2010], 24. We saw an example of this in the proof sketch of the PSA above. In some models the CH holds, in others it fails. In fact, for every model in which the CH holds, there is a forcing extension in which the CH fails and for every model in which the CH fails, there is a forcing extension in which the CH holds. In this sense, the CH is like a lightswitch that can be turned on and off at will via forcing. This is a mathematical fact about forcing in general and the CH in particular. Hamkins' point is that the set theorists have the necessary kind of experience and familiarity with this fact. If someone were to propose a restriction on the kinds of models that are allowed, then, so Hamkins claims, this would counteract the deep experience of the set theorists with the now 'disallowed' models. The restriction would not be seen as natural or obviously correct. According to Hamkins, the accustomedness of the set theorists with both CH and $\neg CH$ models influences what they regard as natural, obvious and intuitively clear here.

The conclusion Hamkins draws from the set theorists' experience with CH and $\neg CH$ models has severe consequences for the dream solution template of the universists. Recall that in step 1 the universists look for an obviously true ϕ and in step 2 they prove that this ϕ resolves the Continuum Problem. By the thought presented above, any ϕ that satisfies step 2 will seem unnatural to the set theorists. But then it does not satisfy step 1. 'Once we learn that a principle fulfils step 2, we can no longer accept it as fulfilling step 1, even if previously we might have thought it did'; [Hamkins, 2011], 16.

Does this mean the Continuum Problem will never find an answer in Hamkins' view? Not at all. In fact, on Hamkins' multiverse view – explained in more detail in the next section – the Continuum Problem is already solved.

> On the multiverse view [...] the continuum hypothesis is a settled question; it is incorrect to describe the CH as an open problem. The answer to CH consists of the expansive, detailed knowledge set theorists have gained about the extent to which it holds and fails in the multiverse, about how to achieve it or its negation in combination with other diverse set-theoretic properties. Of

> course, there are and will always remain questions about whether one can achieve CH or its negation with this or that hypothesis, but the point is that the most important and essential facts about CH are deeply understood, and these facts constitute the answer to the CH question. ([Hamkins, 2011], 15-16)

4.2.2 The multiverse view

Set theorists have deep experience not only with the various CH and $\neg CH$ models. Forcing has been used to force a variety of statements, i.e. to build forcing extensions of given models in which the desired statements hold. And forcing is not the only method to construct models. There are the inner models, ultrapower constructions, cut off universes and so on.

> A large part of set theory over the past half-century has been about constructing as many different models of set theory as possible, often to exhibit precise features or to have specific relationships with other models. Would you like to live in a universe where CH holds, but $\diamondsuit$ fails? Or where $2^{\aleph_n} = \aleph_{n+2}$ for every natural number n? Would you like to have rigid Suslin trees? Would you like every Aronszajn tree to be special? [...] Set theorists build models to order. ([Hamkins, 2011], 3)

Hamkins continues

> As a result, the fundamental objects of study in set theory have become the models of set theory, and set theorists move with agility from one model to another. (ibid.)

According to Hamkins, set theorists do not study sets, they study *models* of set theory.

According to the universists, there is what Hamkins calls an 'absolute background concept of set' (ibid.), a way sets really are. This is instantiated in a single model, often called the 'true universe of sets'. Hamkins points out that the universe view owes us an explanation why the other universes the set theorists study are 'imaginary' and 'illusion[s]'; ibid., 10. This seems difficult, given that 'we have a robust experience in those worlds, and they appear fully set-theoretic to us'; ibid., 3.

Hamkins incorporates the experience with the various models of set theory in his *multiverse view*. On this view, 'there are diverse distinct concepts of set, each instantiated in a corresponding set-theoretic universe, which exhibit diverse set-theoretic truths'; ibid., 2. For Hamkins, 'each such universe exists independently in the same Platonic sense that proponents of the universe view regard their universe to exist'; ibid., 2. The idea is 'to tease apart two often-blurred aspects of set-theoretic Platonism, namely, to separate the claim that the set-theoretic universe has a real mathematical existence from the claim that it is unique. The multiverse perspective is meant to affirm the realist position, while denying the uniqueness of our set-theoretic background concept'; [Hamkins, 2012], 2-3. Playing on the distinction between first-order logic, where the variables range over individuals, and second-order logic, where the variables range over collections of individuals, Hamkins has called his position a 'second-order' platonism: platonism not about sets but models of set theory (i.e. collections of sets).[10]

The full force of Hamkins' multiverse view becomes visible in his arguments in the $V = L$ debate. Recall from section 4.1 Maddy's argument against $V = L$: $ZFC + V = L$ is limiting because it can be recaptured in large cardinal extensions of ZFC but not vice versa. As I mentioned in that section, Maddy's argument relies on a certain understanding of what it means to 'recapture a theory'. I already pointed out that Maddy relies on natural interpretations to cash out the meaning of 'recapturing a theory'. It is at this point that Hamkins disagrees. In [Hamkins, 2014] he collectively calls arguments such as Maddy's '$V \neq L$ via maximize arguments' and aims to erode the force of this type of argument by pointing out some mathematical facts. For example, Hamkins reminds us that every transitive model of ZF can be end-extended to a model of $ZFC + V = L$. Thus, a model for a large cardinal extension of ZFC can be end-extended to a model of $ZFC + V = L$. In Hamkins' words

> Any given set-theoretic situation is seen as fundamentally compatible with $V = L$, if one is willing to make the move to a better, taller universe. Every set, every universe of sets, becomes both countable and constructible, if we wait long enough. Thus, the constructible universe L becomes a *rewarder of the patient*, revealing hidden constructibility structure for any given mathematical object or universe, if one should only extend the ordinals far

[10] Cf. Hamkins' *Exploring the Frontiers of Incompleteness* talk; video available at http://logic.harvard.edu/efi.php. See also [Hamkins, 2011], 2.

> enough beyond one's current set-theoretic universe. ([Hamkins, 2014], 20, italics in original)

In this sense then $ZFC + V = L$ can, contrary to Maddy, recapture large cardinal extensions of ZFC, but of course 'recapturing' means something different here because Hamkins' end-extended models do not qualify for Maddy as natural interpretations.

4.3 Mathematical Depth Again

From the last two sections we can draw the rather obvious conclusion that the practice of set theory is not homogeneous. In her recent book, *Pluralism in Mathematics*, Michele Friend has raised a related point; [Friend, 2014], 246. Friend claims that Maddy does not present the practice of set theory. Rather, Maddy presents the views of some set theorists. I specify: the views expressed are largely those of the Cabal. The Cabal is a loose and informal group of set theorists, formed in the 1970's. Due to its informal nature, it is difficult to pin down the members of this group, but according to Akihiro Kanamori they include Steel, Moschovakis, Martin, Solovay, Kunen, Kechris and Woodin; [Kanamori, 2009] 368. Notice that the views of all these set theorists, and especially those of Steel, feature in various places of Maddy's work. In fact, Maddy refers directly to the Cabal in her [Maddy, 1988] and has dedicated her [Maddy, 2011] to the group. We might say that Maddy is presenting not a philosophy of the practice of set theory but a philosophy of set theory as practised by the Cabal. This jars slightly with the historical cases Maddy discusses because her examples in these cases were not members of the Cabal. But because there is arguably a similarity in the spirit of the historical cases Maddy presents and the spirit of the Cabal (e.g. all are universists), the shorthand 'Maddy discusses the practice of the Cabal' is warranted and I will use it in what follows.

As Maddy tells the story, she presents a philosophy of set-theoretic practice. One might be tempted at this point to argue that this chapter shows that Maddy is doing exemplar philosophy (cf. 1.4) in a way which overlooks a relevant part of set-theoretic practice without telling her readers about it. Let me make two observations at this point.

First, there is a terminological issue with viewing Maddy as an exemplar philosopher. In the above, the charge is that Maddy points to exemplars to support her philosophy. That is, exemplars are taken to be set theorists. However, for Inglis and Aberdein exemplars are pieces of mathematics, not mathematicians; [Inglis & Aberdein, to appear]. This terminological issue has philosophical implications. Inglis and Aberdein have argued that philosophy based on pieces of mathematics as exemplars needs to assume consensus about the exemplar in question. When Steiner aims to explicate the concept of an explanatory proof on the basis of the examples he gives, he assumes consensus about these examples; cf. 1.4. When the examples in questions are mathematicians rather than pieces of mathematics, as in the case of Maddy (and myself; cf 5.1), then no such consensus need be assumed. Maddy does not make the claim that all set theorists think just like those set theorists she has studied. Rather, Maddy describes parts of successful set-theoretic practice in order to further our understanding of good conduct in set theory. To criticise her solely on the point that her analysis of set-theoretic practice does not capture the views of all set theorists misses the point of what she is trying to do. Thus, the criticism that Inglis and Aberdein bring forth against pieces-of-mathematics based exemplar philosophy does not apply equally to the kind of philosophy Maddy is proposing. It is for this reason that I will not call Maddy an exemplar philosopher in this thesis.

Revising the above accordingly leaves us with the claim that Maddy only presents the philosophy of a sub-practice, the practice of the Cabal set theorists, in a way which overlooks a relevant part of set-theoretic practice without telling her readers about it. This is severe criticism of Maddy's work. Maddy may have a response to these criticisms; this is the second observation mentioned above. In the last few pages of this chapter, I suggest what Maddy's response might look like. Some of the ideas raised here will be taken up again in chapter 6. However, keep in mind that it is not a goal of my thesis to refute Maddy. The aim of the following is to give Maddy a fair hearing and introduce some thoughts on mathematical facts, their desirability and Maddy's concept of mathematical depth, not to construct an argument to refute Maddy's philosophical position.

4.4 Desirability

We can learn from Maddy that the Cabal set theorists refer to certain pieces of mathematics in their arguments for the foundational debate. For example, they mention the desirable features of the Axiom of Projective Determinacy,

PD, of the Axiom of Choice and of the other examples explored in 3.2.1. As Maddy has already pointed out, that these axioms have these features is not 'up to us'. Given the historically grown set-theoretic background of accepted axioms, modes of reasoning, accepted logics and proof-styles, it is not up to us that PD has these implications. It might be up to us whether or not these features are desirable - more on this below - but it is not up to us that PD has these kinds of implications. Since these features are independent of our volition, I shall refer to them as mathematical facts.

There is stable agreement about the mathematical facts amongst practitioners of set theory, but there is disagreement about what follows from these facts. Hamkins sees them as supporting his multiverse view; Maddy's Cabal set theorists disagree. Confronted with a body of mathematical facts, the question is how to deal with them. Maddy has proposed as solution: desirability. This is the topic of the next section.

According to Maddy, some pieces of mathematics have the special virtue of 'track[ing] deep mathematical strains that the others miss'; [Maddy, 2011], 79. The implications of PD are desirable, so Maddy tells us, because the mathematical fact that PD has these implications has the virtue of tracking something deep.

With this account of desirability given, Maddy tells us that 'depth is used to adjudicate between axioms [in [Maddy, 2011]]' in her contribution to [Ernst et al., 2015], 6. In fact, not only is depth used to adjudicate between axioms but also between goals: 'our mathematical goals are only proper insofar as satisfying them furthers our grasp of the underlying strains of mathematical [depth]'; [Maddy, 2011], 82, n. 42. This is the path of mathematical depth on which, as Maddy has told us, mathematicians ought to wander; cf. 3.2.2.

Hamkins does not wander on the path of mathematical depth Maddy has laid out. He knows about PD and acknowledges its consequences as fruitful. He mentions the 'attractive determinacy and regularity features, as well as the forcing-absoluteness properties for $L(\mathbb{R})$' ([Hamkins, 2011], 1) and tells us that the multiversist 'may prefer some of the universes in the multiverse to others, and there is no obligation to consider them all as somehow equal'; ibid., 2. But for Hamkins this does not entail lifting the axiom to a similar status as the axioms of ZFC; for Hamkins, PD is not true in the same sense as the axioms of ZFC. The style of Hamkins' arguments however is very similar to the set-theoretic arguments Maddy has considered. Hamkins points out mathematical facts and argues that these support consequences beyond the formal machinery of set theory; he argues that the mathematical facts he points out support his multiverse view. This suggests that Hamkins might wander on a path of his own. I will not explore this path any further here. All I need in this thesis is the suggestion that, contrary to Maddy, there might be more than one path of mathematical depth.

Maddy would resist this suggestion. In [Maddy, 2011], she has claimed that it is an objective fact of the matter that the mathematical facts which surround PD (cf. 3.2.1) have the desirable feature of tracking something deep; for the Maddy of *Defending the Axioms* not only the mathematical facts are objective but also their desirability. Depth is objective, and this, so Maddy has claimed, provides an objective basis for the truth of the axiom.

In a recent special issue of *Philosophia Mathematica*, Maddy and the other contributors to the *Mathematical Depth* workshop[11] have taken up the issue of the objectivity of mathematical depth again; [Ernst et al., 2015]. Maddy admits that this objectivity 'is asserted [in [Maddy, 2011]] with perhaps more bluster than argument'; 247. She does not give an argument for the objectivity of mathematical depth in the special issue. Her writing clearly shows that she sees mathematical depth as an objective feature of some pieces of mathematics, but the points she raises do not amount to an argument. Maddy is aware of this and points out that what is at stake is whether Thin Realism is a tenable position. Her argument is that if mathematical depth should turn out to be subjective, then Thin Realism would lose its objective basis and become philosophically unattractive.

A point similar to Maddy's about Thin Realism can be made in connection with the suggestion of multiple paths of mathematical depth. If there were two such paths, say Maddy's and Hamkins', then the Thin Realist would hold, with the Cabal set theorists, that PD is true in the one true universe of sets whilst simultaneously believing PD to be only relatively true depending on which universe of the set-theoretic multiverse we are in. For the Thin Realist Maddy has presented, this would be incoherent.

The above suggests that Maddy is not merely describing the practice of the Cabal. She makes the normative claim that the proper set-theoretic methods are those of the Cabal and hence that Hamkins uses improper set-theoretic methods in his arguments. However, Maddy has also repeatedly claimed that philosophy can neither criticise nor defend mathematical practices. This raises the question whether Maddy's normative claim fits into her Second Philosophical programme. As mentioned in chapter 1, the aim of this

[11] The workshop was held in April 2014 at the University of Irvine, California.

thesis is not to refute Maddy. I will touch upon the question of normativity again in my conclusion in chapter 6, but even there I will not provide an answer. My goals are to contribute to the debate what kind of philosophical programme to study mathematics we should endorse and to philosophically analyse some features of set-theoretic practice. A discussion of normativity is beyond the scope of this thesis.

In this chapter, I have shown that there is a set theorist – Hamkins – who disagrees with the arguments of the set theorists Maddy has presented. There is disagreement amongst the practitioners of set theory on set-theoretic matters. The Hamkins story focussed in particular on a disagreement about a goal of set theory: is it a goal of set theory to provide a single unified theory of sets? As we have seen, Hamkins, much like the set theorists Maddy has studied, bases his arguments on mathematical facts. These mathematical facts form the discursive reality of the foundational debate some set theorists are currently having. However, there is disagreement about what the mathematical facts show. Maddy's normative claims would resolve this disagreement. I stay descriptive and explore the disagreement the set theorists are having further in the next chapter.

Chapter 5

Metaphysics in Practice

> The philosophical debate will likely not be settled by mathematical proof. But a philosophical view suggests mathematical questions and topics, and one measure of it is the value of the mathematics to which it has led.
>
> Hamkins, EFI Talk 2011

In this chapter, I present my main contribution to the project of philosophically studying mathematical practice: I argue that the mathematics as practised and the metaphysical views of the mathematicians can stand in a reciprocal relationship.[1] I show this for the particular case of set theory. After some preliminary remarks in 5.1, I show in 5.2 how some set theorists have set up mathematical questions in a way that the solutions to these questions can decide a metaphysical problem the set theorists are facing: roughly, whether they should be universists or multiversists. This shows that mathematical results can influence the metaphysical beliefs of mathematicians. In 5.3, I show that metaphysics can influence mathematical practice. I present two types of case studies. The first shows how the metaphysical beliefs of set theorists can influence their research agenda. The second type of case studies

[1] The slides for Hamkins' talk mentioned in the epigraph are available at http://logic.harvard.edu/EFI_Hamkins_MultiverseSlides.pdf, the citation is taken from page 66.

highlights how certain justifications in mathematical practice rely on metaphysical assumptions. In 5.4, I collect what we have learned from the various case studies in this chapter and form the argument that there are cases of a reciprocal relationship between metaphysics and mathematics: metaphysics can influence mathematics and mathematics can influence metaphysics.

In the next chapter, I will return to the question what features we should expect of our programmes to philosophise about mathematics and rely on the philosophical analysis presented in this chapter to form my argument.

5.1 Preliminaries

In this chapter I present the evidence for my philosophical claim that there are instances of a reciprocal relationship between mathematics and metaphysics. In the next chapter, I will use this result to challenge Maddy on her claim that the metaphysical views of the mathematicians 'should be treated as colourful asides or heuristic aides, but not as part of the evidential structure of the subject'; [Maddy, 2011], 52-53. But before I give the arguments for these claims, it will be helpful to discuss the nature of the evidence I rely on.

I draw my evidence mainly from the works of three scholars: Woodin, Koellner and Hamkins. Thus, the size of my samples is small. It directly follows that I cannot make any argument of the form 'all of set-theoretic practice is such-and-such' based on the evidence I present in this chapter; cf. also last chapter. My evidence will only support weaker claims of the form 'there are parts of set-theoretic practice in which...'. Hence my formulation '*there are instances of* a reciprocal relationship between mathematics and metaphysics'. As will become clear in the next chapter, this claim is enough to support the argument that philosophers should not ignore the metaphysical views of the mathematicians.

How relevant is the evidence I present in this chapter? I have already argued that Hamkins has the necessary kind of traction with the set-theoretic community (cf. 4.2), but what about Woodin and Koellner? Woodin hardly needs argument. He is widely considered to be one of the leading set theorists of our time with many prominent and important contributions to the field.[2] The argument that relying on case-studies obtained from Koellner's

[2] A certain type of large cardinal even bears his name: Woodin cardinal; cf. [Kanamori, 2009] for details.

publications can tell us about set-theoretic practice requires a bit more work. For one, one might not even consider Kollner as a set theorist.

Koellner received his PhD in philosophy and is currently employed as a professor in philosophy at Harvard University.[3] Judging by titles alone he would hence be more suitably called a philosopher than a mathematician. His work, however, includes aspects of the work of a mathematician. Koellner has read various set theory courses, has supervised at least two mathematical senior theses and advised on one other. He has published in respected journals, some of these publications in collaboration with other respected set theorists, most notably Woodin.[4] Koellner has organised the *Exploring the Frontiers of Incompleteness* project (EFI), already mentioned in 4.2, in which leading set theorists (Magidor, Martin, Steel, Welch and others) presented their work. Whether Koellner is regarded as a set theorist or not, he is certainly respected by the set-theoretic community and his contributions to the field are valued. Thus, Kollner, Woodin and Hamkins are relevant figures in the contemporary landscape of set theory and this lends force to a philosophical argument based on their contributions to set-theoretic debates. And it is in this sense of having the relevant kind of traction with the set-theoretic community in which I will consider Koellner to be a set theorist for what follows.

Koellner can be seen as a set theorist. However, he is philosophically trained and we can hence expect him to do some philosophically inspired mathematical work. The worry is that Koellner's work might not be representative of set-theoretic practice. After all, is it not reasonable to assume that normal set-theoretic publications deal with set theory and not with philosophical issues?

The answer to the worry about the representativeness of Koellner's work is that his are not solitary musings but rather contributions to a collective practice. In the last chapter we saw that Hamkins engages with some of the philosophical questions in set theory. In this chapter we will see that Woodin does the same. And these three set theorist, Koellner, Hamkins, Woodin, are not the only set theorists thinking about the philosophical issues of their field. There is the list of contributors to the EFI project, which includes names such as Magidor, Martin, Steel and Welch. Sy Friedman and his collaborators also

[3]Koellner's curriculum vitae is available online at http://logic.harvard.edu/koellner/CV_Koellner.pdf.

[4]Examples include [Koellner & Woodin, 2009] and [Koellner & Woodin, 2010].

discuss such philosophical matters; e.g. [Antos et al., 2015]. And Sharon Shelah has written papers on some of these philosophical issues (albeit under external pressure).[5] The set theorists mentioned here have a track record of set-theoretic excellence with important publications in the field and are amongst the leading set theorists of our time. They use set-theoretic means not merely to think about the philosophical issues in set theory but to engage in a debate about such matters. They refer to, discuss and critically assess each others work. We saw in the last chapter that Hamkins discusses the universists. In [Antos et al., 2015], Friedman discusses the views of Hamkins, Shelah and Woodin. Magidor mentions Hamkins' views in [Magidor, 2012]. This list could be continued. And what this shows is that this kind of set-theoretic thinking about some of the philosophical issues in set theory is more than a personal affair; it is a practice in which some set theorists engage. And because Koellner engages in this practice his work suitable for my purposes.

The above paragraph shows more than just that a study of Koellner's work is suitable for what I aim to do. It shows that the kind of activity I investigate in this chapter, the set-theoretic exploration of some of the philosophical questions in set theory, is an endeavour carried out by some of the world's leading set theorists. Not every set theorist participates, yet this practice is nonetheless worthy of philosophical investigation precisely because it is a collective effort of some set theorists to investigate these philosophical matters.

There is one last preliminary caveat to this chapter I need to mention. Hamkins, Koellner and Woodin are realists; all three believe in the existence of abstract mathematical objects. Focusing on these three realists allows me to make the two-way flow of influence between mathematics and philosophy in the works of some mathematicians visible. However, it also means that I do not study non-realist mathematicians and hence, given that there are philosophically relevant non-realist mathematicians,[6] I do not study the practice of set theory at large. It might turn out that non-realist set theorists do not let their set-theoretic practice be influenced by philosophy. This

[5] In Shelah's colourful words: 'under the hypothesis that I had some moral obligation to help Haim in the conference (and the proceedings) and you should not let a friend down, had I been given the choice to help with organizing the dormitories, writing a lengthy well written expository paper or risking making a fool of myself in such a lecture [about the future of set theory], I definitely prefer the last'; [Shelah, 1991], p. 1.

[6] [Friend, 2014] tells us that today many mathematicians consider themselves to be non-realists; p. 84. One example is Nelson; [Nelson, 1995].

would not threaten the argument of this chapter that there *can be* a reciprocal relationship between set theory and metaphysics. A proper analysis of non-realist set-theoretic practice is left for future work.

Because the three set theorists studied in this chapter are realists, they all agree that there are some mathematical objects. A metaphysical question arises: what are these existing mathematical objects like? It is on the answer to this question that Woodin, Koellner and Hamkins disagree. I will show in this chapter that they aim to base their answer to the metaphysical question on mathematical grounds; mathematics influences metaphysics. Furthermore, their answer to the metaphysical question influences their set-theoretic practice; metaphysics influences mathematics.

5.2 Mathematical Influence on Metaphysics

Woodin is, in Hamkins' terminology, a universist. Koellner has introduced a more fine-grained terminology to capture the universist/multiversist split. Furthermore, he has given the dispute a name: the pluralism/non-pluralism debate.

The *non-pluralist* holds that the statements of the language of set theory have determinate truth-values. He holds that theoretical reasons can be given that resolve the independence issues and 'maintains that the independence results merely indicate the paucity of our standard resources for justifying mathematical statements'; [Koellner, 2014], introduction.

Non-pluralism is very similar to what Hamkins calls the universe view. The main difference is that for the universists there is a true universe of sets and hence theoretical reasons that resolve the independence issues can be given, whereas the non-pluralist may either hold the existence of a universe prior to the theoretical reasons or vice versa.

The *pluralist* holds that some independence issues cannot be resolved. We have seen a version of this in Hamkins' multiverse view. A pluralist could, for example, hold that the ZFC axioms are true axioms of set-hood whilst all statements independent of these axioms lack a determinate truth-value.

Pluralism comes in degrees. Another pluralist might hold that, say, ZFC+'there is a measurable cardinal' is a system of true axioms about set-hood. These two pluralists would then disagree on the truth-value of statements such as $V = L$: the former pluralist holds that $V = L$ lacks a determinate truth-value because it is independent from ZFC, the latter holds that $V = L$ is false (because ZFC+'there is a measurable cardinal' implies $V \neq L$).

Koellner tries to capture the various degrees of pluralism in the *interpretability hierarchy*. This hierarchy contains formal (recursively enumerable) theories ordered by their strength to interpret other theories. This can be formalised; cf. [Koellner, 2011], section 2. At the bottom are comparatively weak theories, such as Peano Arithmetic, and the higher one climbs the stronger the considered theories become, via ZFC through $ZFC + V = L$ to ZFC+'large cardinal axioms' and beyond.

The higher we are on the interpretability hierarchy, the stronger the theories become and hence the more formerly undecidable statements become decidable. On the level of ZFC, the statement $V = L$ is undecidable ($V = L$ is independent from ZFC). On the level of ZFC+'there is a measurable cardinal', $V = L$ is decided (the theory proves $V \neq L$). In this way the interpretability ladder is connected to the foundational issues discussed in chapter 2.

One can be a non-pluralist up to any level of the hierarchy and a pluralist beyond. For example, one could be a non-pluralist about ZFC but a pluralist about any theory higher up on Koellner's interpretability hierarchy than ZFC. Alternatively, one might hold that theoretical reasons can be given to resolve the independence issues up to the level of projective determinacy (i.e. up to ZFC+'there are infinitely many Woodin cardinals') and furthermore be a pluralist about anything higher up in Koellner's hierarchy. The choices are, quite literally, infinite.

It is not an easy task to situate Hamkins in this hierarchy. Hamkins' writing does not specify whether he holds ZF, ZFC, $ZFC + Con(ZFC)$ or some other theory to be the shared theory of the universes in his multiverse. That is, it is not clear up to which level Hamkins extends his non-pluralism. The only thing that is clear is that it is situated comparatively low in the hierarchy and we can thus reasonably call him a pluralist. For the purposes of this thesis, this is enough.

5.2.1 The Truth of the Large Cardinal Axioms

Woodin is a non-pluralist. Indeed, Woodin sees set theory haunted by a 'spectre of undecidability'; [Woodin, 2010a], 17. This is a problem for him, which he has called the ZFC dilemma:

> **The ZFC Dilemma**: Many of the fundamental questions of Set Theory are formally unsolvable from ZFC axioms. ([Woodin, 2009a], 1)

Notice that Woodin does not intend 'dilemma' here as a choice between two or more undesirable alternatives but rather in its informal meaning as 'a difficult problem or situation', as becomes clear from his formulation of the dilemma.[7]

As a non-pluralist, Woodin needs to make the case that non-pluralism extends further and further up the interpretability hierarchy. Woodin has recently, in papers that started to appear around 2010, offered such an argument and I present it in 5.2.1. As I will point out, Woodin's argument is an argument in favour of the truth of the Continuum Hypothesis, CH. Interestingly, around the turn of the millennium Woodin gave a different argument to the effect that the CH is false. That is, Woodin has recently changed his mind about the truth of the CH. I explore this in the historically minded paper [Rittberg, 2015]. In this thesis, I concentrate exclusively on Woodin's 2010 argument; his 2000 argument will play no part in what follows.

It is not my aim in this thesis to criticise or defend any arguments given by set theorists. My intentions in giving their arguments are descriptive. Thus, I will not criticise the argument that follows below (with the exception of what I say in 5.3.2). I will also not discuss any of the criticisms Woodin's argument has received from elsewhere, especially the question for the validity of some of the points Woodin makes; e.g. [Steel, to appear], esp. footnote 24. This is because the argument I present below is the argument as it appears in contemporary set-theoretic practice as a contribution by one of the leading figures of the community. Woodin's argument as it stands is part of a highly relevant set-theoretic practice and hence suitable for my purposes. I here only mention the worry about the validity of Woodin's argument to then discard it and leave the debate about such matters to the experts.

Most of the mathematical facts mentioned in what follows can be found in [Woodin, 1999], [Woodin, 2010b] and [Woodin, 2011]. These results are amongst the most advanced results in contemporary research in set theory. I rely on Woodin's expository papers [Woodin, 2009a], [Woodin, 2009b] and [Woodin, 2010a] as well as his slides to a talk, [Woodin, 2010c], for the

[7]Notice that if ZFC were inconsistent, then every set-theoretical statement could be proven from ZFC and hence there would be no dilemma. Therefore, Woodin assumes the consistency of ZFC in the above mentioned dilemma.

reconstruction of Woodin's argument.

As mentioned, Woodin needs to make the case that non-pluralism extends further and further up the interpretability hierarchy. Woodin's argument for 'climbing higher' depends on his claim that the large cardinal axioms are 'true axioms about the universe of sets'; [Woodin, 2009a], 5. The argument starts with Woodin's prediction about the consistency of a certain theory, call it theory T. The precise nature of T is technical and not illuminating for the point I wish to make; see [Woodin, 2009a], 2-5, for details.[8] The consistency of T cannot be proven from ZFC (assuming that ZFC is consistent). Woodin reminds us that if T were inconsistent, then the inconsistency could be proven in finitely many steps. However, there is currently no indication that this is the case. With this in mind, consider the following prediction:

> There will be no discovery **ever** of an inconsistency in [T]. ([Woodin, 2009a], p. 6, emphasis in original)

Woodin calls this a 'specific and unambiguous prediction about the physical universe' which could be refuted by 'finite evidence'; [Woodin, 2009a], 5-6. Therefore, according to Woodin, '[o]ne can arguably claim that if this [...] prediction is true, then it is a physical law'; [Woodin, 2009a], 6. Woodin holds that if this prediction is true (and currently we have no reason to believe that it is not), then set theorists should be able to account for it. For Woodin, the way to do so is via large cardinals. It is a mathematical fact about theory T that it is consistent if and only if the large cardinal extension ZFC+'there are infinitely many Woodin cardinals' is. Using this information, Woodin writes about the above prediction:

> It is through the calibration by a large cardinal axiom **in conjunction with** our understanding of the hierarchy of such axioms as **true axioms about the universe of sets** that this prediction is justified. ([Woodin, 2009a], p. 5, emphasis in original)

Woodin writes '[a]s a consequence of my belief in this claim, I make [the above] prediction'; ibid., 5. Hence, Woodin accounts for the prediction of the consistency of T by

a) the calibration of the problem of consistency of T by large cardinal axioms.

[8] *For connoisseurs*: the theory T is $ZFC+SBH$, whereby SBH denotes the Stationary Basis Hypothesis.

b) the truth of the large cardinal axioms.

Part a) is a mathematical fact. What is at stake here for Woodin is part b). If b) is needed to make the prediction, then, according to Woodin, we have reason to believe in b). Assume that b) does not hold. Then all we have is an equiconsistency result of the theory T with ZFC+'there are infinitely many Woodin cardinals'. However, Woodin claims, '[j]ust knowing the [...] two theories are equiconsistent does not justify [the] prediction at all' (ibid.). Hence we need b) to make the prediction, i.e. Woodin argues that we have reason to believe that large cardinal axioms are true.[9]

5.2.2 Ultimate L

Recall from chapter 2 that the aim of the inner model programme is to construct mathematically accessible structures that can accommodate large cardinals; i.e. to construct models such that these models believe that certain large cardinal axioms hold. As we have seen, all the constructed inner models are limiting in the sense that only large cardinals up to the targeted large cardinal axiom can be accommodated, but no stronger ones.

Woodin has recently presented results in formal set theory which indicate that it might be possible to rid the inner model programme of the limiting results. The point is that the models produced by the inner model programme shed their limiting nature if the level of a supercompact cardinal could be reached. The axiom stating that there is a supercompact cardinal is fairly high up in the large cardinal hierarchy; they are the strongest large cardinals considered in this thesis up to this point and are situated in the upper third of Kanamori's chart of large cardinal axioms; [Kanamori, 2009], 472. This subsection explains Woodin's results on inner models with supercompact cardinals.

Woodin considers what would happen if an inner model which accommodates a supercompact cardinal could be found. Assuming that there is an inner model which can accommodate a supercompact cardinal, Woodin is able to show that, unlike other models built in the inner model programme, this model would accommodate essentially all large cardinal axioms (consistent with ZFC)[10]. Before I say more about the term 'essentially' in the

[9]For some critical remarks on this argument see 5.3.2.

[10]For the remainder of this section, I use 'all large cardinals' to mean 'all large cardinals consistent with ZFC'.

antecedent sentence, let me stress the point. Usually, if set theorists succeed in building an inner model for some large cardinal axiom LCA, then no large cardinal axiom stronger than LCA holds in this model. But, 'and this is the surprise' ([Woodin, 2009a], 20), if the set theorists would succeed in building an inner model for a supercompact cardinal, then this model, unlike the others, can accommodate essentially all large cardinals, even those that are stronger than supercompact cardinals.

What does it mean for a model to accommodate essentially all large cardinals? In [Woodin, 2009a] we find an example. Inner models are constructed by using so-called extenders. These extenders can then witness that certain statements hold in the inner model. Woodin asks us to assume that there is a class of extenders, all satisfying a technical condition,[11] which witnesses that there is a supercompact cardinal in an inner model. Then, so Woodin points out, these extenders also witness, for all natural numbers n, the existence of n-huge cardinals. The existence of an n-huge cardinal is amongst the strongest large cardinal axioms listed on Kanamori's chart of large cardinals; [Kanamori, 2009], 472. The point is, so Woodin remarks, that requiring the technical condition on the extenders witnessing the existence of a supercompact cardinal already ensures that the technical condition is met 'for a *much* larger class of extenders' ([Woodin, 2009a], 20, emphasis in original). At this point, Woodin has fleshed out what he means by 'essentially all' but whether this fleshing out is rich enough to make the notion sufficiently precise for mathematical use is a question I will leave to the experts on such matters.[12] In any case, it seems that Woodin finds the notion clear enough to work with it, and I will follow his lead in this.

An inner model that can accommodate a supercompact cardinal would be an ultimate step in the inner model programme of generalising L to account for more and more large cardinal axioms, precisely because it could account for essentially all large cardinals. This is why Woodin has called such an ultimate inner model *Ultimate*-L, written as *Ult*-L. Thus far, Woodin has been unable to construct an ultimate L; see concluding remarks of [Woodin,

[11] *For Connoisseurs:* For transitive class N such that there is a cardinal δ with $N \models$ 'δ is a supercompact cardinal' and extender E the technical condition is $E \cap N \in N$; [Woodin, 2009a], 20.

[12] In fleshing out the meaning of 'essentially all', Woodin refers, in [Woodin, 2009a] on page 20, to results presented in his over 600 pages long *Suitable Extender Models*, published as the two volumes [Woodin, 2010b] and [Woodin, 2011]. Sadly, he does not indicate which results he means in particular.

2009a] and [Woodin, 2009b]. Hence, we currently do not have a mathematically accessible structure that can accommodate all large cardinals.

There are various axiom candidates which could function as the axiom that the universe of sets is an ultimate-L, expressed as $V = Ult\text{-}L$; [Koellner, 2013a].[13] The various axiom candidates for $V = Ult\text{-}L$ form a family of axioms. Woodin has formulated mathematically precise questions, the answer to which could individuate the most suitable $V = Ult\text{-}L$ axiom. Thus, Woodin relies on future mathematical results here. Koellner connects these future mathematical results to the pluralism/non-pluralism debate. The rest of this sections spells out some of the details of this argument.

The $V = Ult\text{-}L$ axioms are powerful. They would banish the 'spectre of undecidability' because

> There is no known candidate for a sentence which is independent from [a version of $V = Ult\text{-}L$] and which is not a consequence of some large cardinal axiom. [Woodin, 2009a], p. 27

Hence, according to Woodin, all contemporary known formal questions are decidable in the large cardinal extension $ZFC + V = Ult\text{-}L + LCAs$, where $V = Ult\text{-}L$ stands for one of the versions of this axiom and $LCAs$ stands for a schema expressing that large cardinals exist. This result recalls the pluralism/non-pluralism debate. The non-pluralists could strengthen their case if they could find an axiom that would banish the 'spectre of undecidability'. The versions of $V = Ult\text{-}L$ (plus large cardinals) do precisely this job. Assuming $ZFC + V = Ult\text{-}L + LCAs$, there are no more contemporary natural and formally unsolvable questions; the ZFC dilemma is solved (assuming that we agree with Woodin on what the contemporary natural questions are).

But yet again, there is a problem: the different versions of $V = Ult\text{-}L$ contradict each other. For example, there is a version that implies the CH and one which implies $\neg CH$. Hence, those non-pluralists who subscribed to the argument thus far are forced to choose between the different versions of $V = Ult\text{-}L$. But how to do so? The 'spectre of undecidability' resurfaces here because it is, prima facie, unclear how the set-theoretic methods of discovery could deal with the issue. In the next subsection, I present Koellner's proposal to resolve this issue.

[13]For a presentation of one of the axioms for $V = Ult\text{-}L$ and possibilities for its generalisation, see [Woodin, 2010a], 17.

5.2.3 Convergence

Above, I introduced the $V = Ult\text{-}L$ axioms, mentioned that they banish the contemporary 'spectre of undecidability' and argued that the spectre resurfaces in the form of the different and mutually exclusive versions of $V = Ult\text{-}L$. In [Koellner, 2013a], Koellner discusses a method that could lead to a decision between the different $V = Ult\text{-}L$ axioms. In this subsection, I first explain this method and then present how Koellner connects this method to the pluralism/non-pluralism debate.

The proposed method to choose between the ultimate Ls is to analyse, for each version of $V = Ult\text{-}L$, the structure theories of two set-theoretic structures under the assumed axiom. One of these structures is well known to the set theory community; the other has not received much attention yet. The idea is that if the structure theories of these two structures converge in similarity under a version of $V = Ult\text{-}L$, then this counts as evidence for this version of $V = Ult\text{-}L$. To elaborate on this, I present the two structures that are to be analysed and give an idea of what is meant by the similarity of structure theories.

My elaboration starts with the structure which is well-known to the set theory community: $L(\mathbb{R})$.[14] This structure contains all the reals and all their definable subsets. It 'has figured prominently in the investigation of strong hypotheses' ([Kanamori, 2009], 142) and set theorists have come to an intimate understanding of this structure. For example, the structure featured in the arguments Maddy has told us about that support the Axiom of Projective Determinacy, PD. As we know from 3.2.1, the statement that the Axiom of Determinacy, AD, holds in $L(\mathbb{R})$, written as $AD^{L(\mathbb{R})}$, implies PD. As Maddy has pointed out, $AD^{L(\mathbb{R})}$ has been studied, and with it the structure $L(\mathbb{R})$. In fact, the axiom $AD^{L(\mathbb{R})}$ features in Koellner's argument for the similarity condition of the two structures; see below.

The second structure which is important for the method to choose between the different versions of $V = Ult\text{-}L$ is a technical generalisation of $L(\mathbb{R})$. It is helpful to notice here that $L(\mathbb{R})$ is $L(V_{\omega+1})$.[15] We can hence concentrate on $L(V_{\omega+1})$.

[14] $L(\mathbb{R})$ is constructed just like L, but rather than starting the construction with $\emptyset$ one starts with $\mathbb{R}$ instead.

[15] Both $V_{\omega+1}$ and $\mathbb{R}$ are isomorphic to $\mathscr{P}(\mathbb{N})$ and hence isomorphic to each other. This means that the class of definable sets that can be constructed out of $V_{\omega+1}$ is the same as the class of definable sets that can be constructed out of $\mathbb{R}$.

Given that we are thinking about $L(V_{\omega+1})$, we might wonder what happens when we replace ω by some cardinal λ. We obtain the model $L(V_{\lambda+1})$. Now let λ be such that there is an elementary embedding from $L(V_{\lambda+1})$ into itself with critical point below λ.[16] As Koellner remarks, this embedding condition is 'the strongest large cardinal axiom that appears in the literature'; [Koellner, 2013a]. The structure $L(V_{\lambda+1})$ is the second structure considered in the method to choose between the different versions of $V = Ult\text{-}L$. 'The difficulty in investigating the possibilities for the structure theory of $L(V_{\lambda+1})$ is that we have not had the proper lenses through which to view it. The trouble is that [...] the theory of this structure is radically underdetermined'; [Koellner, 2013a].

I now turn to the similarities of the structure theories of the two structures $L(V_{\omega+1})$ and $L(V_{\lambda+1})$. Firstly, observe that one can prove from the ZFC axioms that, under the assumption that $AD^{L(\mathbb{R})}$ holds, the cardinal $\omega_1(=\omega^+)$ is a measurable cardinal in $L(V_{\omega+1})$. Similarly, one can prove from the ZFC axioms that, under the embedding condition, the cardinal λ^+ is a measurable cardinal in $L(V_{\lambda+1})$. In this case, there is an obvious similarity between $L(V_{\omega+1})$ and $L(V_{\lambda+1})$, namely that both ω^+ and λ^+ are measurable in their respective structures. This presents $L(V_{\lambda+1})$ in a light which makes visible that this structure is a generalisation of (the well-known and understood) $L(V_{\omega+1})$.

The proposal is to study these two structures in light of the different versions of $V = Ult\text{-}L$. As mentioned, $V = Ult\text{-}L$ is a powerful axiom which could counteract the 'radical underdetermination' (Koellner) of $L(V_{\lambda+1})$. Thus, assuming one of the $V = Ult\text{-}L$ axioms, it might happen that further similarities between the structure theories of $L(V_{\omega+1})$ (under the assumption that AD holds) and $L(V_{\lambda+1})$ (under the embedding condition) become visible. If this happens for some version of $V = Ult\text{-}L$, then, according to the argument, this counts as evidence for the version of the axiom. However, if dissimilarities of the structure theories of these two structures are revealed under a version of $V = Ult\text{-}L$, then this counts as evidence against this version of the axiom. The thought here is one of convergence: if the structure theories converge under one of the $V = Ult\text{-}L$ axioms, then this is evidence for the axiom; divergence counts as evidence against the axiom.

[16]An elementary embedding between two models is a truth-preserving function between these models. For an elementary embedding j the critical point of j is the smallest ordinal α such that $j(\alpha) > \alpha$.

A negative convergence-result has already been obtained. Woodin showed that for one of the $V = Ult\text{-}L$ axioms the structure theories do not converge in similarity. According to the methodology on offer here, this axiom should not be considered to hold in the true universe of sets; [Koellner, 2013a].

The search for similarity leads to, as Koellner terms them, a 'list of definite questions', [Koellner, 2013a]. These questions are answerable; 'independence is not an issue' (ibid.). The 'spectre of undecidability', which resurfaced in the wake of the choice-problem between the different versions of $V = Ult\text{-}L$, seems finally to be tamed. It is not yet banished, as the analysis of the convergence of structure theories is not yet completed, but the set theorists are now in possession of a method that could potentially banish the spectre for good.

This leads us to Koellner's connection between mathematics and the pluralism/non-pluralism debate. Above, I wrote 'potentially banish' because there is the possibility that the structure theories might diverge for all versions of $V = Ult\text{-}L$. In this case the method to banish the 'spectre of undecidability' considered here would fail. It is this thought that Koellner uses to connect the mathematical results about the axioms $V = Ult\text{-}L$ to the philosophical pluralism/non-pluralism debate.

According to Koellner, if a version of $V = Ult\text{-}L$ is found under which the structure theories of $L(V_{\omega+1})$ and $L(V_{\lambda+1})$ converge in similarity,

> then one will have strong evidence for new axioms settling the undecided statements (and hence non-pluralism about the universe of sets); while if the answers [to the question of convergence] oscillate, one will have evidence that these statements are "absolutely undecidable" and this will strengthen the case for pluralism. In this way the questions of "absolute undecidability" and pluralism are given mathematical traction. ([Koellner, 2013a], closing words)

The idea then is this: if we can find a version of $V = Ult\text{-}L$ for which the structure theories converge, then the 'spectre of undecidability' would be banished, the ZFC dilemma resolved and the mathematical results obtained from the analysis of the relevant structure theories would pull us towards non-pluralism. If, on the other hand, the structure theories diverge, then the 'spectre of undecidability' is not banished by the methodology on offer here, which pulls us towards pluralism. Koellner has set up a mathematical test and given us an argument that the outcome of this test indicates an answer to the pluralism/non-pluralism question. I call this Koellner's *convergence argument*.

5.2.4 Mathematical Pull

The $V = Ult\text{-}L$ story shows that some set theorists are actively working on the connection between mathematics and metaphysics. This means first of all that some set theorists take the metaphysical debate seriously. They realise that their position in the pluralism/non-pluralism debate influences the list of leading problems in set theory; e.g. some non-pluralists are looking to settle CH, some pluralists are not. This makes the metaphysical debate important for these set theorists.

Some philosophers of mathematics currently have a debate about the pluralism/non-pluralism issue. For example, Mark Balaguer has proposed a 'full-blooded Platonism', which assumes that there is a platonic reality for all consistent mathematical theories and which is hence, in the terminology of this thesis, a pluralist position; [Balaguer, 1998].[17] One of his philosophical opponents is Colyvan, who defends a non-pluralistic version of Platonism; [Colyvan, 2001].

I have shown that there are set theorists who do not wait for the philosophers to resolve the pluralism/non-pluralism issue. Woodin, Koellner (and Hamkins) actively participate in this debate, and they use mathematical means to do so. Woodin tries to strengthen the case for non-pluralism by resolving a deep problem the non-pluralists have: the ZFC-dilemma. The 'spectre of undecidability' resurfaces in the various versions of the $V = Ult\text{-}L$ axioms, but it is tamed in the sense that formally solvable mathematical questions can be asked which could settle which of the axioms should be accepted. At this point, Koellner uses his convergence argument to connect the mathematics used to the metaphysical discussion: convergence of the relevant structure theories for a certain $V = Ult\text{-}L$ axiom would be an argument for non-pluralism, divergence an argument for pluralism. Koellner proposes here that mathematical facts, namely the future mathematical results obtained from studying the two relevant structures, should influence our position in the pluralism/non-pluralism debate. This means that we can now

[17]Balaguer defends full-blooded Platonism against some philosophical concerns but ultimately discards it because, according to him, it is disharmonious with mathematical practice.

generate arguments in a metaphysical debate by doing more mathematics. Mathematics is set up to pull the metaphysical debate.

Importantly, Koellner does not point to some past events in the history of mathematics for his arguments in the pluralism/non-pluralism debate. By relying on future mathematical results, he connects mathematics to metaphysics in such a way that mathematics actively influences metaphysics. Koellner establishes mathematical pull. Through the convergence argument, the mathematician has a way to search for an answer to a philosophical question without overstepping the boundaries of the mathematical discipline, and that this is possible is an important aspect of the connection between mathematics and metaphysics.[18]

The story about the Ultimate-L axioms shows that mathematicians can actively search for mathematical facts which exert mathematical pull. However, mathematical pull, the pull mathematical facts exert on the metaphysical beliefs of mathematicians, need not be actively sought. Hamkins for example finds himself confronted with the various kinds of mathematical facts discussed in the last chapter. For him, these facts exert pull towards pluralism; cf. chapter 4.

I have argued that mathematical facts can exert pull on the metaphysical beliefs of the set theorists. In the next section, I argue that metaphysical positions can influence how mathematics is practised.

5.3 Metaphysical Influence on Mathematics

In this section, I argue that the metaphysical positions of set theorists can influence how they practice set theory. I offer two arguments. The first is that the metaphysical position of set theorists can influence their research agenda; 5.3.1. I present Hamkins' modal logic of forcing and his set-theoretic geology project as an example of pluralism influencing the research agenda of a set theorist. I refer to work by Magidor to show how non-pluralism influences can affect research agendas.

My second argument is that some arguments in the foundational debate are metaphysically laden; 5.3.2. I discuss Woodin's argument for large cardinals and Hamkins' arguments about forcing.

[18] The argument that Woodin and Koellner are establishing mathematical pull on the metaphysical debate is also given in [Rittberg, 2016]. For a presentation of the historical development of Woodin's arguments in the foundational debate, see [Rittberg, 2015].

5.3.1 Research Agendas

The claim I wish to defend in this subsection is that the metaphysical position a set theorist holds can influence his research agendas. I first show that some pluralists are analysts of the relations between different universes. I then argue that some non-pluralists are universe builders.

Notice that I do not claim that pluralists are only concerned with the relations between set-theoretic universes. This would be false. As I show below, in Hamkins' study of the multiverse it is at times crucial to study certain specific universes in order to understand the truth-values of certain statements in these universes. I also do not claim that non-pluralists are not interested in the relations between set-theoretic models. Woodin has studied the so-called generic multiverse, which is connected to a more restricted form of pluralism than Hamkins' multiverse view, in detail (see below). My claim is hence not that the pluralism/non-pluralism stance of some set theorists excludes certain areas of set-theoretic practice from their interests. Rather, I argue that metaphysical stances can guide set-theoretic research by attaching importance to certain questions.

Studying the Multiverse

Section 10 of [Hamkins, 2011] is entitled 'Multiverse-inspired Mathematics'. It begins as follows

> The mathematician's measure of a philosophical position may be the value of the mathematics to which it leads. Thus, the philosophical debate here may be a proxy for: where should set theory go? Which mathematical questions should it consider? The multiverse view in set theory leads one to consider how the various models of set theory interact, or how a particular world sits in the multiverse. This would include such mainstream topics as absoluteness, independence, forcing axioms, indestructibility and even large cardinal axioms, which concern the relation of the universe V to certain inner models M, such as an ultrapower. A few recent research efforts, however, have exhibited the multiverse perspective more explicitly, and I would like briefly to describe two such projects in which I have been involved, namely, the modal logic of forcing and set-theoretic geology. ([Hamkins, 2011], 27)

Hamkins tells us here that his multiverist position influences which mathematical questions he thinks set theory should consider. He tells us that these questions include what he calls 'mainstream topics', but particular attention is given to the question 'how the various models of set theory interact, or how a particular world sits in the multiverse'. Here we have a pluralist telling us that his pluralism affects what he thinks the important set-theoretic questions are (without excluding more commonly studied questions). Hamkins is hence a good example of a set theorist who is an analyst of the relations between different universes.

Let me here briefly explain what the analysis of the multiverse looks like in Hamkins' case. I first explain the modal logic of forcing. I then consider the set-theoretic geology project.

The Modal Logic of Forcing

The Continuum Hypothesis (and other statements) are like light-switches with respect to forcing: by moving to a forcing-extension one can make the CH true or false at will; one can turn it on and off. Other statements, such as $V \neq L$, are not recoverable in this fashion. Once they become true in a forcing-extension they necessarily remain true in every further forcing-extension.

Hamkins' 'initial inquiry centered on the question: Could the universe be completed with respect to what is forceably necessary?'; [Hamkins, 2011], 27. Interpreting the modal operator $\Diamond$ as 'it is forcable that...' and $\Box$ as 'in all forcing extensions holds that...', Hamkins' question can be expressed as follows: can a model of set theory be build such that $\Diamond\Box\phi \rightarrow \Box\phi$ holds in this model? Hamkins explains the formula as follows: 'This principle asserts that the universe has been completed with respect to forcing in the sense that everything permanent achievable by forcing has already been achieved'; [Hamkins, 2011], 27. Hamkins' initial results in [Hamkins, 2008] established that the above formula is equiconsistent with ZFC.

Notice that Hamkins' question is a question about 'the universe' and is hence not closely linked to his pluralism. His pluralism affects his research in the interpretation of his result and the follow up questions he asks. Hamkins' result establishes that if there is a model of ZFC, then there is a model of

ZFC which is completed with respect to forcing in the above sense. Rather than focussing on an argument that this completed universe is or is not the true universe of sets, Hamkins asks 'What are the generally correct principles of forcing? Which modal principles are valid in all models of set theory? Which can be valid?'; [Hamkins, 2011], 27. Hamkins' focus is hence on an analysis of the (forcing) relations between the various universes in the multiverse.

Hamkins published his answer to the question about the correct principles of forcing in a joint paper with Benedikt Löwe; [Hamkins & Löwe, 2008]. They show that the modal logic of forcing is $S4.2$, a formal modal theory that is sound and complete with respect to the class of Kripke frames that form a finite pre-lattice, i.e. finite lattices with equivalence classes at their nodes. To prove this, one needs to first establish that the relevant axioms hold. This is relatively easy. For example, the axiom $\Box\phi \rightarrow \phi$ states that what holds in every forcing extension of the considered ground model already holds in the ground model. Noting that the trivial forcing extension is just the ground model, the axiom obviously holds. The difficult part is to show that no other modal axioms hold. Hamkins and Löwe have shown this for the class of all forcings.[19]

It is interesting to note that once one restricts the class of forcings, showing that no other axioms of modal logic hold becomes much harder. Take *c.c.c.*-forcing for example. This class of forcing is in many respects a class of simple forcings with certain nice properties. Cohen forcing is an example of a *c.c.c.*-forcing. However, the modal logic of *c.c.c.*-forcings is still unknown; cf. [Hamkins & Löwe, 2008], section 6. Other alterations, such as the restriction to a single model or considerations of forcing with parameters, open up further questions. The modal logic of forcing is hence a rich field of study which investigates the multiverse. Here is an extendible list of contributions on this topic: [Hamkins & Löwe, 2008], [Hamkins, 2008], [Rittberg, 2010], [Friedman et al., 2012], [Inamdar, 2013], [Hamkins et al., 2015].

Set-Theoretic Geology

[19] For a detailed development of the proof that the modal logic of forcing is $S4.2$, with a small correction of Hamkins' and Löwe's original proof, see [Rittberg, 2010].

Forcing constructs out of a given model of set theory V a 'fatter' outer model $V[G]$; cf. 2. Given a model of set theory W, we can ask whether there is a V and a G such that $W = V[G]$. In a sense, this reverses the direction of forcing. Rather than constructing outer models, we 'dig into' a given model W and search for a model V such that our given model is a forcing-extension of V. This is the core idea of set-theoretic geology.

In the above scenario, V is called a *ground* for W (assuming that G is $\mathbb{P}$-generic over V for some forcing $\mathbb{P} \in V$). A *bedrock* is a minimal ground. As Reitz showed, there are models that do not have a bedrock; [Reitz, 2006], [Reitz, 2007]. This leads to 'the principal new set-theoretic concept in [forcing] geology' ([Hamkins, 2011], 31): the *mantle* M is the intersection of all grounds.

One of the 'fundamental questions' (ibid. 32) is whether the mantle is a model of ZFC. Thus far, this has not been proven. The *directed grounds hypothesis* is that every intersection of two grounds is itself a ground.[20] This hypothesis implies that the mantle is a model of ZF, but not necessarily of ZFC. The *strong directed grounds hypothesis* states that the intersection over A many grounds, where A is a set, is itself a ground. This hypothesis implies that the mantle is a model of ZFC; cf. ibid., 32. For Hamkins, the questions for the truth of these hypotheses are questions about 'fundamental properties of the multiverse'.

'It is natural to consider how the mantle is affected by forcing, and since every ground of V is a ground of any forcing extension of V, it follows that the mantle of any forcing extension is contained in the mantle of the ground model'; ibid., 32. As a limit, we arrive at the *generic mantle* gM: the intersection of all grounds of set forcing extensions of V. This generic mantle turns out to be a more robust notion that the mantle. For example, the generic mantle is always a model of at least ZF.

According to Hamkins

> Set-theoretic geology is naturally carried out in a context that includes all the forcing extensions of the universe, all the grounds of these extensions, all forcing extensions of these resulting grounds and so on. ([Hamkins, 2011], 32)

[20]The *directed grounds hypothesis* was proved by Toshimichi Usuba in late 2015/early 2016. He presented his proof at the IMS-JSPS *Joint Workshop on Mathematical Logic and the Foundations of Mathematics*, Singapore, in January 2016. His slides contain a sketch of the proof; http://www2.ims.nus.edu.sg/Programs/016wjsps/files/usuba.pdf.

It is in this regard that Hamkins' multiverse perspective nourishes his investigation of set-theoretic geology. Hence, his metaphysical views influence his research agenda here.

Not only pluralists are interested in set-theoretic geology. Woodin, for example, has studied the topic as well. In fact, Woodin, Hamkins and Reitz have collaborated in their study of set-theoretic geology and published a paper together; [Hamkins et al., 2008].

Woodin introduces the concept of a generic multiverse of a model, the smallest family of models of set theory containing that model which is closed under forcing extensions and grounds; [Woodin, 2009b], section 1.4. Hamkins remarks: 'Woodin introduced [the generic multiverse] specifically in order to criticize a certain multiverse view of truth, namely, truth as true in every model of the generic multiverse'; [Hamkins, 2011]. Hamkins points out that this is not his position. Nonetheless, Woodin introduces us to the generic multiverse, which he considers to be a 'natural candidate for a multiverse'; [Woodin, 2009b], 18. Woodin then argues against this view. His argument includes two multiverse axioms and the Ω conjecture. A development of these thoughts would lead us too far astray. Suffice here to say that Woodin argues that 'the essence of the argument against the generic-multiverse position is that assuming the Ω Conjecture is true (and that there is a proper class of Woodin cardinals) then this position [the generic multiverse view] is simply a brand of formalism that denies the transfinite by reducing truth about the universe of sets to truth about a simple fragment such as the integers or [...] the sets of real numbers'; [Woodin, 2009b], 17.[21]

In [Woodin, 2009b], Woodin is interested in set-theoretic geology to formulate a reductio ad absurdum argument. Assume the generic multiverse view. Then, by various results, some of them obtained by a study of set-theoretic geology, the transfinite has to be denied. Woodin argues that the transfinite cannot be denied and thus a pluralistic view on set-theoretic truth connected with a 'natural candidate for a multiverse', the generic multiverse, cannot be upheld.

Building Universes

In this part of my thesis, I aim to show that the non-pluralism of some set theorists has influenced their research agendas.

[21]For a historically minded exposition of the Ω-conjecture, see [Rittberg, 2015]. See [Bagaria et al, 2006] for an introduction to more technical details on the Ω-conjecture.

Menachem Magidor asks 'Is [the] choice [between pluralism and non-pluralism] relevant at all to the working mathematician?'; [Magidor, 2012], 2. His claim is 'that the choice of the underlying Set Theory is relevant to the mathematical work' (ibid.) and thus begins his defence of non-pluralism. Magidor holds that 'the main motivation of studying Set Theory is still the foundational goal: creating a framework in which all of Mathematics (or at least a major part of it) can be included under a uniform system'; ibid. 3. For him 'the spectre of having multitude of set theories [...] is as troubling as imagining a city with different sets of traffic rules for every street'; ibid., 3.

Magidor clearly subscribes to Maddy's UNIFY; cf. 4.1. Indeed, Magidor acknowledges Maddy in his paper and points out his sympathy for her naturalism; [Magidor, 2012], 2. Similar to Maddy, Magidor claims that UNIFY is part of set-theoretic practice in a strong sense: it is the 'main motivation' to study set theory. Such a view on set theory allows only for non-pluralism.

Magidor continues

> The fact that the numbers, groups and real valued functions can be construed to be members of the same universe, obeying the same rules is the most important reason d'etre of Set Theory. ([Magidor, 2012], 3)

This shows that for Magidor, UNIFY is a mission. The 'main motivation' to study set theory is to 'create' a unified system in which all mathematical objects 'obey the same rules'. For Magidor, non-pluralism shapes the most important research agendas of set theory.

As Maddy has told us, her Cabal set theorists share the view that set theory has a foundational goal, that UNIFY is intimately connected to this foundational goal and this generates a mission for those set theorists that engage in the foundational debate: search for the one true universe of sets. Magidor is a strong defender of this view in that he believes this is the most important motivation to study set theory, it's 'reason d'etre'. Maddy makes no claim that her Cabal set theorists share Magidor's views on motivation and reason d'etre here. They do, however, share the conviction that UNIFY is a mission to find the true universe of sets, at least in Maddy's presentation of their views.

5.3.2 Metaphysical Justifications

In this subsection, I argue that some arguments presented in the foundational debate are not metaphysically innocent. I discuss Woodin's argument for large cardinals and Hamkins' arguments about forcing.

Woodin's Argument for Large Cardinals

Woodin's argument for the large cardinal axioms rests on the metaphysical belief in non-pluralism. Recall that Woodin gave a prediction that a certain theory T would never be shown to be inconsistent. A formal result shows that this theory is equiconsistent with a certain large cardinal extension of ZFC. According to Woodin, the prediction is justified because of a) the truth of the relevant large cardinal axioms and b) the equiconsistency result. Since equiconsistency alone is not enough to make the prediction, so Woodin claims, we have reason to believe in the truth of the relevant large cardinal axioms.

For the sake of argument, let me agree with Woodin that his prediction can and should be made, that it is correct and so on. Does this commit me to the belief in the truth of large cardinal axioms? Woodin's point is that the prediction is only justified because of the truth of the relevant large cardinal axioms and the equiconsistency results. Could I not weaken this and claim, in a multiversist spirit, that truth of the relevant large cardinal axioms in one of the universes of the multiverse is enough? The idea here is this: because there is a universe in which the relevant large cardinal axioms hold and because of the equiconsistency results, there is a universe in which Woodin's prediction about the consistency of the theory T holds true. Hence we can make this prediction.

Woodin does not claim that T is consistent in some unspecified universe. He makes the stronger claim that T is consistent in our universe. He calls it a 'specific and unambiguous prediction about the physical universe' which could be refuted by 'finite evidence'; [Woodin, 2009a], 5-6. If the prediction should turn out to be true, then, so Woodin claims, it is a 'physical law'; (ibid.).

Woodin makes a category mistake here. He confuses the physical tokens used to write down proofs of inconsistency with the mathematical facts. Assume T was shown to be inconsistent. Then there would be strings of symbols, written on a piece of paper (or else) which convey some information.

These symbols are the physical tokens. Woodin is not making a prediction about them. Rather, Woodin is making a prediction about a piece of mathematics. Knowledge about this piece of mathematics may be transferred through the physical symbols, but it is distinct from these symbols.

What matters for Woodin's predictions are set-theoretic universes. By linking a set-theoretic universe to our physical universe and using the fact that many of us believe that there is only one such physical universe, Woodin suggests his non-pluralistic view of the truth of the large cardinal axioms. However, once we disconnect the set-theoretic from the physical universe, we lose grip on the suggestion of a unique set-theoretic universe. At this point, my pluralistic suggestion above, that a single universe in which the large cardinals hold suffices to make Woodin's prediction, seems unopposed.

For the non-pluralist however, Woodin's argument for the truth of the large cardinal axioms may still go through. Of course there may be 'illusionary' (Hamkins) non-standard submodels of the true universe of sets. However, Woodin's prediction that T is consistent is justified because in the universe that matters, the true universe of sets, there are large cardinals that guarantee the consistency of T.

The above argument shows that Woodin has a non-pluralist stance prior to his argument that the large cardinal axioms are true. Notice how deeply this affects the mathematics that follows. Woodin's whole Ultimate-L argument rests on the idea that we should try to construct a model that can accommodate essentially all large cardinal axioms. Because an inner model that can accommodate a supercompact could serve as such a desired model, these inner models are studied by Woodin. The work on the proposed Ultimate-L axioms will serve, so Woodin and Koellner hold, as an argument for the foundational debate. We have seen all this above in 5.2. What I have shown here is that Woodin's non-pluralistic beliefs play a role in his argument.

Hamkins and Forcing

Hamkins accepts any forcing extension $M[G]$ over a model of set theory M as a universe in the set-theoretic multiverse. As Koellner points out, $M[G]$ is necessarily a non-standard model; [Koellner, 2013b], 18-22. I explain in more detail below.

The question is whether forcing extensions really exist. Here is a quick argument against their existence. The true universe of sets V is meant to include all sets. If there were a V-generic filter G, we could construct a new set-theoretic universe $V[G]$. This new universe would contain sets that are not part of V, such as G for example. But V was meant to be the collection of *all* sets. Hence, there can be no (non-trivial) V-generic filter G to construct $V[G]$. Hamkins calls this a catechism of the universe view: 'There are no V-generic filters'; [Hamkins, 2011], 5.

According to Hamkins, the claim that there is no V-generic filter is similar to the claim that there is no square-root of -1. 'Of course, $\sqrt{-1}$ does not exist in the real field $\mathbb{R}$. One must go to the field extension, the complex numbers, to find it. Similarly, one must go to the forcing extension $V[G]$ to find G'; [Hamkins, 2011], 5. As Hamkins notes, complex numbers can be modelled in $\mathbb{R}$. Similarly, the ground model V has some access to the forcing extension $V[G]$. However, the modelling of the forcing extension is not as powerful as the modelling of the complex numbers in the (two-dimensional) reals; there cannot be a fully isomorphic copy of the forcing extension in the ground model. Nonetheless, the access of the ground model to its forcing extensions comes 'maddeningly close' (ibid.) to this.

According to Hamkins, 'full acceptance of complex numbers was on its way' (ibid.) once it was realised that the reals could simulate the complex numbers. Hamkins argues that we should view the case of forcing extensions similarly. He presents three accounts of forcing to make this point. Koellner counters by pointing out that all three approaches lead to non-standard models of set theory. I discuss the three accounts in turn.

One way forcing can be understood is in the way Cohen used forcing. He started with a countable (and transitive) model M. In this case, a generic filter G can be build for any forcing $\mathbb{P}$; cf. [Kunen, 2006], 186. Once the existence of G is secured, the forcing extension $M[G]$ can be build. Koellner points out that in this case $M[G]$ is countable. 'As such M[G] is (by construction) a non-standard model of set theory'; [Koellner, 2013b], 18.

Another way to construct forcing extensions is via Boolean valued algebras. In this case, one considers a Boolean valued algebra $\mathbb{B}$ rather than the partial order $\mathbb{P}$. This allows to extend any model of set theory V to a forcing extension $V^{\mathbb{B}}$, not only countable ones as in the case above. In $V^{\mathbb{B}}$, one can show that ZFC (plus the statement one wished to force) holds with Boolean value 1. As such, the Boolean valued approach to forcing 'produces a class-size object $V^{\mathbb{B}}$ but one which is not two-valued. As such $V^{\mathbb{B}}$ is (by construction) a non-standard model of set theory'; [Koellner, 2013b], 18.

The two approaches to forcing presented above can be considered as standard approaches and are discussed in many introductory books on set theory;

e.g. [Jech, 2006], [Kunen, 2006]. Hamkins presents a third approach to forcing, 'the Naturalist account of forcing, which seeks to legitimize the actual practice of forcing, as it is used by set theorists'; [Hamkins, 2011], 8. This approach to forcing rests on the theorem that for every set-theoretic universe V and every forcing $\mathbb{P}$ there is a set-theoretic universe V' such that there is an elementary embedding from V to V' and that there is a V' generic filter $G \subseteq \mathbb{P}$. The idea is this. To force over V by using $\mathbb{P}$ one first moves to V', a model which is 'essentially the same' as V because of the elementary embedding. By the theorem, we know that there is a generic filter and we can hence construct the forcing extension $V'[G]$. This can be a class sized model of set theory which is two-valued. Hence, Koellner's criticisms above do not apply here. However, Koellner points out that 'there are three important things to note about $V'[G]$– it need not be transitive, it need not be well-founded, it is a definable class in V. For all three reasons it is as non-standard a model of set theory as those produced in the first two approaches to forcing'; [Koellner, 2013b], 19.

Hamkins remarks

> Of course, one might on the universe view simply use the naturalist account of forcing as the means of explaining the illusion: the forcing extensions don't really exist, but the naturalist account merely makes it seem as though they do. The multiverse view, however, takes this use of forcing at face value, as evidence that there actually are V-generic filters and the corresponding universes $V[G]$ to which they give rise, existing outside the universe. This is a claim that we cannot prove within set theory, of course, but the philosophical position makes sense of our experience– in a way that the universe view does not– simply by filling in the gaps, by positing as a philosophical claim the actual existence of the generic objects which forcing comes so close to grasping, without actually grasping. ([Hamkins, 2011], 10-11)

Hamkins states here that his philosophical position– his form of pluralism– influences the way he thinks about forcing extensions. Notice how Hamkins' understanding of forcing extensions as fully real influences his further argument in the foundational debate. Because we have so much experience with forcing extensions in which CH holds and in which it fails, and because all these forcing extensions are considered as fully real, what Hamkins called the 'dream solution' for the CH can no longer be obtained. In fact, because all the forcing extensions are fully real, we have, according to Hamkins, an answer to the Continuum Problem: 'The answer to CH consists of the expansive, detailed knowledge set theorists have gained about the extent to which it holds and fails in the multiverse, about how to achieve it or its negation in combination with other diverse set-theoretic properties'; [Hamkins, 2011], 15-16. In this case, Hamkins' philosophical position gives him an answer to the long standing set-theoretic Continuum Problem. Metaphysics influences mathematics here.

5.4 What did we learn?

It is now time to draw attention to some of the features of set-theoretic practice we have learned about in this chapter. I argue that there are instances of a reciprocal relationship between mathematics and metaphysics. To do so, I sum up the examples I have discussed in this thesis. I start with cases in which metaphysics influences mathematics, then move to cases in which mathematics influences metaphysics and then move back to cases of metaphysical influence on mathematics. I argue that this back and forth shows the reciprocal relationship between mathematics and metaphysics which reveals itself in some pieces of mathematics.

Some set theorists are having a debate about the metaphysical issues connected to their practice: the pluralism/non-pluralism debate. Some leading set theorists contribute to this debate; I have discussed some of the contributions of Hamkins, Woodin, Koellner and Magidor. This shows that the metaphysical issues discussed in this debate are taken seriously by a relevant group of set theorists. These set theorists realise that metaphysical views can influence set-theoretic practice and this makes the pluralism/non-pluralism debate mathematically relevant.

In this chapter, we have seen some examples of how the metaphysical beliefs of a set theorist can influence his practice. Perhaps the most obvious one is his research agenda. Some of the set theorists who participate in the foundational debate and who subscribe to non-pluralism are trying to find out about the truths that hold in the true universe of sets. Not so the pluralists. Hamkins-style pluralism for example aims at exploring the multiverse, not at finding out truths about a specific universe of this multiverse (even though investigating the truths of a specific universe may at times be part of the investigation of the multiverse). Thus, the metaphysical beliefs of a set theorist can form some of his set-theoretic aims and goals.

The above shows that the pluralism/non-pluralism debate is also a debate about the goals of set theory. One goal has received special attention in this thesis: the goal of finding out about the true universe of sets. The pluralism/non-pluralism debate is also a debate about whether or not this goal is a suitable goal for set theory.

The contributions of the set theorists to the pluralism/non-pluralism debate show that some leading set theorists are not waiting for the philosophers to settle the metaphysical issues involved. Instead, they engage with the relevant problems directly, using the means set theory provides. For example, the members of the Cabal highlight the desirable features of the Axiom of Projective Determinacy explored in 3.2.1. Hamkins aims to criticise the non-pluralistic position by pointing out that 'dream-solutions' to the Continuum Problem, such as Freiling's Axiom of Symmetry, push the mathematical facts about the various models of set theory we have experiences with into the realm of illusions; 4.2.1. The argumentative structure of these arguments fundamentally relies on some mathematical facts; without these mathematical facts there would be no argument.

One contribution to the pluralism/non-pluralism debate, which is of particular interest for the argument of this thesis, is Koellner's convergence argument. Woodin has individuated the *Ult-L* axioms. Given that there are many versions of these axioms, the question is how to decide between them. As detailed in 5.2.3, a certain set-theoretic structure serves as a testing ground. If, under a version of the *Ult-L* axioms, the structure theory of this set-theoretic structure converges in similarity towards the structure theory of an already well-understood structure, then this would be evidence for this version of the *Ult-L* axioms. This argument is of particular interest because it does not simply argue for pluralism or non-pluralism. Instead, it sets up a test for these two positions. In case convergence is observed for a single one of the axioms, this would count as strong evidence for non-pluralism. However, if the structure theories do not converge in this way, then pluralism is further supported, even though the question up to which specific level on Koellner's interpretability hierarchy (cf. 5.2) we should climb would still lack an answer. This shows that mathematics can be set up in such a way that future results have a bearing on the metaphysical issues of the pluralism/non-pluralism debate.

Some set theorists are hence not only interested in resolving the metaphysical issues; the examples above show that set theory provides them with the means to do so without overstepping the boundaries of their discipline. Mathematical facts, such as the ones mentioned above, are used to form arguments that aim to impact the metaphysical debate.

As explored above, the metaphysical pluralism/non-pluralism debate is also a debate about the goals of set theory. We can now see that set theory provides the means to discuss set-theoretic goals within its disciplinary boundaries. Thus, in the pluralism/non-pluralism debate, set theorists can use set-theoretic means to discuss the metaphysical issues and the connected goals of their discipline.

Their metaphysical positions and the connected goals influence the arguments of some set theorists in the foundational debate; some non-pluralists are seeking the one true V, some pluralists are not (and, presumably, there are pluralists and non-pluralists not discussed in this thesis who do not engage in the mathematical exploration of such philosophical issues). In 5.3.2, I showed that the metaphysical views of the set theorists can also influence the conclusions they draw from the mathematical facts they discuss. Woodin argues that his prediction that a certain consistency statement holds is an argument for the existence of certain large cardinals. As I pointed out, Woodin assumes that there is only one set-theoretic universe, that a pluralist could argue that it suffices that there is a universe which accommodates the relevant large cardinals without claiming that this is the single true universe of sets. Similarly, Hamkins adopts a pluralistic stance when he argues that the forcing-extensions are real. As Koellner has pointed out, a non-pluralist would simply regard them as sub-universes of the true universe of sets. Hamkins seems to agree to this point (cf. 5.3.2). These examples show that the metaphysical positions of the set theorists can influence what they take the mathematical facts to point to. Pluralists and non-pluralists find themselves in the same discursive reality provided by the mathematical facts, but how they accommodate these facts in their view on set theory depends also on their metaphysical views.

In this section, I argued that metaphysics can influence the way set theory is practised. The discussion then moved to the influence set-theoretic means can have on the metaphysical debate through the arguments given in the pluralism/non-pluralism debate. I then returned to the influence metaphysics can have on set theory by pointing out that the metaphysical positions of the set theorists can influence how they react to certain mathematical facts. This back and forth of the direction of influence, from metaphysics influencing mathematics to mathematics influencing metaphysics and then back to metaphysical influences on mathematics, shows that there are instances of a reciprocal relationship between metaphysics and mathematics. I highlighted this point for the special case of set theory. We can learn from this that there are instances where contemporary set-theoretic reasoning is intimately connected with metaphysics. To appreciate the arguments some set theorists give in the foundational debate, we need to take into consideration their metaphysical views. Furthermore, in order to appreciate why they hold their metaphysical views, we need to understand the set-theoretic arguments that have led them to their positions.

Chapter 6

Conclusion

With Maddy's Second Philosophical investigation of set-theoretic practice presented and my stories about this practice told, it now time to highlight some of the insights we might draw from this thesis. A summary of the thesis will be helpful at this point.

In chapter 1, I argued that most (if not all) philosophers of mathematics take mathematical practice seriously but they differ about what 'mathematical practice' means and which aspects of it are philosophically relevant. Philosophers of mathematics use different methods in their philosophising about mathematics and some find the methods of their philosophical rivals wanting. From these thoughts it emerges that the philosophical landscape of contemporary philosophy of mathematics is not split neatly into two camps, the mainstream and the practice-oriented camp, but rather sprawls along dimensions such as the understanding of the term 'mathematical practice', the allowed and disallowed methods to philosophically study mathematics and others. In contemporary philosophy of mathematics, the debate about the proper way to philosophise about mathematics is not a debate between two camps but rather between the proponents of the various philosophical programmes to study mathematics. A particularly well developed programme is Maddy's Second Philosophy. Concentrating on this programme in my thesis gave me a methodologically and philosophically rich story about set-theoretic practice. This story served as a starting-point for my own philosophical investigation of the practice and through the disharmonious parts of my story with Maddy's– pluralism and the instances of reciprocal relationship between mathematics and metaphysics– I will develop my contribution to the debate about how to philosophise about mathematics below.

It is part of the aim of this thesis to present results of contemporary research in set theory. As explained in chapter 2, set-theoretic practice reaches, and sometimes pushes beyond, the limits of the formal mechanisms of mathematics. The various set-theoretic models, obtained by forcing, inner model constructions or otherwise, amount to a formal proof that the truth of certain set-theoretic statements cannot be decided from the contemporary axioms of set theory, ZFC. As we have seen in this thesis, some set theorists are trying to determine the truth-value of these independent statements. One of the insights of chapter 4 was that even though the truth-values of the independent statements cannot be determined by purely formal means, some set theorists nonetheless use the formal mechanisms of their practice to determine these truth-values; set theorists use mathematical facts in their arguments in the foundational debate. The mathematical facts form a discursive reality for the set theorists and this requires us, as philosophers, to understand this reality. I aimed to provide such an understanding in chapter 2.

With the groundwork done, it was then time to discuss Maddy's Second Philosophical programme and her investigation of set-theoretic practice in chapter 3. Maddy spends considerable effort to argue that mathematical practice is philosophically relevant. These efforts situate her philosophical programme to study mathematics in the debate about how to philosophise about mathematics and thereby show that Maddy takes this debate seriously. Maddy argues that the attention to mathematical practice in the works of 'mainstream' philosophers of mathematics such as Quine is insufficient for a satisfactory philosophical account of mathematics. I then moved on to a presentation of Maddy's thoughts on how to philosophically investigate mathematical practice. Maddy introduces us to a methodological tool: the Second Philosophical inquirer. She, the inquirer, is idealised in the sense that she is at home in all scientific disciplines. Her aim is to find out what the world is like and she realises that mathematics is a powerful and helpful tool in this endeavour. Thus, the Second Philosopher wants to know what the proper methods of mathematics are. In her uses of the Second Philosophical methodology, Maddy quickly reduces the scope of her investigation to the proper methods of set theory. To find out about these, Maddy as the Second Philosophical inquirer rationally reconstructs resolved methodological debates in set theory and purifies them from all those elements of the arguments that did not play a role in the resolution of the debate; I stressed in particular that metaphysics is merely one such colourful aside for Maddy's Second Philosopher. From these purified rational reconstructions Maddy obtains some set-theoretic methods. She concludes that these methods are the proper methods of set theory. In an attempt to determine the ontological status of set theory– should we be Realists?– Maddy worries that her account of Thin Realism might be too thin, that we need an argument why the methods her Second Philosophical investigation has identified as the proper methods of set theory correctly describe the realm of the abstract objects called sets. Maddy presents mathematical depth as a way out. According to her, mathematical depth is not ontologically committing. Nonetheless, it is sufficiently objective to serve as a philosophical basis for her Thin Realist (and her Arealist). From this, Maddy frames a 'path of mathematical depth' and claims that mathematicians who do not stick to this path are going astray.

One set theorist who is straying from Maddy's path of mathematical depth is Joel David Hamkins. As I have shown in chapter 4, Maddy argues that set theory has a foundational goal. Hamkins agrees with this. However, Maddy argues further that the foundational goal implies UNIFY, i.e. the aim to search for a single unified theory of sets. Hamkins disagrees with this. Hamkins argues that any single theory of sets, and thus the concentration on a single set-theoretic model, would degrade our profound experiences with the various set-theoretic models to mere illusions. Just like the set theorists Maddy has studied, Hamkins bases his arguments on the kind of pieces of mathematics I have called mathematical facts. Hamkins points to the deep and mathematically fruitful implications of these mathematical facts. At this point, it looks as if, contrary to Maddy, there might be more than one path of mathematical depth.[1]

Maddy argues that metaphysics is not part of the justificatory structure of set theory. The Hamkins story has drawn attention to the fact that Maddy concentrates solely on non-pluralists. The fact that the set theorists are having the pluralism/non-pluralism debate shows that the issue is not as clear cut as Maddy presents it to be. Through her argument for UNIFY, Maddy has taken sides in a metaphysical debate, and this argument for non-pluralism affects her account of proper set-theoretic practice. In chapter 5, I showed that the positions some (realist) set theorists take on the metaphysical pluralism/non-pluralism issue stand in a reciprocal relationship with their set-theoretic practice. I presented in detail how Woodin and Koellner set up set theory in such a way that future mathematical results

[1]However, cf. 4.4.

can exert mathematical pull on the metaphysical pluralism/non-pluralism debate. I also argued that Hamkins is similarly led by mathematical facts to his pluralistic position. This shows that mathematics can influence the metaphysical beliefs of set theorists. I then showed that the metaphysical beliefs of set theorists can influence the way they practice set theory. In particular, I argued that pluralistic and non-pluralistic stances can influence the research agendas of set theorists and that the set theorist's position on the pluralism/non-pluralism issue can influence his or her interpretation of the mathematical facts.

With this summary of the first five chapters of my thesis given, I return to the issues raised in chapter 1: how should we philosophically study mathematics? A comprehensive answer to this question lies well beyond the scope of a PhD thesis. Based on the case-studies presented in this thesis, I can however give a partial answer to this question. In the following, I highlight three inter-connected lessons we can learn from this thesis about what our philosophical studies of mathematical practice should take into consideration. In the last part of this chapter, I then briefly touch again on Maddy's Second Philosophical investigation of set-theoretic practice and the question whether the insights of this thesis clash with Maddy's investigation or, more generally, with Second Philosophy as a programme. I end by speculating a little about philosophical methodology.

The pluralism/non-pluralism debate stood at the center of attention in chapters 4 and 5. As I have highlighted (esp. 5.3.1), this debate is also a debate about the goals of set theory. The stance a set theorist takes in this debate can have direct bearing on his arguments in the foundational debate. Pluralists like Hamkins might regard the foundational issues as (essentially) resolved; recall that according to Hamkins the Continuum Problem is a settled question. Non-pluralists such as Woodin and Maddy's Cabal set theorists disagree. Their one true universe of sets is not yet identified and it thus remains a goal of set theory to find this universe.

The pluralist and non-pluralist positions may not only influence the goals of set theory connected with the foundational issues. I showed that Hamkins is interested in exploring the multiverse. He has offered his project to explore the modal logic of forcing and his set-theoretic geology project to do so; cf. 5.3.1. The results produced are the kind of formal results upon which there is stable agreement within the set theory community. In the terminology of this thesis, Hamkins' projects make visible mathematical facts. Hamkins' interest in these facts is carried by his pluralistic conception of set theory; these facts tell him about the set-theoretic multiverse. As mentioned, Woodin is also interested in these mathematical facts. Unlike Hamkins however, Woodin aims to use these facts to construct an argument against pluralism. Woodin studies the multiverse as a means to obtain an argument in the foundational debate. Hamkins studies the multiverse also because for him it is a goal of set theory to find out about the multiverse. Hamkins' pluralism influences his set-theoretic goal here.

The pluralism/non-pluralism debate then is also a debate about the goals of set theory. This shows that the set theorists are having a debate about the goals of their discipline. In particular, there is disagreement amongst the set theorists about the goals of their practice. This means first of all that the question 'What are the goals of set theory?' is not a trivial one. Therefore, our philosophical programmes to study mathematics need to provide us with some guidance how to find out about the goals of the mathematical practice we are interested in. We need an anthropology of the goals of mathematical practices.

Secondly, the pluralism/non-pluralism debate shows that the goals of a mathematical discipline may change. As Maddy has pointed out, in its beginning the non-pluralistic goal to provide a single unified theory of sets was a goal of set theory. Hamkins makes the argument that we now have new evidence– our extensive experience with the various models of set theory– that supports a change in set-theoretic goals. This opens up the possibility that the goals of a mathematical discipline can change and our stories about how to find out about the goals of a mathematical practice need to be able to account for that.

Lastly, we have seen in this thesis that relevant set theorists disagree about some goals of their discipline; for Woodin it is a goal of set theory to provide a single and unified theory of sets, Hamkins disagrees. Our philosophical programmes to study mathematics hence need not only provide us with an anthropology of mathematical goals which is sensitive to change but one which is also sensitive to disagreement amongst the practitioners on the current goals of their discipline as well.

I now move the discussion to the second insight we can draw from the case-studies presented in this thesis: parts of set-theoretic practice are not metaphysically innocent. This point has become partly visible in the discussion about mathematical goals above and in chapter 5. The point is that some of the set-theoretic goals of the set theorists studied in this thesis are influenced by their metaphysical views. Thus, some goals of set-theoretic practice are not metaphysically innocent. Hence, there are instances in which set-theoretic practice is not metaphysically innocent.

In chapter 5, I argued in detail that there are cases of a reciprocal relationship between mathematics and metaphysics. As we have seen there, this relationship extends beyond the influence the metaphysical positions of some set theorists have on their goals. I have argued that the metaphysical views of these set theorists can influence how they account for certain mathematical facts. For example, I have discussed how the pluralist Hamkins accounts differently for the existence of the various forcing-extensions of set-theoretic models from the non-pluralist Koellner; 5.3.2.

Throughout this thesis we have seen cases in which set theorists embed their arguments in the pluralism/non-pluralism debate into the discursive reality the mathematical facts provide. Set-theoretic practice produces formal results which can reflect back onto the metaphysical beliefs of its practitioners. As the story about the Ultimate-L axioms shows, set theorists can even set up the formal machinery of set theory in such a way that future set-theoretic results may provide such metaphysical guidance. Thus, set-theoretic practice is also metaphysically laden because its results can influence metaphysical views.

From the last three paragraphs, we learn that a faithful analysis of mathematical practice needs to take the metaphysical beliefs of the set theorists into consideration, at least where the foundational debate in set theory is concerned. Thus, we should expect our programmes to philosophically analyse mathematics to be sensitive to the metaphysical beliefs the mathematicians hold.

Thus, my thesis has shown that the metaphysical views of the mathematicians can influence their mathematical goals and how they interpret and account for some mathematical facts. Furthermore, mathematical facts can influence the metaphysical views of the mathematicians. I have shown that in the particular case of set theory there are instances of metaphysically laden mathematical practice.

Mathematical practices are not defined in terms of how the mathematicians conceive of them or by the mathematical goals they pursue. I have shown in this thesis that the practice of set theory is not homogeneous enough to support such a definition; according to Magidor the foundational goal as understood by Maddy is set theory's reason d'etre, Hamkins disagrees. But how the mathematicians conceive of their practice and which goals they pursue is also not entirely disconnected from mathematical practice. In this thesis, I have argued that the metaphysical positions of the set theorists can influence their set-theoretic goals. Thus, the metaphysically laden instances of set-theoretic practice do not define what the practice of set theory is, but they do influence it. And this is why our philosophical programmes to study mathematics ought to be sensitive to these metaphysical influences on mathematical practices.

My third and last point is the importance of the philosophical study of contemporary mathematical practices. Mathematics is a human practice. It has a temporal dimension and is subject to change. Hamkins' argument for pluralism makes this visible: non-pluralism was once tenable and perhaps even desirable, but now that our experiences with the various set-theoretic models have accumulated to the point where, according to him, we can no longer accept the degradation of these experiences to mere illusions, we should accept pluralism. Woodin's and Koellner's set-up for the individuation of the correct $V = Ult\text{-}L$ axiom is similarly time-dependent: today we do not have access to the mathematical facts necessary to make an informed choice of axiom, but in the future, so they hope, we will.[2] Mathematical practices can change over time and thus contemporary mathematical practices may differ from the historical mathematical practices from which they developed. To understand these differences, we need to get a philosophical grip on contemporary mathematical practices. This does not discredit the philosophical study of historical mathematical practices – without them the changes in the practice could hardly be identified as change – but it does show that our philosophical programmes to study mathematics need a story about how to study contemporary mathematical practices.

The case-studies I have presented in this thesis show that contemporary set-theoretic practice is rich enough to sustain philosophical investigation. I concentrated on disagreement in this practice. This thesis showed that some set theorists are still disagreeing about the fundamentals of their field. It also revealed that some set theorists today have proposals for solutions which could, if accepted, resolve the debate. This shows that there is progress in the foundational debate in set theory, even though no final solution has yet been reached. This progress deserves our philosophical attention because it can tell us about set-theoretic practice. Furthermore, if we as philosophers wish

[2] As I explore in detail in [Rittberg, 2015], Woodin has recently changed his mind about the truth of the Continuum Hypothesis, which further supports my claim that mathematical practices have a temporal dimension.

to make a contribution to this debate, then we need to take the progress the set theorists have made into account, lest we provide out-dated arguments. This is a big 'if'. I touch again on this point below. That is, I will continue my argument for the philosophical investigation of contemporary mathematical practices shortly. But before I do, I now return to Maddy's Second Philosophical investigation.

Above, I argued that our philosophical programmes to study mathematics should have three features: they should provide an anthropology of mathematical goals; they should account for the fact that mathematical practices can be metaphysically laden; they should provide us with the means to study contemporary mathematical practices. Below, I present an argument which could imply that Second Philosophy as a programme has none of these features. There are two moves one could make to save the Second Philosophical programme from my criticisms. One is to meet the challenge head on and argue that Second Philosophy as a programme does indeed have the three features mentioned above. The other is to concede the point and retreat to the claim that my criticisms hit Maddy's use of the Second Philosophical programme, but not to programme itself. I discuss all this in turn below. My argument then moves to some considerations about the possibility for philosophers to provide impactful arguments for set-theoretic debates.

The Second Philosopher studies resolved methodological debates in order to individuate the proper methods of the practice. This does not seem to extend to the individuation of the goals of the practice. Recall here that the argument that set theory has a foundational goal which implies UNIFY is Maddy's, not that of any set theorist (even though some set theorists might agree with Maddy; cf. 4.1). To identify this goal, Maddy does not study resolved methodological debates. Rather, she tells us a story about general mathematical desirability: it is mathematically desirable to have a single unifying mathematical theory and since set theory could provide such a theory it is (or ought to be?; see below) a goal of set theory to provide such a theory. As mentioned above, this argument may be an idiosyncrasy of Maddy's philosophy rather than the methodological choice of the Second Philosopher. Or it may be a programmatic argument of the Second Philosopher along the lines of mathematical depth: mathematicians ought to stay on the path of mathematical depth, UNIFY pushes the set theorists along this path and hence set theory should aim to provide a single unified theory of sets. For now, I am working under the assumption that my thesis conflicts with Second Philosophy as a programme. Idiosyncrasy is discussed below.

Thus, the point here is that the Second Philosophical argument for UNIFY does not lead to a faithful description of contemporary set-theoretic practice.

According to Maddy, some methodological debates have been resolved but the philosophical debates have not, from which she follows that the methodological debates have not been resolved on philosophical grounds. The Second Philosopher thus excludes the metaphysical views of the set theorists from her philosophical analysis of the practice, dubbing them as colourful asides which are not part of the evidential structure of the subject. As I have shown in this thesis, the metaphysical beliefs of some set theorists can stand in a reciprocal relationship to the way these set theorists practice set theory. To ignore these metaphysical beliefs hence misses relevant parts of set-theoretic practice and leads to an unfaithful description of the practice.

The Second Philosopher studies resolved methodological debates. This programmatically excludes the philosophical study of ongoing unresolved contemporary debates in set theory. Nevertheless, Maddy still engages with these debates. For example, she presents her argument against $V = L$; cf. 4.1. However, she engages with these debates as a participant providing arguments, not as a philosophical inquirer aiming at philosophical analysis of an ongoing debate. The exception to this is her analysis of the debate surrounding the Axiom of Projective Determinacy. In this case she studies a contemporary debate in set theory. However, in light of pluralists such as Hamkins, she misinterprets the debate as settled for the set-theoretic community. The point is that a Second Philosopher who accepts that the debate about the Axiom of Projective Determinacy is not settled would, in accordance with the Second Philosophical programme, not aim at a philosophical analysis of the debate.

The Second Philosopher might have answers to all three of my criticisms above. As mentioned, she could meet my challenges head on or she could try to redirect the criticism onto the idiosyncrasies of Maddy's use of the Second Philosophical programme. I discuss all this in turn, starting with the redirection to the idiosyncrasies of Maddy's use of the Second Philosophical programme.

In Maddy's Second Philosophical investigation of set-theoretic practice, Maddy assumes the role of the Second Philosophical inquirer. My three points above rely on Maddy's investigation of set-theoretic practice. Thus, it might be argued, all my points can show is that Maddy has misrepresented set-theoretic practice in some way. Second Philosophy as a programme, so the thought, remains untouched.

I argue that it is incorrect to say that the Second Philosophical programme remains untouched when the burden is shifted onto the idiosyncrasies of Maddy's investigation. Recall how Maddy tells us what Second Philosophy is. According to her, 'it has no theory'; [Maddy, 2007], 1. The way we learn about the programme is through Maddy's use of it. Maddy's Second Philosophical investigations play an integral part in fleshing out what Second Philosophy is supposed to be. Hence, any criticisms directed at Maddy's investigations are also directed at Second Philosophy as a programme. As an example, take the point about the individuation of the goals of a practice. The way we learn about how a Second Philosopher would individuate the goals of a practice is through following Maddy's lead by example. Doubt about the example erodes the Second Philosophical programme because the example serves an important purpose in fleshing out what Second Philosophy is.

One might argue that stripping away the idiosyncrasies of Maddy's investigation still leaves us with the core ideas of the Second Philosophical programme, such as being practice-focussed and broadly speaking scientific for example. However, in chapter 1 I showed that there are various practice-focussed approaches to the philosophy of mathematics. Practice-focussedness is not enough for a philosophical programme to study mathematics. I have argued in that chapter that questions about the philosophical methods to study practice or the understanding of the term 'mathematical practice' are pressing issues. Maddy provides answers to these questions by doing Second Philosophy, not by giving us some methodological framework. And this means that stripping away the idiosyncrasies of her investigation strips away the answers to these pressing questions.

I have argued that redirecting my criticisms to the idiosyncrasies of Maddy's investigation does not save a philosopher in favour of Second Philosophy from engaging with the points I raised above. There are, however, answers such a philosopher might give to my three points. I discuss these in turn.

The Second Philosopher could point out that she is interested only in the proper set-theoretic methods. There may well be set theorists, such as Hamkins, who do not think that the foundational goal implies UNIFY. The point is however, so the Second Philosopher might argue, that the proper understanding of the foundational goal implies UNIFY. To provide a unified arena for all of mathematics is, so she has argued, mathematically desirable. Hence, so the Second Philosopher might point out, she did not aim at describing set-theoretic practice at this point but rather pointing out what set theorists ought to accept as their goal.

In regards to set-theoretic practice not being metaphysically innocent, the Second Philosopher will agree that the set theorists often refer to their metaphysical views (as Maddy has already done, cf. chapter 3). She might even agree that these views influence set-theoretic practice. However, she might claim, the actual justificatory structure of the subject is based on the kind of objective mathematical fruitfulness Maddy has called mathematical depth, not on the subjective metaphysical beliefs of the set theorists. The claim is that metaphysics is a heuristic aid at best, what really matters, what settles mathematical debates, is mathematical depth. Thus, the proper kind of arguments in those debates are metaphysically innocent arguments.

The Second Philosopher could offer a two piece argument why she does not study contemporary set-theoretic practice. Firstly, she is interested in the proper methods of set theory. Unresolved contemporary debates do not reveal properness in a way suitable to her philosophical programme and hence these debates need not be studied. Secondly, the Second Philosopher actively participates in these debates. She has identified which are, according to her, the proper methods of set theory and she then brings these methods to bear on the contemporary debates in order to make her contribution in terms of normative claims. Examples of this are her argument for UNIFY, where she participates in the pluralism/non-pluralism debate, and her argument against $V = L$ and for the axiom stating the existence of a measurable cardinal, which is a contribution to the debate about axiom choice.

In all three answers the Second Philosopher does not aim at a description of the practice. Rather, she makes normative claims about the proper conduct in set-theoretic practice. Prima facie, this stands in conflict with Maddy's claim that philosophy can neither criticise nor defend mathematical practice; recall here Maddy's principle discussed in 3.1. However, it is not purely philosophy that does the criticising here. It is philosophy informed by mathematical practice. At various points in Maddy's writing this kind of mathematically informed philosophy makes normative claims; examples are Maddy's contributions to the mathematical debates mentioned in the last paragraph and Maddy's claim that mathematicians ought to stick to the path of mathematical depth.

The thoughts I presented above suggest a possible route to criticism and show how one might defend Second Philosophy against such criticism. At this point, the question arises whether the kind of mathematically informed philosophy the Second Philosophical programme provides us with can effectively criticise mathematical practice and provide the kind of normative guidance Maddy's path of mathematical depth and her contributions to the set-theoretic debates aim to provide. In short, does Second Philosophy have some teeth?

The aim of this thesis is to present two stories about set-theoretic practice– the pluralism/non-pluralism debate and some instances of the reciprocal relationship between mathematics and metaphysics– and to draw some methodological conclusions from this about philosophical programmes to study mathematics. My aim is not a refutation of Maddy's Second Philosophy (neither idiosyncratic nor programmatic). The question whether any mathematically informed philosophy can have the teeth to effectively provide some normative guidance to mathematical practices is a deep and open philosophical question which lies well beyond the scope of this thesis. In these last few paragraphs, I speculate a little about these matters, provide a possible route to further the above indicated criticism of Second Philosophy and strengthen my argument for a philosophical engagement with contemporary mathematical practices.

In [Lakatos, 1976], Lakatos presents various ways in which mathematicians have reacted to the proofs and refutations of the Euler formula. One of these Lakatos calls the method of surrender: once a refutation to the proof is found, the proof no longer receives any attention and is discarded. Other methods, such as the method of proofs and refutations, do not discard the proof. These methods investigate the proof in order to improve the theorem/proof couple accordingly. As Lakatos convincingly argues, the method of surrender is less sophisticated than the method of proofs and refutations. Here we have a piece of mathematically informed philosophy which provides some normative guidance to mathematical practices. Lakatos' philosophy has some teeth.

The Second Philosopher's teeth grow out of her analysis of resolved methodological debates. One of her assumptions is that what resolved the debates back then were proper mathematical methods. Another assumption is that the methods that were proper back then are still proper today. This provides arguments which are aligned with the history of the subject and which can reasonably be called strong. It also introduces conservatism. It does however not provide an explanation why we should be conservative in this way. There is no philosophical engagement with the mathematical thought that calls for change, such as pluralism for example, and no weighing of the virtues of the call for change against the virtues of more conservative approaches. The set-theorists calling for change are not given a fair hearing by the Second Philosopher. In fact, they are programmatically not given any hearing at all. The Second Philosopher studies only resolved methodological debates which, by definition, excludes the ongoing debates about possible change in the discipline. The Second Philosophers teeth are blunted by the fact that she does not consider some parts of the practice which are relevant to the argument the Second Philosopher is trying to make.

To really make the above point stick I would have to ask more carefully whether Maddy intends Second Philosophy to have any teeth at all. She has repeatedly said that she does not. The philosophy she presents however suggests otherwise. This is a deep question about Maddy's philosophy which will not find an answer in this thesis.

A further question is whether philosophy can have any teeth at all. It is the mathematicians, not the philosophers, who decide mathematical debates. Philosophers may offer their expertise and insights, but the question remains how impactful on the practice such philosophical efforts are or can be. In the above, I assumed that philosophers can provide mathematicians with strong arguments for their debates, and I hope that I can argue for this point in future work. Here however it has to remain an assumption.

The Second Philosopher's teeth are blunted because she does not study all sides of the mathematical debates. This suggests a way to provide a philosophical programme to study mathematics with sharper teeth: study contemporary mathematical debates. Investigate the mathematical disagreement present in contemporary mathematical practices and identify the virtues and short-comings of the different positions. Then provide arguments to the still ongoing mathematical debates which are based on these virtues. These arguments, so I claim, can be the sharp teeth of our philosophical programme to study mathematics. In future work, I aim to develop this suggestion into a coherent argument and investigate in detail the values and virtues of the set-theoretic cultures.

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