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Reply to Crispin Wright and Richard Zach

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I am very grateful to Crispin Wright and Richard Zach for their thoughtful and penetrating comments on my book, *The Boundary Stones of Thought* (henceforth *BST*; unadorned page numbers below refer to this volume). The author of any reasonably long philosophical work will feel more confident in some of his claims than in others, and in studying Wright's and Zach's essays I have been struck by how often they home in on elements which gave me most trouble when writing the book and of which I was least certain having finished it. While that makes the job of defending my position harder, it is in one important respect comforting, for it suggests that the book was sufficiently clearly written to have enabled this meeting of minds. In a symposium of this kind, there can be few things more depressing than for the author to have to write: '*X* attributes to me such-and-such an argument, but what I really meant was instead this'. Thankfully, very little of that will be needed here.

While Wright and Zach focus on different parts of *BST* (Zach on Chapters 6–8, Wright on Chapters 9 and 10), there are many thematic connections between their commentaries. Rather than give separate replies, then, I shall address the relevant topics in the order in which they appear in my book, dealing (as I hope) with all their main points as I go.

1 Possibilities, pretopologies, and the meanings of the connectives

I am particularly grateful to Richard Zach for the very clear account he gives (in §1 of his paper) of the relationship between my ideas and the semantic theory presented in Giovanni Sambin's paper 'Pretopologies and Completeness Proofs' (Sambin

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1995). Any attempt to provide reasons for preferring one logical school to another runs the risk of falling into circularity through appealing, in the meta-logic, to rules which members of the preferred school accept but which their rivals do not. Since Sambin's completeness proofs need only a weak meta-logic, their potential utility in avoiding this kind of circularity, and thereby advancing a philosophical comparison of rival logical systems, was clear. As Zach remarks, though, my purposes are different from Sambin's. His method of proving completeness applies to a wide range of logics, whereas I was chiefly concerned with the contest between classical logic and its intuitionistic rival. (In one section of *BST*, §6.6, I consider the case for restricting proof by cases in the way that is characteristic of quantum logic.) Accordingly, the generality of Sambin's theory was largely unexploited in *BST*.

Much more importantly, though, I offered a novel interpretation of that theory. For most of his paper, Sambin treats his pretopology purely algebraically, viz. as a structure with specified mathematical features. To be sure, he proposes at one point 'an independent intuitive interpretation [of the pretopology] as a universe of concretely produced objects, or occurrences of pieces of information, which can always be combined by means of •' (i.e. the combination operator of the pretopology) (Sambin 1995, 862). My interpretation is very different. For reasons set out in Chapter 3 of *BST*, I regard logical consequence as an inherently modal notion; when a conclusion follows logically from some premisses, the conclusion is true at any logically possible circumstance at which all the premisses are true. Accordingly, any characterization of the logically relevant parts of the meanings of the connectives and quantifiers must specify how they contribute to determining the logically possible circumstances at which statements containing them are true.

What, though, is a possible circumstance? For reasons given in §§6.1 and 6.2 of *BST*, I have long been dissatisfied with the now prevalent answer—namely, that it is a set of possible worlds, a possible world being a fully determinate way in which all the things in the universe could be or could have been. What struck me about Sambin's theory is that it provides a way of theorizing rigorously about possible circumstances (or, as I call them, *possibilities*) without assuming determinacy. As Zach remarks, the two key notions in a pretopology are the *combination* of two elements, and that of a set of elements being *closed* (or, as Sambin prefers, 'saturated'). Both these notions find ready application when the elements are possibilities. There is a natural conception of the combination of two possibilities—although not every combination of two possibilities is itself possible, which is why I supplemented the space of elements with an impossible circumstance, $\perp$. Moreover, if we call a set of possibilities *closed* when it contains every possibility which includes what all its members have in common, we can show (a) that this notion satisfies the conditions for a pretopological closure operation and (b) that the set of possibilities at which a given statement is true (what I call its *truth-grounds*) will always be closed in this sense. Since the closed sets of a pretopology are 'well-behaved' from a logical point of view (cf. Sambin 1995, 863), it was finding this interpretation of his formal system that enabled me to apply Sambin's theorems in advancing my philosophical project of rationally comparing rival logical systems.

What, though, of the particular characterizations of the meanings of the connectives that I gave in this framework? Zach (2018, introduction, p. 2) is entirely right to say that I intend my semantic axioms for '$\wedge$', '$\vee$', and '$\neg$' to capture the logically relevant aspects of the meanings of the familiar English connectives 'and', 'or', and 'not'. While the construction of formal languages can certainly expedite the assessment of deductive arguments, most of the deductions which interest mathematicians and philosophers are expressed either informally or semi-formally, so the significance of the discipline of logic would be greatly reduced were it not applicable to arguments propounded in English and other natural languages. Whether the axioms I proposed capture the logically relevant aspects of these ordinary words' meanings is clearly an empirical matter, to be tested by comparing their implications with judgements made by native speakers as to whether statements containing the words are true, or not, in a wide range of specified possible circumstances. A full vindication of the proposed semantic axioms along those lines is clearly out of the question here. I can, though, reply to the particular objections which Wright and Zach lay against my proposals.

(Z) As Zach notes, the introduction of the impossible circumstance $\perp$ induces a relation of incompatibility between elements of the pretopology: x is incompatible with y (written '$x \perp y$') if and only if $x \bullet y = \perp$. I used this notion of incompatibility in stating the semantic axiom for sentential negation. The *orthocomplement*, $U^{\perp}$, of a set of possibilities U, contains exactly those elements of the pretopology that are incompatible with all the members of U. The truth-grounds of $\ulcorner \neg A \urcorner$ are then identified as the orthocomplement of the truth-grounds of A. In the notation that I used throughout *BST*, $|\neg A| = |A|^{\perp}$. Zach does not directly challenge my treatment of negation. He suggests, though, that it exposes a limitation in my method of adjudicating between rival logical systems (Zach 2018, §2, p. 5). Let us say that a statement *has a back* when the set of its truth-grounds is the orthocomplement of another set. Under my semantics, we shall get classical logic if, and only if, any statement has a back. If we require only that the truth-grounds of a statement form a closed set, we get only intuitionistic logic. Zach complains that this treatment throws little light on particular cases where classicists accept, while intuitionists deny, the applicability of (say) the Law of Double Negation Elimination. 'Possibilities', he writes, 'only exist as truth-grounds for possible statements' (Zach 2018, §2, p. 5), so a judgement about whether a statement has a back simply reduces to one about its logical relations to other statements. Zach, however, gives no argument for this claim about the metaphysical status of possibilities, and it is one I would firmly reject. There were ways the Sun actually was, and other ways it was not but could have been, long before there were any speakers capable of making statements about it. To this extent, we should be 'realists' about possibilities. The logical relations that a given statement enters into are certainly *evidence* for its having, or not having, a back. However, a statement's having a back does not reduce to the obtaining of those relations. Reconceiving the debate between classical and intuitionistic logicians as one concerning the structure of the space of logical possibilities was vital to exposing the fallacy in Dummett's argument that the rival parties *must* be assigning different meanings to the connectives (see *BST* §7.5). I want to hear a good reason for rejecting a realist view of possibilities before sacrificing that reply.

(*W*) Wright (2018, §III, p. 8) objects to my treatment (in *BST* §9.5) of disjunctive statements made in the language of set theory. When interpreting this language, I contended, the relevant possibilities are axiom systems that consistently extend second-order ZF (see *BST* §9.3). Since Wright does not challenge this claim, I shall take it as agreed for present purposes. I then argued that the best way to interpret the language of set theory was via a two-stage process. We start by constructing a Kripke semantics for the language, by reference to the recommended space of possibilities. The fundamental semantic relation in such a theory is that of a possibility's (i.e. an axiom system's) *forcing* a formula. s forces an atomic formula A if and only if s semantically entails A (p. 279). Recursive clauses then extend the forcing relation to complex formulae. The clause for disjunction says that s forces $\ulcorner A \vee B \urcorner$ if and only if s either forces A or forces B (*ibid.*). Given this clause, there will be axiom systems which leave certain instances of Excluded Middle unforced. On my view, however, this theory of forcing is only a station we pass through *en route* to a proper semantics for the language of set theory. The key semantic notion is, as ever, truth at a possibility and we should not say that a formula is true at a possibility (i.e. at an axiom system) only when the system forces the formula. Rather, we should apply a negative translation and say that a formula is true at s if and only if s forces its double negation (p. 288). It may then be shown that every instance of $\ulcorner A \vee \neg A \urcorner$ is true at every s, so the whole interpretation does validate Excluded Middle (and, in fact, all the laws of classical logic).

Wright objects that by this interpretation I have merely 'saved something that *looks like* classical logic, at the cost of jettisoning the intended understanding of any concept that incorporates a pukka, properly distributive disjunction' (Wright 2018, §III, p. 8). As he appreciates, I reject the thesis that disjunction must be distributive; that is, I reject the thesis that any true disjunction must contain a true disjunct. I shall return to that bone of contention in the following section. Independently of that issue, though, it may seem that my interpretation of the language of set theory does not really vindicate Excluded Middle, but instead merely validates all instances of $\ulcorner \neg\neg(A \vee \neg A) \urcorner$. Since even an intuitionistic logician accepts *that* schema as a logical truth, I may seem to have provided no defence of the use of classical logic in set theory.

While this may be a tempting objection to make, it is wholly mistaken. I would, indeed, have failed to vindicate Excluded Middle if the '$\vee$' of the language of set theory did not mean 'or'. However, when we consider closely how we actually use signs for disjunction, we can see that the proposed two-stage interpretation does ensure that '$\vee$' means 'or'. In order to see this, the first point to note is that, if we tried to dispense with the interpretation's second stage and identified truth with being forced, we would be notably unfaithful to the ordinary meaning of 'or'. The identification would be tenable only if all consistent extensions of ZF^2 were guaranteed to have the disjunction property. As I remarked on pp. 131–132 of *BST*, though, there is no general guarantee even that constructive mathematical theories have this property, and some of the extensions of the intuitionistic second-order set theory ZF^- that I consider in §9.6 lack it. The underlying point here is a general one that arises whenever one specifies the meaning of 'or' by reference to possibilities that are not fully determinate; it has nothing specifically to do with the language of set theory. As I explained on p. 156 of *BST*, it would mischaracterize our use of 'or' to say that $\ulcorner A$ or $B\urcorner$ is true at a possibility x if and only if either A is true at x or B is true at x. For 'Either a boy or a girl is at home' is true at the possibility of a child's being at home, even though neither disjunct is true there. What we have to say instead is that $\ulcorner A$ or $B\urcorner$ is true at x if and only if x belongs to the *closure* of those possibilities at which either A or B is true. The two-stage interpretation for the language of set theory implements this general point about the meaning of 'or' in the case where the relevant possibilities are consistent extensions of ZF^2.

There remains a question, though. As already noted, my semantic theory interprets '$\neg$' as signifying the operation of orthocomplementation. To take A to be true at s when s forces $\ulcorner\neg\neg A\urcorner$, then, is to take the closure of a set of possibilities to be its double orthocomplement. Why should closure amount to double orthcomplementation in the present case? Again, the essential part of the explanation has nothing specifically to do with set theory but lies in a general point about content. Throughout *BST*, I recommended adopting what I call an 'exclusionary' conception of content. Following Ramsey, Dummett, and Stalnaker, I claim that our best grip on the possibilities at which a statement is true comes from first identifying the possibilities which the statement rules out. Now the possibilities which $\ulcorner A$ or $B\urcorner$ rules out are precisely those which falsify both A and B. That is to say, they are precisely those possibilities which are ruled out by A and by B. Contrary to what Wright says (2018, §III, p. 8), this account of the meaning of disjunction in no way presupposes Bivalence: it requires only the validity of the restricted form of proof by cases (see *BST* p. 118). It does, however, show why closure must, in the present case, be equivalent to double orthocomplementation. For the truth-grounds of $\ulcorner A \vee B\urcorner$ are $\left(|A|^{\perp} \cap |B|^{\perp}\right)^{\perp}$, which in turn equals $(|A| \cup |B|)^{\perp\perp}$—the double orthcomplement of the union of the truth-grounds of A with those of B.

2 Vagueness

Wright and Zach raise rather different objections to my treatment of vagueness.

(*W*) I accept the Law of Excluded Middle while rejecting the Principle of Bivalence. As Wright appreciates, defending this position involves diagnosing the fallacy in the apparently compelling Aristotelian argument which purports to derive the Principle from the Law (I call this the 'Simple Argument'). My diagnosis is as follows. Consider the inference 'u says that P; P; therefore u is true'. I accept that this inference is valid in the sense that its conclusion is true whenever both its premisses are true. I hold, however, that the inference cannot licitly be applied when it is indeterminate whether P. In the Simple Argument, however, the inference is applied as a subordinate deduction for arbitrary u. Accordingly, that Argument only goes through for those utterances for which we have a guarantee that it is never indeterminate whether what they say is the case. Consider now an utterance, v, which attributes the property of being red to a tube of paint, t, whose perceived colour is equidistant between polar red and polar orange. (Talk of equidistance in this context may be more than a metaphor; see Gärdenfors 2000, 71–78.) This utterance does not meet the specified condition. We can, then, consistently affirm 'Tube *t* is either red or not red' while rejecting 'Utterance *v* is either true or false'. I detect the same fallacy in the parallel argument for the conclusion that a true disjunction must contain at least one true disjunct (cf. *BST* §8.7), which is why I do not accept that the operation of disjunction must be distributive.

Wright contends that my treatment of utterances like *v* 'is a great mistake. Indeterminacy in those cases, properly viewed, is just that—a situation where neither truth nor falsity is *settled*; where the matter of truth-value is open, a situation consistent with each of the poles. If the truth-value of a statement is unsettled, that is not a way of *failing* to be true. To think otherwise is to take indeterminacy to be a kind of settlement after all' (Wright 2018, §III, p. 9). Wright refers to some of his published papers for a justification of this position.

I have read all of those papers and, at the risk of seeming unteachable, must confess that I find their argument unconvincing. Their central thesis is that thinking of indeterminacy as a third alethic status, on a par with truth and falsity, misrepresents the phenomenology of vagueness. Recognizing such a third status, Wright contends in one of these papers, is

> *un premier pas fatal*. It is quite unsatisfactory in general to represent *in*determinacy as any kind of determinate truth-status—any kind of middle situation, contrasting with both the poles (truth and falsity)—since one cannot thereby do justice to the absolutely basic datum that in general borderline cases come across as hard cases: as cases where we are baffled to choose between conflicting verdicts about which polar verdict applies, rather than as cases which we recognize as enjoying a status inconsistent with both (Wright 2001, 70).

This argument seems to me to confuse verdicts about whether a vague non-semantic predicate applies to objects within its range of significance with verdicts about the semantic classification of statements. Let us revert to our borderline red–orange tube, *t*. Suppose that the only colour predicates in our language are the so-called 'colours of the rainbow'. Then, if we are called upon to say what colour *t* is, we shall indeed be baffled. Since, *ex hypothesi*, tube *t* is perceived to be equidistant in colour between polar red and polar orange, we shall be torn between classifying it as red and classifying it as orange. We may introduce a new colour term—as it might be 'cinnabar'—of which *t* will be a paradigm. But we need not do this and, even if we do, there will be other tubes of paint that are equidistant in colour between polar cinnabar and polar red. So we may accept that assigning a colour to tube *t* is, in Wright's sense, a hard case.

It simply does not follow, though, that the alethic classification of our statement *v* must also be a hard case. Because the phenomenon of indeterminacy is ubiquitous, we have every reason to posit a third alethic status—*indeterminate*—which contrasts with both truth and falsity. 'Indeterminate' will be vague, just as 'true' and 'false' are. All the same, statement *v* is a paradigm of this third status. Moreover, despite its vagueness, the term 'indeterminate' has a clear utility when classifying statements, a utility that lies partly in the fact it has other paradigms among statements ascribing very different properties ('John is bald', 'Susan is rich', etc.). This third status, moreover, is as settled as any alethic status needs to be. To be sure, if we were to introduce the term 'cinnabar' into our language in such a way that it is understood to be inconsistent with 'red', then the alethic classification of *v* would shift from *indeterminate* to *false*. However, the meaning of 'red' is specified by the entire system of colour poles (see *BST* pp. 239f.), so introducing a new pole changes that meaning. It is then no surprise that *v*'s alethic status should also change.

On any view, there must be a close connection between the colour classification of *t* and the alethic classification of *v*. What is at issue, though, is what this connection is. As I remarked on pp. 258–259 of *BST*, if *t* were a polar case of red, *v* would be a polar case of truth, and if *t* were a polar case of blue, *v* would be a polar case of falsity. In fact, *t* is not a polar case of any recognized colour, but it does not follow that *v* cannot be a polar case of any alethic status. It is entirely open to us to take it to be a polar case of *indeterminate*. Introducing that status is not a fatal misstep. Rather, it is a sensible and helpful move to make when theorizing about vagueness.

(*Z*) Richard Zach's doubts about my account of vagueness concern my formal semantic theory of polar predicates (*BST* §§8.3–8.4). He finds merit in the theory as it applies to monadic predicates (Zach 2018, §4, p. 8). He is, however, unconvinced by its extension to multi-place predicates and to complete sentences (*op. cit.*, §4, p. 9). He worries that the number of poles invoked by the theory increases exponentially with the number of objects referred to in a statement or argument. He also cannot see how my solution to the *Sorites* paradox preserves classical logic.

Let me say at once that I share Zach's first worry. I am glad that he finds it illuminating to take the semantic value of a monadic colour predicate to be a regular open set in the topology generated by a finite number of colour poles; I still find it so. I agree, however, that my way of extending the theory to sentential logic via the product topology is cumbersome and artificial. I expect there is a better way of doing this, but so far I have not found it. I hope that someone will.

Cumbersome though it is, however, I still maintain that my solution to the *Sorites* preserves classical logic. Following Zach (2018, §4, p. 9), let $\ulcorner Ra_i \urcorner$ be a statement saying that the *i*th member of the sequence of tubes of paint is red, let A_i be the statement $\ulcorner \neg(Ra_i \wedge \neg Ra_{i+1}) \urcorner$, and let C be the conjunction $\ulcorner A_1 \wedge \ldots \wedge A_{99} \urcorner$. As Zach observes, the negation of C follows classically from the premisses $\ulcorner Ra_1 \urcorner$ and $\ulcorner \neg Ra_{99} \urcorner$. Moreover, the negation of C is classically equivalent to

$$\neg A_1 \vee \ldots \vee \neg A_{99}$$

i.e. to

$$(Ra_1 \wedge \neg Ra_2) \vee \ldots \vee (Ra_{99} \wedge \neg Ra_{100}).$$

This last formula *seems* to imply that somewhere in the sequence there is a sharp cut-off, thereby reinstating the paradox. Under the proposed semantic theory, however, the long disjunction does not actually imply this. A sharp cut-off would be a case where some particular tube, a_n, was red but where its successor a_{n+1} was not.

Without the semantic principle that a true disjunction has at least one true disjunct, we cannot infer that there is such a case.

3 Categoricity and determinacy in mathematics

I turn next to another topic which engaged both commentators: the role of categoricity in the philosophy of mathematics.

(*W*) Chapter 9 of *BST* is devoted to close analysis of an argument against the use of classical logic in set theory that I found in the writings of William Tait (see especially Tait 1998). Wright comments that Tait's argument 'draws on assumptions about categoricity of axioms and determinacy of content that are…quite prevalent in contemporary philosophy of mathematics' (Wright 2018, §II, p. 6). There is no doubt that they are widely accepted. Wright, however, believes that 'these assumptions are in general very questionable and that the Tait argument, as a stand-alone challenge [to the use of classical logic in set theory], is weakened in consequence' (*ibid.*).

Wright begins his attack by questioning the claim that 'categoricity suffices for determinacy' (2018, §II, p. 6). In describing this claim as 'strictly orthogonal to the present purpose' (*ibid.*) he is, I trust, acknowledging that neither Tait nor I advanced it. Like Wright, I think that the claim is false.

What Tait's argument requires is the converse claim, viz. that categoricity is necessary for determinacy. Wright deems this claim 'very challengeable' (2018, §II, p. 7). When properly understood, I now argue, it is eminently defensible.

Wright's challenge runs as follows:

> That a mathematical subject matter admits of a categorical axiomatisation is surely not a necessary condition for the availability of a determinate conception of it. Most of us believe that we have a determinate conception of the structure of the natural numbers, but it would be very far-fetched to claim that the source of that—the way we arrive at that conception—is via the categorical axioms of second-order Peano arithmetic. Rather there is, or so we think, a determinate intended interpretation of arithmetic which is already available at first order and precisely contrasts with what we recognize as the unintended interpretations which first-order arithmetic allows (Wright 2018, §II, p. 7).

These points concern arithmetic, but Wright goes on to draw parallels with the pertinent case of set theory:

> Suppose a set theorist claims <to have>, correspondingly, an intended conception of the universe of sets that transcends what is characterised by ZFC^2. Then a philosopher who is running the Tait argument needs to be able to argue either that there is no such concept to be had or, perhaps more plausibly, that any such concept must anyway be in turn indeterminate in certain respects. Given, however, that whatever the set theorist may have to say by way of articulation of her claimed conception will, as she will

> acknowledge, not fare any better than the ZFC^2 axioms in point of categoricity, a *dialectically effective* argument that the claimed conception does not eliminate indeterminacy will have to resort to other considerations (*ibid.*).

Those other considerations, Wright thinks, lie in the 'indefinite extensibility of the pre-formal notion of *set*'.

Although the argument that Wright is attacking concerns set theory, let me first reply to his points about arithmetic. The first thing to say is that the 'far-fetched' claim that the 'source' of our conception of the natural numbers is 'via the categorical axioms of second-order Peano arithmetic' is a straw man. I agree that it is far-fetched, and expressly rejected it on p. 206 of *BST*. Indeed, nowhere in the book do I put forward a positive view about the 'source' of our conception of the natural numbers; that is surely a matter for psychologists, not philosophers. My argument for the thesis that number theory must have a categorical axiomatization if we are to have a determinate conception of the natural numbers has nothing to do with 'sources'. Rather, it runs as follows. Any thinker who has a determinate conception of the natural numbers must have a grasp of the distinction between collections that are finite and those which are not. Yet, as soon as one grasps the notion of finitude, one has the key conceptual resource needed to produce a categorical axiomatization of arithmetic. It is in this sense that categoricity is necessary for—one might better say 'implicit in'—determinacy. There are various ways of giving a categorical characterization of the natural numbers. The most direct is simply to include an axiom saying that every natural number is the result of applying the successor operation to zero a finite number of times; this excludes the non-standard models of arithmetic and thereby ensures that all the remaining models are isomorphic. In fact, on p. 207 of *BST*, I took a slightly less direct route by using John Myhill's 'ancestral logic', in which an operator, *, which forms the ancestral of any two-place relation is designated to be a logical constant (Myhill 1952). In this system, one may produce a categorical axiomatization of arithmetic by adding $\forall x(Nx \rightarrow x = 0 \vee S^*0x)$ to the first-order Peano postulates. (Here, of course, 'Nx' means 'x is a natural number and 'Sxy' means 'y immediately succeeds x'.) The philosophical point, however, is the same: it is one's grasp of the notion of finitude which enables one to grasp the intended sense of Myhill's * operator. For to explain that operator one says: R^*xy if and only if there is a finite sequence of objects $a_0,\ldots, a_n$ such that $x = a_0$, $y = a_n$, and for each i between 0 and $n - 1$, Ra_ia_{i+1} (see *BST* p. 206).\[1]/

What, though, of the relationship between categoricity and determinacy in set theory? Wright imagines a theorist who claims to have a determinate conception of the set-theoretic universe even though she acknowledges that she can give no categorical characterization of it. As he says, an adherent of Tait's argument will

[1] I confess that I do not understand Wright's suggestion that 'a determinate intended interpretation of arithmetic…is already available at first order' (2018, §II, p. 7). Grasping the intended interpretation surely involves making sense of the notion of finitude, which cannot be characterized in first-order logic. The conceptual resources needed to understand ancestral logic are significantly weaker than those required to understand full second-order logic. Ancestral logic, however, is still stronger than first-order logic.

contend that this conception must be indeterminate in certain respects. Wright complains, however, that Tait can give no 'dialectically effective' argument to show that her conception must be indeterminate. Accordingly, Tait's argument needs to be bolstered by another argument for that conclusion. Drawing on some suggestions of Dummett, Wright advances what he takes to be a more persuasive argument in the first section of his paper.

I shall address Wright's argument in the next section. For now, though, let me explain why I do not see the dialectical situation regarding Tait's argument in at all the way that Wright does. Tait's argument is intended to undermine confidence that all the statements of set theory are bivalent. To say that the imagined set-theorist's conception is 'indeterminate in certain respects' is, then, to say that it does not ensure that every statement of set theory is either true or false. Now we are surely entitled to ask the theorist what it is about her conception of the set-theoretic universe that makes her confident that every statement that can be formulated in the language of set theory is either true or false. And then the point is that, if she acknowledges that her conception is not categorical, she cannot avail herself of a natural answer to this question. For there is a general argument, which I set out on p. 268 of *BST* (for the case of number theory) and again on p. 315 (for set theory), which purports to show, given classical logic and a categorical axiomatization of a theory, that statements made in the language of the theory must be either true or false. As I remarked on p. 316, I regard this argument as questionable. However, it is at least an argument. So far as I can see, once Wright's theorist acknowledges that her conception of the universe of sets is not categorical, she is left with no argument for saying that an arbitrary set-theoretic statement must be bivalent.

It may be said that all this shows is that we have no positive reason to assert that every such statement is either true or false. Tait gives us no reason to hold that some such statements are neither. Tait's reasoning, however, has a number of stages and, having shown that the usual general argument for bivalence is inapplicable to set-theoretic statements, we can look more closely to see whether they really are bivalent. It is here that Tait's focus on the particularities of set theory comes into its own. There is every reason to reject bivalence if there are incompatible but equally consequent expansions of ZFC^2 (Tait 1998, 478; cf. *BST* p. 277). Indeed, if the sense of 'set' remains constant while we adopt successively stronger axioms, we have direct challenges not only to bivalence but to certain laws of classical logic, notably to $\ulcorner \forall x \varphi x \vee \exists x \neg \varphi x \urcorner$ (Tait, *ibid.*; cf. *BST*, pp. 280–281). For these reasons, Tait's argument still seems to me to be a powerful and self-standing challenge to the use of classical logic in set theory. As I argue in Chapter 9, it can be answered. However, it needs no bolstering by considerations about indefinitely extensible concepts.

(*Z*) Zach's attack on the use I made of the notion of categoricity is very different. For my argument for the determinacy of the natural number structure in *BST* §7.3 to be dialectically effective, the intuitionist, as well as the classicist, must 'accept the result that ancestral logic provides a categorical characterization of the natural number structure' (Zach 2018, §3, p. 6). According to Zach, though, 'it is not clear that he must accept' this. Zach admits that he has no 'proof that no intuitionistically acceptable argument can be given' (*ibid.*), but he gives two reasons for doubting that it can. First, the very formulation of categoricity involves quantifying over models, i.e. completed infinite sets. Second, the theory of the ancestral operator conservatively extends Heyting arithmetic (HA).

There are, in fact, more directly relevant formal results than the one Zach cites. Zach is correct to say that an intuitionist will not accept the *classical* conception of a model as a completed infinite set. The pioneering investigations of Kreisel and Kleene, though, led to what is now a well-established body of mathematics, constructive model theory, which explicates metamathematical notions such as categoricity in intuitionistically acceptable terms. Some 30 years ago, Charles McCarty made a striking contribution to constructive model theory by showing that even first-order Heyting arithmetic is categorical (McCarty 1988). His result brings out deep differences between classical and intuitionistic metamathematics, and makes it unsurprising that the theory of the ancestral operator should conservatively extend Heyting arithmetic. From the perspective of a McCarty-style constructivist, first-order HA is already categorical.

McCarty's proof has premisses; Zach may think that an intuitionist should not accept them. The active ingredients are weak versions of Markov's Principle and Church's Thesis. The form of Church's Thesis used in the proof says that every decidable predicate of natural numbers is almost recursive; this is widely accepted by contributors to constructive model theory and McCarty has defended it (McCarty 1991, 336–337). Even when restricted to decidable predicates, which all McCarty's proof needs, Markov's Principle is more doubtful. However, McCarty's example reminds us that some mathematicians with constructivist sympathies accept weak versions of the Principle, so my argument will have purchase on them. Moreover, there are categoricity theorems for subsystems of HA which do not require any form of Markov's Principle (see e.g. Visser 2006).

4 Wright on indefinite extensibility

Crispin Wright devotes the first section of his paper to reconstructing what he takes to have been Michael Dummett's argument that statements quantifying over all ordinals cannot be assumed to be bivalent.\[2]/ I did not discuss this argument in *BST*, but Wright thinks it needs to be brought into bolster an argument I did discuss, namely Tait's. I have already said why I believe that Tait's argument can stand on its own. I shall conclude by explaining—perforce briefly—why I think that Wright's argument is weaker than Tait's. Wright's argument involves a premiss which is independently doubtful; when we reject it, as we should, his challenge to Bivalence falls away.

Wright's argument goes like this. It starts by adopting an 'unrestrictive' conception of the ordinals. That is, it starts by assuming that every well-ordered

[2] I shall not try to judge how faithful Wright's reconstruction is to Dummett's intentions. However, the fact that Øystein Linnebo has recently produced a similar, but in crucial respects distinct, reconstruction founded on the same Dummettian passages may be thought to support my complaint that those texts are somewhat 'dark'.

series has an ordinal number greater than that of any of its proper segments. When so conceived, the ordinals may be extended by any possible number of iterations of the successor and limit operations. However, what it is for a number to be 'possible' is that it appears somewhere in the series of ordinals. The tight explanatory circle that arises here makes it impossible, even in principle, for us to attain any determinate grasp of the extent of that series. According to Dummett's anti-Platonism in the philosophy of mathematics, though, a mathematical domain is determinate only if we humans can in principle attain a determinate grasp of it. It follows that the extent of the ordinals is inherently indeterminate. Hence we cannot think of the truth-value of a statement which quantifies over all ordinals as being determined by the truth-values of its instances. Instead, such a statement will have a truth-value only when that value is settled by the mathematical axioms that characterise ordinals (what Wright calls the 'rules of construction'). Thus the statement 'Every ordinal is self-identical' does have a truth-value, because those rules stipulate that an ordinal is a Fregean object, and that every Fregean object is self-identical. However, 'Every ordinal is less than ε_0' does not have a truth-value, unless the rules stipulate that the series of ordinals either does, or does not, go beyond ε_0.

The argument is interesting and ingenious, but I think we should reject its starting point. Not only do we have no reason to accept the unrestrictive conception of the ordinals that it requires, we have strong reason to reject the conception, for the assumption that every well-ordered series has an ordinal number greater than that of any of its proper segments is the active ingredient in the derivation of the Burali-Forti Paradox.

In a footnote, Wright acknowledges that he needs to say something about that paradox. We await his solution with interest.\[3]/ Meanwhile, my own solution is this (see Rumfitt 2018). The moral of Burali-Forti's Paradox is that some restriction must be placed on the Comprehension Schemata of second-order logic: $\exists X \forall x (Xx \leftrightarrow \varphi x), \exists R \forall x \forall y (Rxy \leftrightarrow \psi xy)$, etc. Specifically, I propose that we only have a legitimate instance of Comprehension when the formulae 'φx', 'ψxy', in the schemata may be shown to be Δ^1_1. This restriction is not motivated solely by the need to block the paradox (although it does block it). It also captures the thought that underlies Dummett's anti-Platonism—viz. that a mathematical property, or relation, exists only if we can in principle apprehend it.

Suppose we solve Burali-Forti's Paradox by imposing this restriction on Comprehension. Then we must sacrifice the unrestricted conception of the ordinals. Indeed, the notion of *ordinal* ceases to be an example of an indefinitely extensible concept in Wright's absolute sense. All the *bona fide* ordinals are bounded by the Feferman-Schütte ordinal Γ_0, the so-called 'limit of predicativity' (see Feferman 1964). We can, then, identify the extent of the ordinals. The sky, one might say, has

[3] Especially since, in the paper co-authored with Stewart Shapiro that he cites, Wright had appeared pessimistic about finding a solution. Having canvassed what they then took to be all the available options, he and Shapiro wrote that every solution 'has difficulties which would be justly treated as decisive against it, were it not that the others fare no better' (Shapiro and Wright 2006, 293).

a limit, namely Γ_0. On this view, Wright's argument for the claim that statements which quantify over all ordinals are non-bivalent falls to the ground.

This analysis of the Burali-Forti Paradox may have revisionary consequences for classical logic. It definitely has such consequences if you count the unrestricted Comprehension Schemata as part of classical logic. For myself, I regard these schemata as principles of metaphysics rather than of logic, but on any view they are a far cry from the logic of conjunction, disjunction, and negation, which is what I was concerned with in *BST*. More interestingly, this theory of the ordinals eventually led Feferman to put forward a 'semi-constructive' logic, in which unbounded quantification is intuitionistic while bounded quantification is classical. The issues here are closely related to those surrounding the Kripke-Platek set theory which I discussed in §9.6 of *BST*, although they are not identical: I reject the view that ordinals are sets; rather, what is constitutive of ordinals is that they conform to a certain abstraction principle. But I must refer readers to my forthcoming paper for a fuller treatment of these difficult matters.

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