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ORIGINAL PAPER

Mathematical determinacy and the transferability of aboutness

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Abstract Competent speakers of natural languages can borrow reference from one another. You can arrange for your utterances of 'Kirksville' to refer to the same thing as my utterances of 'Kirksville'. We can then talk *about* the same thing when we discuss Kirksville. In cases like this, you borrow "aboutness" from me by borrowing reference. Now suppose I wish to initiate a line of reasoning applicable to any prime number. I might signal my intention by saying, "Let p be any prime." In this context, I will be using the term 'p' to reason *about* the primes. Although 'p' helps me secure the aboutness of my discourse, it may seem wrong to say that 'p' refers to anything. Be that as it may, this paper explores what mathematical discourse would be like if mathematicians were able to borrow freely from one another not just the reference of terms that clearly refer, but, more generally, the sort of aboutness present in a line of reasoning leading up to a universal generalization. The paper also gives reasons for believing that aboutness of this sort really is freely transferable. A key implication will be that the concept "set of natural numbers" suffers from no mathematically significant indeterminacy that can be coherently discussed.

Keywords Categoricity · Set theory · Continuum hypothesis

1 Porosity

The concept "set of natural numbers" suffers from no mathematically significant indeterminacy that can be coherently discussed. The same holds for the concepts "real number," "set of real numbers," "set of sets of real numbers," and so on. To understand why this is so, we must first reflect on our capacity to let other people determine what we ourselves are discussing.

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Suppose I overhear my daughter Emily saying, "These penguins do so-and-so." This sounds interesting; so I step into the conversation and say, "Tell me more about these penguins of yours." Now if Emily has been referring to some particular penguins, she will assume that I too am referring to those penguins, even though I may have no idea what penguins they are. I am certainly encouraging her to make this assumption when I use the phrase "these penguins *of yours*." Unless I am being intentionally deceptive, I will understand myself to be referring to the very penguins to which Emily has been referring, if indeed she has been referring to some particular penguins. If I am wrong about what I am doing, if I am really referring to some other things or maybe to nothing at all, then my idiolect is wonderfully suited to produce miscommunication. Since I have good evidence that my idiolect routinely allows me to communicate successfully, I am going to suppose that I really was able to let Emily determine what penguins I was discussing. I am going to suppose that my idiolect is *referentially porous*; that is, porous to other speakers' successful acts of referring.

This much should not be too controversial. It is widely acknowledged that we can "borrow" reference from other speakers. *Referential* porosity amounts to no more than that. But the central argument of this paper requires that we recognize a form of porosity that we might hesitate to call "referential" (unless we have quite a broad concept of reference). Employing terms that refer is one way of securing the "aboutness" of our discourse. Reference borrowing is one form of aboutness borrowing. Readers can decide for themselves whether the form of aboutness borrowing we are now going to discuss deserves to called "reference borrowing" or something else.

It is possible that Emily was not using "these penguins" to refer to any particular penguins. She might have earlier said, "Consider any emperor penguins less than two years old," or, "Consider any emperor penguins who share a common parent," or even, "Consider any penguins at all." Then she will be using subsequent occurrences of "these penguins" to reason about any penguins drawn from the indicated range. If I join the conversation and start talking about Emily's penguins, then the most natural assumption for Emily to make is that the phrase "Emily's penguins," as I employ it, has the same range of possible values as the phrase "these penguins," as she is employing it. Again, I am encouraging her to make this assumption when I use the phrase "these penguins *of yours*." I realized all along that she was not necessarily referring to any particular penguins. I was open to the possibility that she had initiated a line of reasoning intended to yield information about any penguins drawn from some range and I intended, in that case, for my expression "these penguins of yours" to have the very range of possible values contemplated by Emily. If I am fooling myself, if I am really using the phrase "these penguins of yours" in some other way, then, once again, my idiolect is wonderfully suited to produce miscommunication. Since my idiolect seems generally not to suffer from such impairments, I am going to suppose that I can let other speakers determine what I am talking about even when this means determining a range of possible values for a variable-like expression. I am going to suppose that my idiolect is porous in this broader sense. If our penguin example seems contrived, consider the following. If Emily makes a quantificational claim and I attribute truth to it, then I am affirming exactly what Emily affirmed and, hence, Emily and I are asserting something about the same quantificational domains. So my mastery of a truth predicate is enough to guarantee my idiolect a substantial degree of porosity.

Just one more observation about how broad our notion of porosity is going to be. When Emily says, "Consider any emperor penguins less than two years old," she is probably initiating a line of reasoning applicable to any such penguin *taken* *individually*. She could reach a conclusion of the form, "If Dana is such a penguin, then Dana is so-and-so." On the other hand, when Emily says, "Consider any emperor penguins who share a common parent," her use of the plural carries more weight. She has probably initiated a line of reasoning applicable only to certain penguins taken several at a time. She is pointing toward conclusions of the form, "If Dana and Meredith are emperor penguins who share a common parent, then they are so-and-so." It seems unlikely that she is pointing toward any conclusion of the form, "If Dana is an emperor penguin who shares a common parent, then Dana is so-and-so." We are going to suppose that "aboutness" is freely transferable even when the lender is employing variable-like expressions that are logically, not just grammatically, plural. If Emily has initiated a line of reasoning intended to yield information about any penguins drawn from some range, then I can join in that reasoning even if the penguins are being "drawn" more than one at a time.

This sort of plural reasoning is not unknown in mathematics. In his number theory text, Davenport (1983, 25) offers the following definition: "Several numbers are said to be *relatively prime* if there is no number greater than 1 that divides all of them." He immediately concludes that if several numbers are relatively prime, then "there shall be no one prime occurring in all the numbers." The logic here is plural: Davenport is reasoning about numbers *taken several at a time*. Now imagine Emily has initiated a line of reasoning applicable to any relatively prime numbers. I join in late and, wishing to get up to speed, ask, "Can we take the product of your numbers?" My question is about something. Emily is reasoning about something. My question and Emily's reasoning are, in fact, about the same thing. How does it come to pass that they are about the same thing? In my effort to enter into Emily's reasoning I borrow aboutness from her. I am not prevented from doing so by the plural character of Emily's reasoning.

Let me be clear about the argument here. The situations we have considered seem to have no special features that would thwart my ardent desire to let Emily determine what I am discussing. Furthermore, these situations seem fairly commonplace. A modest conclusion would be that my desire can be satisfied in circumstances that are likely to occur. A bit weak, that. We can do a little better. Letting other people determine what I am discussing *seems* like the sort of thing I can do just by intending to do it. That could be wrong, but I *act* as if it is right. I routinely act on the assumption that I am doing just what I intend to do when I intend to let others determine what I am discussing. I act on this assumption even when I have little evidence for it other than my own intention. If my intention were *routinely* unfulfilled, I would be engaged in a communicative practice with a feature that seems profoundly maladaptive, one that would lead my interlocutors and me to believe I am discussing one thing when I am actually discussing another (if I am even discussing anything at all). One would expect such a feature to have made itself known to me by having frequently brought me to grief. This has not been my experience. So it seems likely that my desire to let others determine what I am discussing is not routinely thwarted: my idiolect is, under normal circumstances, porous; it allows me to borrow aboutness from other speakers.

What about abnormal circumstances? I take the forensic situation to be the following. Suppose someone insists that, in a particular circumstance or class of circumstances, my aforementioned desire is, in spite of my sincere efforts, thwarted. That person owes us, first of all, evidence that such a thing has occurred. If I perform the appropriate behaviors with the appropriate intentions, why should I believe my intentions are unfulfilled? Furthermore, it seems to me, we deserve an account of how such a thing could have happened. What special features conspired to thwart a desire that, we believe, is generally satisfied? I do not claim such questions are unanswerable. I do propose that the sceptic bears the burden of answering them.

Parsons (2001) suggests an interesting approach to the first question, the question of evidence. Again suppose I overhear Emily discussing some penguins. Let us suppose there really are some penguins she is discussing. (She does not just *appear* to be discussing some penguins.) I say, "Tell me more about these penguins of yours." We have a little talk in the course of which Emily says, "These penguins do so-and-so." Suppose I believe that the penguins I am discussing do no such thing. Charity dictates that I interpret Emily's claim in a way that gives it the best chance of being true. So the principle of charity gives me some reason to suppose that Emily and I are discussing different penguins.

That seems correct. In some circumstances, the principle of charity can supply a defeasible reason for believing in a failure of porosity. But such a failure is not the only hypothesis that gives Emily's statement a fighting chance at truth. Perhaps Emily and I understand "do so-and-so" differently. Perhaps Emily and I are discussing the same penguins, but I have acquired some misconception about which penguins they are. Perhaps I initially succeeded in discussing the same penguins as Emily, but one or both of us then began to discuss other penguins. Perhaps I need to rethink my own beliefs about penguins. Since my faith in the porosity of my idiolect is well-founded, it seems bad strategy to admit an exception when other plausible options are available.

Furthermore, in some cases, failure of porosity will not be a hypothesis worth entertaining. Suppose Emily says, "The chief predator of these penguins is the polar bear." I have very good reasons for believing there are no such penguins. Unless these reasons are defeated, I should refrain from supposing that Emily and I are discussing different penguins, mine lacking the indicated property, hers possessing it. In such a case, the principle of charity should not lead me to believe that my idiolect has been uncharacteristically impermeable. A better-supported hypothesis would be that Emily has said something false.

2 Natural numbers

Though sceptical interventions are always welcome, I am going to suppose for now that Emily and I both speak idiolects that are porous and that each of us recognizes that the other speaks such an idiolect. I will try to indicate clearly each point where a failure of porosity would cripple the argument. At the very least, then, the following will provide an analysis of the logical relationship between the porosity assumption and the conclusions about mathematical determinacy.

Emily and I are going to explore how porosity might contribute to our efforts at mathematical communication. Let us start with arithmetic. Both of us believe there are objects and functions with the properties normally attributed to the natural numbers and the successor operation. Wishing to initiate a line of reasoning about models of arithmetic, Emily says, "Consider any objects and any function on those objects satisfying the principles of Dedekind/Peano arithmetic." She introduces a predicate 'N' that applies to all and only the number-like objects under consideration. Of course, there are no *particular* objects under consideration. Emily is using 'N' to reason about a range of possible values. I can also use 'N' to reason about a range of possible values and I can let Emily determine that range. I do so. (If I intend to do so, but fail, then the argument will not work. Our first invocation of porosity.) Emily also introduces a function symbol 'S' that will range over all the functions she is now contemplating. I can use 'S' to reason about a range of possible values and I can let Emily determine that range. I do so. (Porosity again.) Emily lets '0' denote the zero element with respect to S, as do I. (I could invoke porosity here or could describe 0 using 'N' and 'S'.)

I am assuming, by the way, that arithmetic *as understood by Emily* has at least one model. I am assuming that she is reasoning about something when she reasons about an arbitrary model $\langle N, S\rangle$. I allow myself this assumption because (1) we affirm number theoretic principles of the same form; (2) I believe arithmetic has models; and (3) Emily has given no evidence that she understands arithmetic in a way that would render it incoherent.

I now imitate Emily, calling upon her to consider any objects and any function with the Dedekind/Peano properties and introducing new vocabulary 'N'', 'S'', and '$0'$'. Emily decides she will follow me in using 'N'' and 'S'' to reason about ranges of possible values, letting me determine those ranges. (Porosity twice more.) She also follows me in my treatment of '$0'$'. Emily is assuming that arithmetic *as understood by me* has at least one model and that, within the line of reasoning I have initiated, the principles of arithmetic are true when we read "number," "successor," and "zero" as N', S', and $0'$ (or, if not literally true, since 'N'', 'S'', and '$0'$' do not pick out any particular model, then at least permissible links in a chain of reasoning that eventually yields truths). In particular, she believes each instance of my induction principle is true (or permissible in this discourse) when interpreted in this way.

The porosity of both our idiolects allows us to assert the same thing when we affirm an instance of induction. Porosity allows us to assert what the other person asserts. It does not justify that assertion. That justification comes from elsewhere. Emily will affirm instances of my induction principle for the three reasons already enumerated: (1) we affirm number theoretic principles of the same form; (2) Emily believes arithmetic has models; and (3) I have given no evidence that I understand arithmetic in a way that would render it incoherent. These three considerations lead Emily to believe that I am reasoning about something when I reason about an arbitrary model $\langle N', S'\rangle$. She chooses to join me in this reasoning.

Emily can now transcribe, word for word, the version of Dedekind's categoricity proof she finds in Parsons (1990).[1] Emily uses recursion to introduce a function f that assigns objects satisfying 'N'' to objects satisfying 'N'.

[1] See also Shapiro (1991, 210–219) and Lavine (1994, 224–240). For different approaches, see Field (2001, 338–342) and Halbach and Horsten (2005). Lavine, Parsons, and Shapiro offer insightful discussions of one form of generality with which a number theoretic induction scheme might be endowed: mathematicians might freely endorse new instances of the induction scheme when they adopt new mathematical vocabulary. Their understanding of the scheme might be inclusive with respect to expansions of their mathematical language. I have no quarrel with this reasonable observation. I do want to emphasize, though, that we can exploit another person's linguistic resources without expanding our idiolect. If Emily has singled out some penguins, I can refer to those very penguins even if Emily has employed vocabulary that remains foreign to me. It is her successful act of reference, however accomplished, that puts me in a position to refer likewise. Similarly, if Emily is reasoning about any penguins drawn from some range, I can reason about them too without expanding my idiolect. This means my treatment of my induction scheme can endow that scheme with another form of generality: I might freely affirm instances of the scheme even in contexts where I am letting Emily determine (at least in part) what I am discussing. My understanding of the scheme might be inclusive with respect to the aboutness of other people's discourse. Emily and I typically borrow aboutness from one another by talking about one another's talk. If I do such borrowing in the course of number theoretic reasoning, I may find myself entertaining instances of induction in which I advance claims about "Emily's numbers"

$$f(0) = 0'$$
$$f(S(x)) = S'(f(x))$$

Since her own N, S, and 0 satisfy induction, Emily can show that f is one–one. She wants to show: $\forall y(N'y \rightarrow \exists x(Nx \wedge f(x) = y))$. She can do so, to her own satisfaction, because she believes the following instance of induction is true (or permissible).

$$(\exists x(Nx \wedge f(x) = 0') \wedge \forall y(N'y \rightarrow (\exists x(Nx \wedge f(x) = y) \rightarrow \exists x(Nx \wedge f(x) = S'(y))))) \rightarrow \forall y(N'y \rightarrow \exists x(Nx \wedge f(x) = y))$$

So f is a pairing. Indeed, it is an isomorphism. (Treating S and S' as two-place relations, we have: $S(xy)$ if and only if $S'(f(x)f(y))$.) Emily concludes: arithmetic-as-she-understands-it has the same models as arithmetic-as-I-understand-it and all these models are isomorphic. I am in a position to reach the same conclusion. We conclude that we understand arithmetic in the same way and that our shared concept of natural number is as determinate as one could hope.

We pause to consider two sceptical challenges. First, a paraphrase of an objection advanced by Field (2001, 358) against a slightly different argument. (Field's target is Parsons (1990). For Parsons' own response, see Parsons (2001).) It was essential to the above argument that I have an inclusive understanding of induction, an understanding that allows me to affirm instances containing vocabulary I have acquired through interactions with other people. The instance cited in the preceding paragraph features the predicate '*N*' whose extension was determined by Emily and the function symbol '*f*' that Emily introduced by recursion. Field could now respond as follows.

> Even if Emily recognizes that her father subscribes to an inclusive induction principle, that only means that she views her father as committed to accepting all new instances *in her father's own language*. Unless Emily can argue that for any predicate p in *her* language, her father can expand *his* language to include a term that she should translate as p, there is no reason to assume that Emily should view her father's induction principle as inclusive with respect to *her* language. And there is no way to argue this without begging the question.

Well, consider any predicate Emily has introduced into her language by stipulating its extension or by indicating a range of possible values from which its extension might be drawn. ('*N*' was introduced in the latter way.) I can introduce any such predicate into my idiolect by letting Emily determine either its extension or a range of possible values. Of course, it would then be quite proper for Emily to translate this predicate back into her language homophonically.

One might insist that Emily has no good reason to believe my idiolect is porous and, so, has no good reason to believe the homophonic translation is reliable. But that seems wrong. Emily will believe my idiolect is porous for essentially the same reason she believes her own idiolect is porous. I routinely engage in behaviors (asking about her penguins and so on) that would be profoundly misleading and disruptive of communication if my idiolect were not porous. The more evidence she has that our efforts at communication are successful, the more reason she has to believe my

Footnote 1 continued
or "the numbers Emily is discussing right now." I may find myself entertaining instances containing vocabulary that is not purely number theoretic. This may strike some readers as odd, particularly if they think of number theory as a family of formalizations rather than a human activity.

idiolect is porous. Now a sceptic might concede that Emily has good reason to treat my idiolect as porous under normal circumstances, but still insist that special features of the case now under discussion render my idiolect impermeable with respect to 'N' and 'f'. Maybe there are such features. I do not claim this is impossible. The important point is that I am under no obligation to show it is impossible. Let the sceptics convince us that something has gone wrong and help us to understand how this could have occurred. Until they do so, it is entirely reasonable for us to suppose that our idiolects have displayed their usual permeability.

To make the Dedekind/Parsons argument work, we must also show that Emily and I understand the function symbol 'S' in the same way (since 'S' appears in the definition of 'f' and 'f' appears in the crucial instance of induction). But this is no problem. If Emily can use 'S' to reason about any items in a certain range, then I can use it to reason about those very items. Since Emily has evidence that I am doing just that, since she has evidence that my idiolect is porous and that I am taking advantage of that porosity at this very point, she has evidence that her homophonic translation of 'S' is reliable. She might be wrong. Her reasons are defeasible. She is ready to consider contrary evidence. But she cannot consider it when it has not been presented.

That was the first sceptical challenge. Now for the second. Let us grant, says the second sceptic, that all the models of arithmetic Emily and her father are able to contemplate are isomorphic to one another. This does not mean they have characterized the natural numbers categorically. Perhaps their models are all isomorphic because they are only able to contemplate models falling within some limited range. The models they neglect will not necessarily be isomorphic to the models they contemplate.

In response, Emily and I ask the sceptic whether he has, just now, been talking about the neglected models, if indeed they exist. Well, yes, if they exist, he has been talking about them. Then we too can talk about them, can apply the Dedekind/Parsons argument to them, and can prove they are isomorphic to our other models of arithmetic. (Consider any objects and any function on those objects that constitute a neglected model. Now argue as before.) The proof does not make the models isomorphic. Our conversation with the sceptic does not make them isomorphic. They were isomorphic all along. If we had never conversed with the sceptic or even known anything about the sceptic, they still would have been isomorphic. At most, what seems to be required is that it be possible for us to exploit the sceptic's ability to talk about various things. So any model that any possible conversational partner is able to discuss will be isomorphic to our models. When we appear to be discussing non-isomorphic models, we are talking about nothing. We might just as well spend our time contemplating the grains of sand that will never be contemplated. (This may be the same conclusion expressed in Parsons (1990) and (2001). That it is hard to tell is a mark of how complicated this issue is.)

3 Sets of natural numbers

Emily and I now feel justified in reasoning about *the* natural numbers (or, more briefly, "the numbers") and *the* structure they form. We do not pretend to have singled out any particular structured items. Making assertions about "the numbers" is just a convenient way to talk and reason about models of arithmetic in general. We are now going to explore the truth conditions of the following assertion.

Any numbers form a set.

We call this assertion "COM(ω)" since it applies the combinatorial conception of set to the members of ω.[2] We say that numbers form a set just in case there is a set whose members are exactly *them*. For example, 27, 12, and 158 form a set just in case there is a set whose members are exactly 27, 12, and 158. More formally, numbers form a set if and only if

$$(\exists x)(y)(y \in x \leftrightarrow y \text{ is one of those numbers}).$$

Emily and I are thoroughgoing structuralists. At the moment, we do not attribute any meaning to '$\in$' beyond what is implicit in COM(ω). We are interested in any possible world whose numbers behave like numbers and in which '$\in$' expresses a relation satisfying COM(ω). Given any numbers at all and any possible world they inhabit, Emily and I agree on how that world would have to be structured in order for

$$(\exists x)(y)(y \in x \leftrightarrow y \text{ is one of those numbers})$$

to be true there. That is, we agree on how the first-order logical vocabulary contributes to the determination of truth conditions. The question now is whether we agree on how a possible world must be structured in order to satisfy COM(ω). In particular, do we agree on which numbers would form sets in a world satisfying COM(ω)? The following facts about our idiolects will be important.

In Emily's idiolect, the phrase "any numbers" obeys the principle of universal generalization. If, in Emily's idiolect, "These numbers do so-and-so," follows from premises containing no information that would distinguish the numbers in question from any other numbers, then, "Any numbers will do so-and-so," follows from those same premises.

In my idiolect, the phrase "any numbers" obeys the principle of universal instantiation. From the statement, "Any numbers will do so-and-so," I can infer, "These numbers do so-and-so," no matter what particular numbers or range of possible values I might be discussing.

Suppose Emily and I agree on what it means for numbers to "do so-and-so." Then, in a context where "these numbers" refers to the same numbers or has the same range of values in her idiolect and mine, the proposition, "These numbers do so-and-so," will be assertable in my idiolect if and only if it is in hers. This allows us to argue as follows.[3]

[2] Aficionados will now recognize that this paper is a contribution to the literature on "plural quantification and set theory." The prevailing view seems to be that serious discussion of this issue began with Boolos (1984). One reason Boolos' contribution so influenced me was that my mind was well prepared. I had already been thinking about the topic for a year or two thanks to some neglected papers: Black (1971), Stenius (1974), and, most importantly, Simons (1982). In much of the subsequent literature, philosophers get down to the business of using plural quantifiers without too much meta-talk about them. Some examples: Boolos (1985, 1989); Burgess (2004); Hellman (1994, 1996, 2003); Lewis (1991, 1993); Pollard (1986, 1988a, 1988b, 1992, 1996, 1997). See also Cartwright (1993) and Uzquiano (2003). For examples of critical responses, see Resnik (1988), Hazen (1993), Linnebo (2003), and Jané (2005). Lavine (1994) offers an alternative to our COM(ω): namely, the scheme "The **F** numbers form a set" where it is understood that we can replace the place-holder **F** with any predicate first-order definable in some expansion of our mathematical vocabulary. In place of plural quantification over numbers, Prof. Lavine offers singular quantification over expansions of our language. I find the former clearer than the latter. (This is just a report about my own cognitive state. I do not know how to show that one is clearer than the other *simpliciter*.)

[3] What follows is, very roughly, what I took McGee to be saying in his presentation at the Boolos Memorial Symposium in 1998. See McGee (2001) and also (1997). Prof. McGee has graciously

- Premise: "Any numbers will do so-and-so" is true in my idiolect.
- Emily says, "Consider any numbers."
- I join in the conversation, letting "these numbers" have the range of possible values determined by Emily. (Porosity again!)
- Then (speaking my idiolect in the indicated context): These numbers do so-and-so.
- So (speaking Emily's idiolect in the indicated context): These numbers do so-and-so.
- Our premise carries no special information about these numbers.
- So (speaking Emily's idiolect): Any numbers will do so-and-so.
- So: "Any numbers will do so-and-so" is true in Emily's idiolect.

If Emily and I agree on what it means for numbers to "do so-and-so," then the assertion, "Any numbers will do so-and-so," will be true in my idiolect only if it is true in Emily's.

In case that went by too quickly, let us run through the argument again from Emily's perspective.

> "Any numbers will do so-and-so" is true in my father's idiolect. I am interested in properties that will apply to numbers no matter what those numbers might be. In order to initiate a line of reasoning that might identify such properties I propose to my father that we consider "any numbers at all." Since my father's idiolect is porous, he can use a phrase like "Emily's numbers" as a kind of variable whose range of possible values has been determined by me (in this case, by my understanding of the phrase "any numbers"). I have observed that the phrase "any numbers" obeys universal instantiation in my father's idiolect. Indeed, he has announced that this is part of his understanding of how the term is to be used. So, in his idiolect and in the current context, he can legitimately infer, "Emily's numbers do so-and-so." My father and I have determined that we mean the same thing by "do so-and-so." So, since my father is using the phrase "Emily's numbers" to talk about the numbers I am now contemplating, I can infer that the numbers I am now contemplating do so-and-so. But I am not contemplating any particular numbers. I am in the middle of a line of reasoning that will apply to any numbers at all. So, "Any numbers will do so-and-so" is true in my idiolect. My father's endorsement of the inference from "Any numbers will do so-and-so" to "Emily's numbers do so-and-so" tells us something important about his understanding of the quantifier "any numbers." His quantifier cannot be more restricted than mine because he is open to any range of possible values that I am able to contemplate.

I repeat: if Emily and I agree on what it means for numbers to "do so-and-so," then the assertion, "Any numbers will do so-and-so," will be true in my idiolect only if it is true in Emily's.

Now suppose the phrase "any numbers" obeys universal generalization in my idiolect. Suppose the phrase "any numbers" obeys universal instantiation in Emily's idiolect. Repeating the above argument yields our grand conclusion.

Footnote 3 continued
observed that the formulation below is not just a rehash of his own argument and may accomplish substantially more. I understand myself to be offering an interpretation, elaboration, and defense of McGee's position.

(†) *If Emily and I agree on what it means for numbers to "do so-and-so," then the assertion, "Any numbers will do so-and-so," will be true in my idiolect if and only if it is true in Emily's.*

Suppose, quite plausibly, that Emily's understanding of "any numbers" entails that this phrase will obey universal generalization and instantiation in her idiolect. Suppose, quite plausibly, that my understanding of "any numbers" entails that this phrase will obey universal generalization and instantiation in my idiolect. Then (†) is true by virtue of the porosity of our idiolects and the meaning of the phrase "any numbers" in our idiolects. If our idiolects assign non-equivalent truth conditions to some assertion, this cannot be because of some disagreement we have about the meaning of "any numbers."

Emily and I agree on what it would mean for some numbers to form a set in a possible world. That is, given any numbers, we agree on what '$(\exists x)(y)(y \in x \leftrightarrow y$ is one of those numbers)' says about the $\in$-structure of a world inhabited by those numbers. So we agree on how a possible world inhabited by the natural numbers would have to be structured in order for COM(ω) to be true there (because we agree on which numbers would form sets there). So Emily and I understand arithmetic-plus-COM(ω) in the same way.

Suppose someone now suggests that our theory or, better, our theory plus an extensionality axiom might have models with the same numbers but different sets of numbers (in the non-trivial sense that some numbers form a set in one model but not another). The suggestion is easily dismissed. If some numbers form a set in one model but not another, then the latter model will not satisfy the principle that any numbers form a set. So any models contemplated by our critic will agree on which numbers cooperate to form sets. Indeed, it should be evident to anyone who shares our understanding of "any numbers" that arithmetic-plus-COM(ω)-plus-extensionality is structurally determinate. COM(ω) determines what extensions our sets of numbers will have and the extensionality axiom guarantees that only one set will have a given extension. So the $\in$-structure is fully determined for the sets of numbers (though this structure might be embedded inside of a larger set theoretic structure).

4 Isomorphisms

I think we understand our talk about "structural determinacy" perfectly well. But suppose someone is more comfortable talking about isomorphisms. Can Emily and I show that the models of arithmetic-plus-COM(ω)-plus-extensionality are all isomorphic (in the usual sense of there being an isomorphism between any two models)? Before we try to answer this question, let us be clear about something. Emily and I will not be constructing isomorphisms in order to show that we understand the theory in question in mathematically indistinguishable ways. We already know that we understand the theory in the *same* way. By reflecting on the capacity each of us has to exploit the linguistic resources of the other, we have verified that we attribute the same truth conditions to arithmetic-plus-COM(ω)-plus-extensionality. (I have felt free to include extensionality here because it is a sentence in the language of first-order set theory and I am taking for granted that we agree on how that vocabulary contributes to the determination of truth conditions.) At the moment, we are trying to confirm that our shared understanding determines the theory's models up to isomorphism. We have already established that any models a sceptic might entertain will agree structurally. We are now trying to confirm that this structural agreement entails the existence of a function witnessing to that agreement.

Emily and I introduce two binary relation symbols 'η' and 'η''. We are going to act as if these symbols have particular meanings, though we are really just using them as place-holders to reason in general about situations in which the axioms of our theory come out true. Now consider any objects, hereafter known as "the *M*s," that make both COM(ω) and our extensionality axiom true when their number quantifiers are taken to range over the natural numbers, their set quantifiers are taken to range over the *M*s, and '$\in$' is taken to express what 'η' expresses. More briefly: $\langle M, \eta \rangle$ satisfies COM(ω)-plus-extensionality (or, at least, does so when supplemented by the standard model of arithmetic lurking in the background). Consider also some objects, hereafter "the *M*'s," that make both COM(ω) and our extensionality axiom true when their number quantifiers are taken to range over the natural numbers, their set quantifiers are taken to range over the *M*'s, and '$\in$' is taken to express what 'η'' expresses. More briefly: $\langle M', \eta' \rangle$ satisfies COM(ω)-plus-extensionality.

Now let A be any one of the *M*s. We will associate A with the M's whose η'-members are exactly the η-members of A. Let G be the relation that assigns M's to *M*s in this way. That is,

$$\forall x, y(Gxy \leftrightarrow (Mx \wedge M'y \wedge \forall z(z\eta x \leftrightarrow z\eta' y))).$$

Extend G to the natural numbers by saying: Gmn if and only if $m = n$. Is G an isomorphism? G is one–one because $\langle M, \eta \rangle$ satisfies our extensionality axiom. G is a function because $\langle M', \eta' \rangle$ satisfies our extensionality axiom. Each of the M's is assigned by G to an M because $\langle M, \eta \rangle$ satisfies COM(ω). G assigns each of the *M*s an M' because $\langle M', \eta' \rangle$ satisfies COM(ω). Say that $g(a) = b$ if and only if Gab. Then, if n is a number, $n\eta x$ if and only if $g(n)\eta' g(x)$. So G is an isomorphism, as desired.

But what exactly have we shown? A sceptic might inquire about our place-holders 'η ' and 'η''. What range of values do we have in mind here? If we are reasoning about, say, predicates translatable into our idiolects and there are predicates not so translatable or relations not expressible by any translatable predicate, one might complain that our argument lacks full generality. We could try to avoid such complaints by dropping the "place-holder" business and explicitly quantifying over "all binary relations between the natural numbers and existing things." Unfortunately, I am not sure what this means. And even if I could convince myself that I attach a definite meaning to such a quantifier, it is far from clear that this would put me in a position to show that others attach the same meaning. We could try running through our argument from Sect. 3, replacing "any numbers" with "any relations." But we ventured that earlier argument only after having established that Emily and I understand "numbers" in the same way. I do not know how to argue that Emily and I have such a shared understanding of "relations."

In our version of the Dedekind/Parsons argument, I did not hesitate to quantify over number theoretic functions. That seemed innocent because such quantification is a regular part of ordinary mathematical discourse. I was taking for granted that number theoretic talk is coherent and intelligible. The question was how determinate it is. It seems too much of a stretch, however, to insist that quantification over all possible membership relations is a staple of mathematical conversation.

The idea of some numbers not forming a set in a model of COM(ω) is plainly incoherent. The idea of models lying outside the scope of our isomorphism argument is not. We might have to concede, then, that the language of our isomorphism argument does not allow us to express the claim we wish to make about the structural determinacy of arithmetic-plus-COM(ω)-plus-extensionality. That is not a big problem. Emily and I were already convinced that this theory fully characterizes the $\in$-structure of the sets of numbers. The theory can do so because COM(ω) succeeds in telling us which numbers form sets. Which numbers? Any numbers! Perhaps someone will complain that this formulation is not fully general. Perhaps, says a sceptic, your quantifier "any numbers" is restricted in some way of which you are unaware. Restricted? Well, yes, perhaps some numbers fall outside the range of your quantifier. Some numbers, eh? The sceptic is claiming to have a plural numerical quantifier less restricted than ours. But, as we have already seen, this is not so. If the sceptic can reason about some numbers, so can we. But in reasoning about them, we can make all the more clear that our quantifier "any numbers" suffers from no restrictions that anyone can discuss coherently.

5 Higher types

Emily and I have confirmed that we assign the same truth conditions to the theory arithmetic-plus-COM(ω)-plus-extensionality. We would like to say that any models of this theory will be isomorphic. But it is not clear that we can give the quantifier "any models" the generality we desire. Nonetheless, we think we have shown that our theory is structurally determinate. At the very least, we are convinced that there is no way to entertain coherently the idea of non-isomorphic models. So we feel justified in reasoning about *the* natural numbers, *the* set formed by any such numbers, any of *these* sets, and *the* structure *these* numbers and sets form. We do not pretend to have singled out any particular structured items. Making assertions about "the natural numbers and the sets thereof" is just a convenient way to explore the consequences of arithmetic-plus-COM(ω)-plus-extensionality and its extensions.

We are now going to investigate the truth conditions of a new set-formation principle, COM($\wp(\omega)$):

Any sets of numbers form a set.

We can adapt the argument from Sect. 3 to show the following.

(††) *If Emily and I agree on what it means for sets of numbers to "do so-and-so," then the assertion, "Any sets of numbers will do so-and-so," will be true in my idiolect if and only if it is true in Emily's.*

As before, we conclude that Emily and I attach the same truth conditions to arithmetic-plus-COM(ω)-plus-COM($\wp(\omega)$)-plus-extensionality. Furthermore, we can argue that this theory fully determines the $\in$-structure of the sets of numbers and the sets of sets of numbers. So we are ready to move on to COM($\wp(\wp(\omega))$) adapting our earlier arguments to show that Emily and I understand arithmetic-plus-COM(ω)-plus-COM($\wp(\omega)$)-plus-COM($\wp(\wp(\omega))$)-plus-extensionality in the same way and that this theory fully determines the $\in$-structure of the sets of numbers, the sets of sets of numbers, and the sets of sets of sets of numbers.

Since our theory fixes the $\in$-structure, it will decide statements about the contents of $\omega \cup \wp(\omega) \cup \wp(\wp(\omega)) \cup \wp(\wp(\wp(\omega)))$. That is, if such a statement is not a consequence of our theory, its negation will be. If we use sets of natural numbers to represent real numbers and sets of sets of sets of natural numbers to represent ordered pairs of reals, then we can use our theory to reason about real valued functions. So our theory will decide statements such as Cantor's continuum hypothesis.[4] (The continuum hypothesis or "CH" says that infinite sets of real numbers come in only two sizes: if you pick infinitely many reals, you either have as many reals as there are natural numbers or you have as many reals as there are reals.)

Suppose Emily and I have disagreed all along about CH, one of us believing it to be true, the other believing it false. This will not have interfered with any of the arguments rehearsed above. It would mean, however, that one of us has a notion of real-valued function inconsistent with our core concept of set. (Of course, we do not know which of us is the guilty party.) I do not see how this consideration makes the above argument any less interesting. Parsons, however, insists that this sort of disagreement among set theorists seriously limits the reach and interest of that argument.

Parsons (2001) asks us to consider two set theorists, Tony and Ron, with conflicting conceptions of the set theoretic universe.

> Suppose Tony accepts some large cardinal axiom, let's say that there is a measurable cardinal (MC), and Ron accepts some axiom that conflicts with it, let's say $V = L$. It is natural in such a case to apply the principle of charity and assume that each should be interpreted so that his favorite axiom comes out true. But then of course their "intended models" cannot be isomorphic.

We make three additional assumptions. (1) Tony and Ron have versions of arithmetic whose models are all isomorphic. (2) They both endorse COM(ω). (3) They agree on the meaning of the predicate "is constructible." ($V = L$ asserts that every set is constructible in a sense definable in the language of first-order set theory.) Now charity leads us to believe that Tony can assert truly that some numbers form a non-constructible set (since this follows from MC). Charity leads us to believe that Ron can assert truly that no numbers form a non-constructible set (since that follows from $V = L$). This leaves us three possibilities. First, Ron's idiolect suffers from a mysterious failure of porosity. Second, Ron's numerical quantifier "any numbers" does not obey the principle of universal instantiation. Third, Tony's numerical quantifier "any numbers" does not obey the principle of universal generalization. If none of these three obtained, Tony and Ron would be able to argue as follows.

- Premise: "Any numbers will form a constructible set" is true in Ron's idiolect.
- Tony says, "Consider any numbers" and introduces the phrase "these numbers" as a device for reaching conclusions applicable to any numbers at all.[5]

[4] The undecidability of CH in large cardinal extensions of ZFC (the canonical first-order set theory) has led some authors to argue that standard ways of thinking about sets do not decide CH. See, for example, Robinson (1965) and Cohen (1971). For some responses and elaborations, see Kreisel (1969) and (1971), Weston (1976), Giaquinto (1983), and Pollard (1990). For more recent contributions, see Feferman (1999) and (2000), Martin (2001), Woodin (2001), and Hauser (2002). The undecidability results imply, of course, that our COM principles are not firstorderizable. Though our set theoretic principles decide CH, we do not know whether it is CH itself or its negation that follows from those principles. A pessimist might insist that we are unlikely ever to sort this out: we are unlikely ever to reach a rational decision about the truth or falsity of CH. On that issue, I have nothing of importance to contribute.

[5] Suppose, instead, that Tony introduces a description that purports to refer to some particular numbers: say, "God's favorite example of numbers that form a non-constructible set." If Ron had reason to believe there are no such numbers, he could reasonably decline to enter into Tony's reasoning.

- Ron joins in the conversation and, taking advantage of the porosity of his idiolect, lets "these numbers" have the range of possible values determined by Tony.
- Then (speaking Ron's idiolect in the indicated context and applying universal instantiation to the initial premise): These numbers form a constructible set.
- Tony and Ron mean the same thing by "form a constructible set" and, in this context, they are using "these numbers" to reason about the same range of possible values. So (speaking Tony's idiolect in the indicated context): These numbers form a constructible set.
- Tony is using the phrase "these numbers" as a variable-like expression in the course of some reasoning that applies to any numbers at all. So (speaking Tony's idiolect and applying universal generalization): Any numbers will form a constructible set.
- So: "Any numbers will form a constructible set" is true in Tony's idiolect.

Clearly, something has gone wrong, since we are assuming that "Some numbers form a non-constructible set" is true in Tony's idiolect. So let us consider the three other possibilities we just mentioned. In the first case, Ron is somehow prevented from letting Tony determine what he is discussing when he employs the phrase "these numbers" in the above discourse. It is quite obscure why Ron would be impaired in this way. In the second case, Ron is able to let Tony determine what he is discussing, but he resists the move from "Any numbers will form a constructible set" to "These numbers form a constructible set." One might then question whether Ron really understands what "any" means. In the third case, Tony resists the inference from "These numbers form a constructible set" to "Any numbers will form a constructible set." Yet Tony himself has initiated a line of reasoning in which conclusions about "these numbers" will be applicable to any numbers at all or, at least, will be so applicable as long as no step in the discourse relies on a premise attributing special distinguishing features to "these numbers." Since the argument nowhere relies on such a premise, Tony's resistance might well lead us to think that *he* does not really understand what "any" means. In the first case, Ron is seriously impaired; in the second, he is greatly confused; in the third, it is Tony who is greatly confused. None of these alternatives will be plausible if Ron and Tony have shown themselves to be competent speakers of English. It is far more plausible that one of them has an incoherent conception of the set theoretic universe. This does not require that either of them be greatly impaired or confused. Thinkers as profound and clear-sighted as Frege have endorsed inconsistent set theories.

Parsons observes, quite correctly, that we will violate the principle of charity if we insist that Tony and Ron are committed to the same sets of natural numbers. That is not the end of the story, however. Applications of the principle of charity are always defeasible. In the present case, our applications of charity force us to conclude, quite implausibly, that either Tony or Ron suffers from a serious lapse in logical or linguistic competence. This gives us good reason to believe that charity has been misapplied here. No need to despair: there is an entirely plausible alternative. On the most reasonable interpretation of what Tony and Ron are saying, one of them is saying something

Footnote 5 continued
In our example, however, Tony does not claim to be referring to any numbers in particular. He is inviting Ron to join him in a line of reasoning applicable to any numbers at all and is announcing his intention to use the expression "these numbers" in the service of this project. We welcome sceptical counter-arguments; but, while waiting for them to appear, we are supposing that Ron can discuss exactly what Tony is discussing as long as Tony is discussing something. In this case, I see no reason for Ron to doubt that Tony is discussing something.

incoherent. They share a core concept of set that appears to be coherent; but *MC* and $V = L$ do not both provide a consistent elaboration of that core concept.

6 I Mean *any*

Emily and I have declared allegiance to an unrestricted, combinatorial conception of set formation by affirming our various COM principles. COM(ω), for example, makes it quite clear which natural numbers will cooperate to form sets: any of them! If someone challenged us to explain what we mean by “any,” then, as Feferman (2000, 410–411) has observed, we would face a dilemma. Suppose we expand upon COM(ω). “Any numbers form a set; and by ‘any numbers’ I mean _____.” We then risk *qualifying* COM(ω) in some way. “Any _____ numbers form a set.” But this is contrary to the combinatorial spirit. The combinatorialist means “any whatsoever” or “any *without qualification*.” On the other hand, if we are not going to expand on COM(ω) and the other COM principles, what *are* we to do? We cannot say anything with *fewer* qualifications than plain old “any.” We could pronounce the words “any whatsoever” in an especially earnest way. But if this is all we do, we are not doing a very good job of clarifying our meaning. If this is the best we can do, our listeners would be wise to withhold judgment about what we mean.

As we have already seen, we can do better. Consider any mathematician whose version of number theory has models all isomorphic to ours and whose idiolect has the following characteristics: (1) it is porous in the ways we have discussed; (2) its plural universal quantifiers satisfy universal generalization and instantiation; and (3) its sentential connectives and singular quantifiers are classical. Any such mathematicians can show quite clearly what they mean by “any number.” They do not have to perform the impossible task of showing that they understand “any” in an unqualified way by qualifying it in various ways. They do not have to *talk about* “any” at all; they just have to *use it* in perfectly standard ways, ways exemplified by my interactions with Emily back in Sect. 3. When such mathematicians endorse COM(ω), they signal clearly that their concept “set of numbers” is determinate in every mathematically important way.

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