Non-mathematical Content by Mathematical Means

There is no mathematical substitute for philosophy.
Kripke (1976, p. 416)

Abstract

In this paper, I consider the use of mathematical results in philosophical arguments arriving at conclusions with non-mathematical content, with the view that in general such usage requires additional justification. As a cautionary example, I examine Kreisel's arguments that the Continuum Hypothesis is determined by the axioms of Zermelo-Fraenkel set theory, and interpret Weston's 1976 reply as showing that Kreisel fails to provide sufficient justification for the use of his main technical result.

If we take the perspective that mathematical results are used in the context of a modelling of something not necessarily mathematical, then the situation is clarified somewhat, and the procedure for arriving at justification for the use of such results becomes clear. I give an example of a particularly strong form this justification might take, using the idea of formalism independence due to Gödel and Kennedy.

§1. Introduction. This paper takes place in the context of the philosophical analysis of the mathematical activity of a mathematician or group of mathematicians. Our primary concern is the situation in which this analysis makes use of mathematical techniques and results, but is also something more than a purely mathematical investigation. In other words, while the conclusions drawn are *about* mathematics, they themselves have some non-mathematical content. The main thesis of this paper is that in such a situation one should be careful about the use of mathematical results, and that in general their inclusion in philosophical arguments requires some buttressing.

Let us consider some examples. (i) Kreisel's (1965) arguments that the axioms of Zermelo-Fraenkel set theory determine the Continuum Hypothesis. This is our main example, and will be considered in detail below. For the time being, note that Kreisel uses a technical result of Zermelo concerning the models of second-order ZF (from now on ZF2) in his argument. What of non-mathematical content? First notice that I have been somewhat conservative in stating Kreisel's conclusion. I might have given it more simply as "the Continuum Hypothesis has a determinate answer". Kreisel does not conclude this explicitly, but others have. For instance, citing Kreisel (1965), Isaacson writes:

> In a sense made precise and established by the use of second-order logic, there is only one set theory of the continuum. It remains an open question whether in that set theory there is an infinite subset of the power set of the natural numbers that is not equinumerous with the whole power set. (Isaacson 2011, p. 38)

It might not be so apparent that the conservative conclusion stated above has any non-mathematical content, but I will argue below that it does. This is exactly the point in arguments which go further (such as Isaacson's) at which the non-mathematical content first emerges. (ii) Putnam's Skolemisations. Putnam (1983) makes various uses of the Downward

Löwenheim-Skolem Theorem; for instance, he considers the theorem applied to the hypothetical formalisation of all science, and concludes that "'theoretical constraints', whether they come from set theory itself or from 'total science', cannot fix the interpretation of the notion set in the 'intended' way." (iii) Hamkins' Ancient Paradise. Hamkins (2012) provides a mathematical argument against what he calls the "ancient paradise position".

> This position holds that there is a highly regular core underlying the universe of set theory, an inner model obscured over the eons by the accumulating layers of debris heaped up by innumerable forcing constructions since the beginning of time. If we could sweep the accumulated material away, on this view, then we should find an ancient paradise. (Hamkins 2012)

The so-called "mantle" of a model of ZFC is the intersection of all models of which it is a forcing extension, and Hamkins claims that this is a good candidate for this hypothetical "ancient paradise". The argument he gives that such a regular core does not exist makes use of a theorem stating that every model of ZFC is the mantle of another model of ZFC.

By way of further elucidation, let us consider our main thesis in the context of Kreisel's programme of informal rigour. This is a programme which concerns the process of obtaining "rules and definitions by analyzing intuitive notions and putting down their properties", and wants:

> (i) to make this analysis as precise as possible (with the means available), in particular to eliminate doubtful properties of the intuitive notions when drawing conclusions about them; and (ii) to extend this analysis, in particular not to leave undecided questions which can be decided by full use of evident properties of these intuitive notions. (Kreisel 1965, p. 138)

Consider any such rule or definition. If we are successful, then it should indeed be a part of mathematics; however many (if not all) arguments we might give for this will not be purely mathematical results, and as such will have some non-mathematical content. Of course, by desideratum (i), our justificatory arguments may well make use of mathematical techniques (and by *use of mathematical techniques*, in this context I mean as applied with these intuitive notions, rules, and definitions as the objects of investigation). Similar remarks apply to desideratum (ii), since we may well entertain non-mathematical questions which could be answered with the aid of mathematical techniques.

We can understand the main thesis of this paper as putting a requirement on the process of informal rigour. Kreisel's description brings to mind an image of the 'zealous application of all the means which we have at our disposal' in "analyzing intuitive notions and putting down their properties". What our thesis urges is that we should rein in our zeal a little. As far as mathematical results are concerned, we should exercise some caution and take care to buttress their use in arriving at conclusions which are not purely mathematical.

§2. Kreisel on the Continuum Hypothesis. In the same paper in which he introduces his programme of informal rigour, Kreisel gives arguments for the claim that the Continuum Hypothesis is determined by the axioms of ZF set theory. He makes use of a technical result, due to Zermelo, concerning the second-order axioms (which Kreisel takes to be the *full* axioms of set theory).

Theorem 1 (Zermelo 1930). *Any model of* ZF2 *is isomorphic to* $(\mathrm{V}_\alpha, \in)$ *for some limit ordinal* $\alpha > \omega$.[1]

This gives the following important corollary concerning the Continuum Hypothesis (CH).

Corollary 2. ZF2 $\vDash$ CH *or* ZF2 $\vDash \neg$CH

Proof. As an instance of Excluded Middle, we have CH $\vee \neg$CH. Which ever way this goes, it is decided by $\mathrm{V}_{\omega+3}$, hence all stages V_α for a limit $\alpha > \omega$ agree on CH. Therefore, by Theorem 1, every model of ZF2 agrees on CH, so ZF2 $\vDash$ CH or ZF2 $\vDash \neg$CH. □

In slogan form: models of ZF2 correspond to 'high' initial segments of the universe, so all models agree on lower-level statements such as CH.

We will consider the move from Corollary 2 to the following.

Conclusion 3. The axioms of ZF determine CH.

Of what does the move consist? Superficially at least, we have lost any specific reference to second-order aspects. If we agree with Kreisel that the second-order axioms are the *full* axioms of set theory, then this is not so much of a move. I will argue however that something more is going on here; in particular, that Conclusion 3 has some non-mathematical content, while Corollary 2 does not, and that Kreisel does not account for this.

The starting point is Weston's (1976) reply to Kreisel, in which he contends that "his [Kreisel's] second order argument actually presupposes a unique intended interpretation for ZF". In fact, Weston argues against a slightly modified version of Kreisel's argument. The following result (which Weston refers to as the 'almost categoricity' result for ZF2) is an immediate corollary of Theorem 1.

Theorem 4. *Given any two models of* ZF2*, one is isomorphic to an initial segment of the other.*

Now, one can easily run Kreisel's argument making use of this result. For; if one model is an initial segment of another, then both must agree on CH (so long as they both contain $\mathrm{V}_{\omega+3}$, which they must if they are to be models of ZF2). This modified argument is what Weston attempts to counter.

> [S]uppose that [...] there are two or more interpretations of the axioms of ZF which are equally natural candidates for "the" intended interpretation, but which are not almost isomorphic. Then the "all subsets" condition [of the second-order quantifier] is ambiguous, for what may be "all subsets" in one interpretation need not be "all subsets" in another. That is, suppose that two mathematicians have in mind different (not almost-isomorphic) natural interpretations of ZF2. Then each could use his notion of set to prove the almost categoricity of ZF2 according to his own interpretation of that theory, and yet, their interpretations are, by hypothesis, not almost isomorphic. [...] As it stands, the argument merely shows that the CH has the same truth-value in the group of $*$-structures [models] associated with each natural interpretation.[2] (Weston 1976, p. 288)

[1] In fact, Zermelo proved a stronger result, namely that the models of ZF2 are, up to isomorphism, precisely the stages V_κ for κ an inaccessible cardinal.

[2] Weston's "$*$-structures" are the same thing as our (full) models. He uses the term "structure" for so-called 'Henkin' models.

Weston does not dispute the *mathematical* result that Corollary 2 follows from the almost categoricity of ZF2. Rather, he points out that "ZF2 $\vDash$ CH or ZF2 $\vDash \neg$CH" is misleading, and that it does not follow that CH is *really* determined by the axioms of ZF.

Let us now modify Weston's argument so that it is more faithful to Kreisel's original reasoning, and in doing so consider things in more detail. Weston's talk of 'interpretations in the mind of mathematicians' helps to make his argument more perspicuous, but we can dispense with it here. The important point is that Zermelo's theorem is a result in (some) set theory. Call this the 'background set theory', to distinguish it from the set theory ZF2, which is the mathematical object/structure which Zermelo's theorem concerns. Note, this background may actually be ZF2 again; in any case it must be strong enough to prove Zermelo's theorem.[3] I will consider things from two perspectives. (1) From a more realist stance (which is the main perspective considered in this section) in any interpretation of this background set theory, the way in which CH is decided by the axioms of ZF is exactly the value which it takes in that interpretation. The natural question to ask is, why stop here? If Kreisel's argument is needed to convince us that CH is determined by the axioms of ZF, might we also need an argument to convince us that CH is determined in the background set theory? In other words, if we do not already know that there aren't two interpretations of the background set theory which answer the CH question in different ways, can we *really* say that ZF determines CH? (2) Taking a less realist perspective, the statement ZF2 $\vDash$ CH or ZF2 $\vDash \neg$CH corresponds exactly to the instance of the Law of Excluded Middle CH $\vee \neg$CH. So we should have as much confidence that ZF2 determines CH one way or the other as we have that CH is determined one way or the other *simpliciter*.

I do not claim that the above offers an insurmountable challenge to Kreisel. Rather, it demonstrates that Kreisel's (implicit) move from Corollary 2 to Conclusion 3 is too quick, and requires some further justification to go through.

What of non-mathematical content? Above I talked of the axioms of ZF *really* determining CH. It seems that the purely mathematical result "ZF2 $\vDash$ CH or ZF2 $\vDash \neg$CH" is not enough to give us this, since it is made relative to a context in which CH need not be determinate (already). So this '*really*' is where the non-mathematical content comes in: we want to say that the axioms determine CH *absolutely*: there is an answer to the Continuum Hypothesis such that it, and it alone, is compatible with the Zermelo-Fraenkel axioms. And this is what Kreisel would like too, if his suggestion — that in order to decide CH we should consider "new primitive notions, e.g. properties of natural numbers, which are not definable in the language of set theory" (i.e. notions lost when one replaces second-order quantification with a first-order schema) — is to carry any weight.

In the next section, I will — from a particular perspective — investigate the general problem of buttressing arguments making use of mathematical results. But for now I will examine the present situation and consider some means by which we might justify the emergence of this pesky non-mathematical content, as well as some challenges to be met. I give two general forms of argument.

[3] As to the background set theory with which Zermelo proves his result, the relevant passage from his paper is the following.

> We call "normal domain" a domain of "sets" and "urelements" that satisfies our "ZF'-system" with regard to the "basic relation" $a \in b$. We shall treat "domains" of this kind, their "elements", their "subdomains", their "sums" and "intersections" *according to the general set-theoretic concepts and axioms exactly like sets*, from which they do not substantially differ anyway. (Zermelo 1930; translation in: Zermelo 2010, p. 405; emphasis mine)

First, one might try to block the counterargument by claiming that the consideration of incompatible interpretations of the background set theory is not valid since it is something rather different from ZF2. Note that Kreisel should already be unhappy about this, since this background set theory is naturally his intuitive "precise notion of set", which he appears to identify, in some way, with ZF2 — or at least, it seems likely that any justification he might give for CH's determinacy on his "precise notion of set" will stem from its determinacy from the axioms of ZF.

Notwithstanding Kreisel's objections, there are several ways in which we might flesh-out this counter-argument-blocking claim. (1) "This background set theory is a stronger *formal* theory — strong enough in particular to decide CH 'on its own'." It is difficult to imagine any justification for the use of this theory over ZF2 which does not on its own immediately give a justification for believing an answer to CH. (2) "While having multiple incompatible interpretations is a possibility, there is only one *intended* interpretation of this background set theory (at least up to compatibility with respect to the answer to CH)." The qualifier "*intended*" brings with it its own baggage, but moreover, as with (1), one wonders why this 'intended interpretation' solution is not applied directly to 'normal' set theory, to get the result without invoking any second-order machinery (Weston makes a similar point in his reply to Kreisel). (3) "This business of 'going up another level' is simply not valid." In other words, while we may fruitfully examine interpretations of ZF2, this examination takes place in some kind of 'mathematical bedrock', and talk of multiple interpretations of this bedrock is meaningless. This is perhaps the most promising alternative. We might claim, for instance, that this bedrock cannot be considered in any way as a mathematical object/structure (or that it cannot be modelled in this way at least with respect to the determinacy of the Continuum Hypothesis). Of course, one *can* consider some mathematical situation in which we are able to 'go up one level' (i.e. in which we can investigate multiple interpretations of a background set theory) so the holder of such a view must be able to show why this is not in fact what is going on. Furthermore, we might question why it is that exactly *one* step up is permissible, and not more or less. Perhaps allowing two steps might prove fruitful in some situations, or, again, we might just take 'normal' set theory as the bedrock and be done with it.

Second, and going in the other direction, we could argue that the apparent circularity indicated above is not, in fact, vicious. We might say, for example, that Kreisel's argument establishes the determinacy of CH *simultaneously for all levels*. That is, we take the background set theory to be ZF2 (or perhaps rather to involve an intuitive notion of set which can be formalised — or modelled — by ZF2), and claim that the argument leading to Corollary 2 also happens simultaneously for this background set theory (and its background and so on), so that the possibility of it having incompatible interpretations with respect to CH is blocked.

This notion of 'simultaneity' requires some further working out and justification, but even granted that, someone unconvinced that ZF determines CH might raise the following objection. The strategy does not preclude the possibility of an 'independent tree of levels with a different answer to CH'. I will not treat this issue in full generality here, but will instead paint a mathematical picture which should indicate the form of the problem. I will work with the assumptions (a) that each set theory, background set theory, and so on is ZF2, (b) that interpretations are models in the sense of Model Theory, (c) that every interpretation of a set theory is an object within an interpretation of the background to this set theory, and (d) that we can 'step outside' and look at the whole situation mathematically. With this in mind, we can consider the discrete partial order P of interpretations, where the successor of an interpretation is the 'background' interpretation in which it is an object. Now, the simultaneity argument claims that for every interpretation I, all predecessors of I in P agree on the answer to CH. It does not follow from this however that every interpretation agrees on the answer to CH, for it does not preclude the possibility of two independent (disjoint) trees of interpretations, each with a different answer to CH. See Figure 1 for a picture.

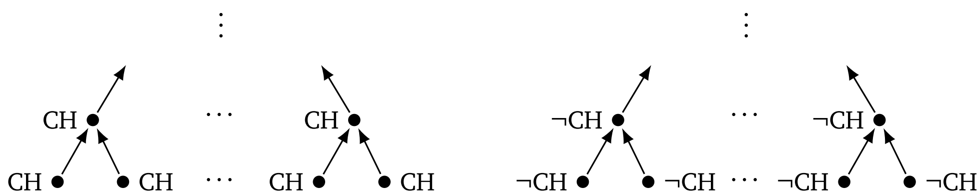

Figure 1: Two 'independent trees of interpretations' with different answers to CH

One way in which we might make all of this more concrete is to return to Weston's talk of "interpretations in the minds of mathematicians". For, we can model a mathematician considering an interpretation in which she proves the second-order determinacy of CH as 'going one step up in the partial order'. Then two mathematicians having disagreeing interpretations of set theory with respect to the Continuum Hypothesis correspond to two points in the partial order at the bottom of independent trees of interpretations with different answers to CH.

Note that if the preceding mathematical analysis really is needed for anything more than a compact and reasonably clear (but eliminable) means to explicate the problem facing the 'simultaneity' strategy, then its use in the argument must be subject to the main thesis of this paper, and hence must require some buttressing before it can hold weight.

§3. Mathematical Models and Independence. We now move away from Kreisel and the Continuum Hypothesis, and worry about the general question of how we can justify the use of mathematical results in arguments leading to conclusions with some non-mathematical content. I will here adopt a certain perspective in which the distinction between mathematical and non-mathematical content is given prominence and which makes the path to the justification of the use of mathematical results clear.

The starting point is this: we will explicitly *not* assume that the things about which we wish to argue mathematically behave in a wholly mathematical fashion. Instead, what we take to be happening is a process of *mathematical modelling*. This is a more general version of the position espoused by Shapiro in his 1998 paper on the notion of logical consequence.

> A formal language is a *mathematical model* of a natural language (of mathematics), in roughly the same sense as, say, a Turing machine is a model of calculation, a collection of point masses is a model of a system of physical objects, and the Bohr construction is a model of an atom. In other words, a formal language displays certain features of natural languages, or idealizations thereof, while ignoring or simplifying other features. (Shapiro 1998)

This perspective separates the mathematical usage in our arguments from that which is under analysis. Talking of the 'emergence' of non-mathematical content is no longer so appropriate: mathematical analysis is subsidiary to philosophical, and serves only to model something which may have a non-mathematical component. The procedure for justification is then clear:

once we have noted that our mathematical results concern a model of what is under analysis, we must provide evidence as to why this model is a good and accurate one with respect to the features relevant to the result. In particular, we should explain why the thing so modelled behaves in a mathematical way, and why the assumptions and rules of the model correspond well enough to what it models, so that the model tracks the real thing, at least as far as the desired result.

How do we now understand the troubles with Kreisel's arguments concerning the Continuum Hypothesis? From the present perspective we can say that the move from Corollary 2 to Conclusion 3 implicitly assumes that second-order determination is a good model for the determination of statements by axioms; the Weston-style argument given above then reveals this assumption as faulty.

There is one kind of evidence for the accuracy of a model which should be regarded as particularly strong. Gödel in his remarks before the 1946 Princeton Bicentennial Conference (see (Gödel, 1946)) notes how the notion of computability displays a kind of 'formalism independence', in contrast with the notions of provability and definability. Kennedy (2013) develops these ideas further, and isolates two senses in which Gödel talks of formalism independence with respect to computability. (a) "Stability under a class of presentations":

> Whether one defines the notion of computability by means of the Gödel-Herbrand-Kleene definition (1936), Church's λ-definable functions (1936), Gödel-Kleene μ-recursive functions (1936), Turing machines (1936), Post (1943) systems, or Markov (1951) algorithms, one ends up with the same class of functions. (Kennedy 2013, p. 362)

(b) "The absence of the sort of relativity to a given language that leads to stratification of the notion such as (in the case of definability in a formalized language) into definability in languages of greater and greater expressive power" (quoted from Parsons' introductory note to (Gödel, 1946)). We are especially interested in the first sense; however, here we talk of 'model independence' as opposed to 'formalism independence'. Let us suppose that we have isolated some aspect of mathematical activity for which we have a *diverse* collection of models, all of which turn out to be equivalent. This is indeed a rather special situation, and — absent of arguments that these models fail to capture something essential — we are furnished with evidence of a strong link between model and modelled.

The qualifier "diverse" plays an important role here. Let us consider the case of computation as a prototype. As indicated above, Gödel and Kennedy consider the various definitions of computation as *formalisms* of an intuitive notion; however, in line with the perspective declared at the beginning of this section, and in agreement with Shapiro (1998), such definitions will here be considered to specify *mathematical models*. Now, what is special about computation is not simply that we have many equivalent models: it seems that *all* ways one thinks to devise a model — and a great variety of methods have been proposed, involving thinking about the problem in very different ways — turn out to specify the same class of functions. This observation forms the key part of the justification for believing in a strong link between the models of computation and computation itself. Thus, in the general case, the fulfilment of this "diverse" qualifier corresponds to providing evidence for a kind of Church-Turing Thesis for the aspect being modelled.

In what kind of situations might we hope for model independence? The notion of computation may sometimes be useful in analysing mathematical activity, but it arguably has much less utility in this respect when compared to the notions of provability and definability.

Gödel's lecture takes the form of a challenge: to conduct, for these two notions (and others), an analysis analogous to that of computability, arriving at a kind of formalism independence. Kennedy takes up this challenge with respect to the second, and forms her Church-Turing Thesis for definability in terms of the set-theoretic hierarchy constructible using a particular logic. This programme, if successful — and as long as we can convert any arguments for formalism independence into arguments for *model* independence — should furnish us with the material with which we may securely buttress arguments which employ mathematical results concerning the notions of provability and definability.

§4. Conclusion. Kreisel does not provide sufficient justification for the employment mathematical results in obtaining his not-entirely-mathematical conclusion. Even granted the possibility of patching up his argument, this determination stands. We *should* worry about how to justify the use of mathematical techniques and results in our arguments.

I considered above one potential path leading to such justification, which is visible from a view that very much embraces the distinction between mathematical and non-mathematical content. It would however be interesting and sensible to examine a more conservative reaction, one which tries to justify in a negative fashion: by reducing the importance and urgency of the problem. For instance, one might claim that some apparently purely-mathematical results already have non-mathematical content. Or, going in the other direction, it might be contended that mathematical analysis exhausts what there is to say about mathematical activity, so that any appearance of non-mathematical content is illusory, or points to faulty reasoning.

In any case, the principle aim of the considerations made here is to clarify and make precise investigations of mathematical activity. Doing so is very much in the spirit of Kreisel's programme of informal rigour, and I regard the present essay as product of his rich and stimulating 1965 paper.

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