Charles Parsons
Mathematical Thought and Its Objects

Peter Smith

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This is a section-by-section discussion of Charles Parsons's eagerly awaited *Mathematical Thought and Its Objects* (CUP, 2008: pp. xx + 378)[1] – though not every section gets the same level of attention. For convenience, the sectioning here corresponds to the chapter/section divisions of the book.

Parsons himself says that his book has been a very long time in the writing. Its chapters extensively "draw on", "incorporate material from", "overlap considerably with", or "are expanded versions of" papers published over the last twenty-five or so years. What we are reading is a multi-layered text with different passages added at different times.[2] This does make for a bumpy read, with the to-and-fro of argument not always ideally well signalled. The prose style can make for hard going too. So I sometimes am not too confident that I am reading Parsons aright. But let's dive in ...

1 Objects and Logic

§§1–4 Logic and the notion of an object The key claim of the book's opening sections is that "Speaking of objects just is using the linguistic devices of singular terms, predication, identity and quantification to make serious statements".

Thus construed, the idea of objects in general is loosened from ties with any idea of 'actuality' (Kant's *Wirklichkeit*) – where this has something to do, in Frege's words, with "act[ing] on our senses or at least producing effects which may cause sense-perceptions as near or remote consequences". Talk of objects is also loosened from ties with ideas of intuitability (whatever that Kantian idea comes to – things are in fact left pretty murky at this stage, but then Parsons is going to talk a lot about intuition later in the book). Consequently, endorsing the logical conception of an object will "defuse too-high expectations of what the existence of objects of some mathematical type such as numbers would entail." The suggestion is that those who are inclined to deny abstract

[1]Revised and necessarily much abbreviated excerpts will appear as a Critical Notice in *Analysis Reviews.* I am very grateful to fellow members of a reading group – especially Bob Hanna, Luca Incurvati, Steven Methven, Michael Potter, Tim Storer and Rob Trueman – for illuminating discussions, though they will probably think I've not been illuminated enough.

Inevitably, there are quite a few disagreements in what follows – this is philosophy, after all! The considerable length of these remarks witnesses, however, that I think Parsons's book is intriguing and important.

[2]So there is another route Parsons could have taken: he could have reprinted the relevant papers with with postscripts, and then top-and-tailed the collection with an extensive preface and perhaps added concluding reflections. It sounds ungrateful, but I rather suspect that that route might have worked better.

objects – meaning, broadly, things outside the causal nexus – or who find abstract objects puzzling, are imposing requirements on being an object that go beyond those captured in the logical conception.

Now, I am highly sympathetic to the broadly Fregean line Parsons is following here. He says that "its most important advocates in more recent times are Carnap and Quine". But that's a slightly odd claim. We should certainly add Dummett's name to the list, starting with his early paper on nominalism. And Dummett's work in turn is the spur to what is surely the most sophisticated development of the Fregean line in the hands of Crispin Wright in his *Frege's Conception of Numbers as Objects*, in Bob Hale's *Abstract Objects*, and then in further debates e.g. between Dummett on the one hand and Hale and Wright on the other.

Parsons talks of "the view that the most general notion of object has its home in formal logic". But *that* surely isn't the happiest way of summing up the view. After all, suppose we translate back from first-order logical notation into a disciplined core fragment of English – the sort of regimented English whose sentences are equivalent to the content of the logical wffs (and indeed the sort of regimented English which we use in giving determinate content to the wffs of the artificial language in the first place). Then here too, in regimented English, we will find the core devices of singular terms, predication, identity and quantification. And the Quinean will presumably say that our commitments to objects are revealed equally well by rendering our theory of the world into the idioms of this disciplined core of ordinary language. Or if that's not exactly right, because English can't be disciplined rigorously enough without rather radical mutilation, then this is not, so to speak, a deep failing of the vernacular. After all, formal languages don't magically do what ordinary language can't do: they just do ordinary things like use singular terms and quantify in much more reliably uniform ways. So turning to "formal logic" doesn't really give us a different take on the general notion of object. Surely Parsons spoke better when he expressed the position he is proposing as the view that "speaking of objects just is using the linguistic devices of singular terms, predication, identity and quantification" to make serious, and indeed true, statements.

Now note that, unqualified, that is quite a radical view. We might well all agree with something like the following: Such is the mess and conversational plasticity in our various ordinary ways of talking that you can't just read off our ontological commitments from our unregimented talk. We speak of doing things for Mary's *sake*, or having *ideas* at the back of our *minds*. Are we really committed to sakes, ideas and minds as objects, though? Well, surely, when we regiment our story of the world into a logically well-behaved language (whether fully formalized, or a decently regimented fragment of ordinary language), we won't end up quantifying over such things, making identity claims about them, etc. Our genuine commitments are only revealed by the behaviour of the quantifiers and singular terms of such a regimented version.

However, thus far, this is quite consistent with two further thoughts. The first thought is that there are constraints on the project of regimentation that are provided by some prior conception of what kosher objects must be like (actual, intuitable, or whatever). True, this thought does presumably offend against another Quinean idea, namely that there is no first philosophy prior to regimented science. But *that* idea is independent of the claim that ontological commitment is revealed in the use of the devices of singular terms and quantification: it *is* a further thought.

Second, even if we think that there is no first philosophy and regimentations just need to have the usual scientific virtues of simplicity, organizational economy, etc. (broadly conceived), we don't yet get to Parsons's position. For it is possible to hold that, while it is in the use of regimented singular terms and quantification to make true claims that we must discern reference to objects, not all such use is fully committing. We might try to discriminate (e.g. by reflecting on how some singular term is definitionally introduced into the regimented language).

That, indeed, seems to be Dummett's late position: we can distinguish expressions which have robust reference from those which have reference only in a thin sense, depending on how expressions are canonically introduced (e.g. by abstraction principles). Contrast this with the Hale/Wright line which resists making such discriminations:[3] at a first pass, the truths are what the relevant discipline (fallibly) aims at, a singular term in such truths is whatever walks, quacks, and swims like a singular term in a disciplined way, and the objects are (without distinction) whatever are the referents of such singular terms in true sentences. So on this second line, we don't discriminate among regimented singular terms by how they are introduced, picking out some as having robust reference, and taking the kosher objects to be what are referred to by *them*. Rather, in mathematics for example, we first identify true sentences by the appropriate criteria; we identify the singular terms in those sentences by their compositional behaviour: and then (the claim is) the relevant objects – mathematical objects in our example – are what those singular terms, functioning in the given truths, need to refer to make the truths indeed come out true.

Some of Parsons's remarks about his conception of objects, e.g. the unqualified claim that "speaking of objects just is using the linguistic devices of singular terms, predication, identity and quantification", suggest that he does intend to endorse the full-blown Hale/Wright position. But then it is perhaps odd that he doesn't more explicitly argue further for it and engage with the running debates about it.

§5 Is whatever is an object? Parsons has been proposing the view that "speaking of objects just is using the linguistic devices of singular terms, predication, identity and quantification". And the focus so far has been on first-order quantification. But what about generalizations about *properties*, the sort of generalization apparently involved in familiar mathematical statements like the full induction principle for arithmetic, or the separation axiom in set theory? Should we construe those as involving generalization over something like Frege's "unsaturated" concepts, entities which aren't objects? Or is the commitment here just to more objects?

One way of perhaps resisting the Fregean line arises from noting that we can easily parlay quantification into predicate position into just more quantification into subject position (or so it seems). Suppose, using Parsons's notation, we use '$(Ox)Fx$' to denote some object corresponding to the Fregean concept expressed by 'F'. And suppose we use 'η' for an appropriate copula ('has' if the object is a property/quality, 'is a member of' if the object is a set, etc.) Then we have Ft if and only if $t\ \eta\ (Ox)Fx$. And so, given a context when we are minded to quantify into the position held by 'F' we could instead first nominalize and then quantify into the position held by the singular term '$(Ox)Fx$' instead. It seems then that we can treat quantification over properties (as we might initially put it) as just more quantification over objects. This after all is a common mathematical practice, as e.g. when we familiarly regiment informal second-order arithmetic into a formal theory of numbers and *sets* of numbers.

[3]Latterly, e.g. in a response to Rumfitt, Hale and Wright have seemingly wanted to qualify what I'm calling the Hale/Wright line: so take the description with a pinch of salt.

Still, at least two objections to the nominalizing strategy as an across-the-board way of eliminating 'direct' quantification into predicate position readily suggest themselves (as Parsons notes). First, the claim that Ft if and only if $t \ \eta \ (Ox)Fx$ is, itself, intended as a generalization, to express which we need to generalize into predicate position in a way that can't be nominalized away. And second, that generalization in any case has to be restricted or else or we could instantiate with the predicate '$\neg x \ \eta \ x$', and paradox ensues.

However, that's not yet game set and match to the Fregean. Can't the force of the first objection be turned by adding the device of semantic ascent to our armoury? We can, for example, generalize about the possibility of nominalization by saying that for any predicate 'F' (and term 't'), 'Ft' is true if and only if '$t \ \eta \ (Ox)F$' is true.

Ah, it will be protested, the device of semantic ascent still doesn't really allow us fully to capture what we want to say by means of quantifications over properties. Compare for example the familiar thought that the content of the full informal arithmetic induction axiom is not captured by semantically ascending and saying that all instances of the first-order schema are true. Reply: that familiar thought is true, *if* we require the instances to be drawn from a fixed language. But suppose we treat the schema in an open-ended way, available to be instantiated however we extend our language (as Parsons puts it, "In practice, in any language in which we talk about natural numbers, we are prepared to affirm induction for any predicate of that language"). Then, by treating the schema in this way, we arguably recapture the intended sweep of the informal axiom, still without taking on ontological commitments to Fregean concepts.

And as to the second objection against the nominalizing strategy, the threat of paradox only arises if we take the reference of '$(Ox)Fx$' as an object that is, so to speak, already in the original domain of objects (i.e. the original domain of subjects of predication). But we could take the moral here to be that objects segregate into different types, the references of nominalized predicates being of a different type to the references of common-or-garden singular terms.

So where does this take us? Parsons summarizes: "the present discussion does show that considerations about predication do not lead inevitably to our taking second-order logic as our canonical framework and admitting, as values of our second-order variables, entities that are not objects."

Three comments about all this. First, about semantic ascent and the open-ended nature of our commitment e.g. to the induction schema. Just *why* do we stand prepared to take on all-comers and instantiate the schema with any novel predicate we care to extend our language with? Kreisel suggested long since that we accept the instances of the induction schema because we *already* accept the full second-order induction axiom. I think there are issues about that claim: but the claim is a familiar one that many have found persuasive. So a fuller defence of the idea that we can avoid taking second-order quantifications at face value needs Parsons to say more about this – which he doesn't do here (though there are some relevant discussions much later, in §47).

Second, about avoiding paradox on the nominalizing strategy. The Fregean might well riposte that saying that the way to go is to segregate objects into different types just sounds like theft of Frege's key insight rather than an alternative story. After all, speaking with the vulgar, the Fregean will say that what he is arguing for is precisely a distinction among "entities" between saturated and unsaturated types, between objects and concepts. So he has a principled story about types to tell. Further, he will add, once the distinction is made in the right way, the temptation to pursue the nominalizing strategy, putting all the work of unifying propositions into a copula, should evaporate. And what is the alternative principled story supposed to be?

Third, I'm left unclear exactly how Parsons thinks about the relationship between the two ways of avoiding second-order quantification that he discusses (i.e. the routes via nominalization and ascent). He does say that "The laws of logic have a certain dialectical character, in that the method of nominalization and the method of semantic ascent can both be used to state them, and neither can completely displace the other." But *that* is unclear to say the least. Perhaps Parsons is here harking back to remarks he makes in his 'Sets and Classes' paper where again there is discussion of the interplay between dealing with class talk via nominalization and via semantic ascent: but making that connection didn't really help me here.

But finally, a comment before proceeding. Note that Parsons has proposed that (1) "speaking of objects just is using the linguistic devices of singular terms, predication, identity and quantification" to make serious, and indeed true, statements. And defending that view about, so to speak, the measure of what *objects* we are committed to falls well short of saying that (2) standard first-order logic is the universal measure of ontology in general. Resisting the more sweeping claim is quite consistent with accepting Parsons's initial Fregean claim about objects. Not that I'm suggesting that Parsons thinks otherwise. I'm just emphasizing that if you are not persuaded by Parsons's suggestions in this current section, and hold that we are committed to entities that are not objects – first-level Fregean functions and concepts, for a start – then you can accept formulation (1) without accepting (2).

§6 Being and existence At the outset of this section, Parsons writes that one point at which "reservations about standard first-order logic as the universal measure of ontology can affect the notion of mathematical object is the ancient question whether reference to objects is necessarily reference to objects that exist." Parsons discusses Meinongian views in some detail: indeed, this is one of the longest sections in the book. Here's part of his final summary of the discussion:

> We are left with the question whether the "true" meaning of the existential quantifier is [i] the permissive Meinongian one [allowing quantification over objects that do not exist], [ii] existence that allows freely for abstract objects but that rules out impossibilia, or [iii] something like actuality. The logic based concept of object does not decide between these alternatives, although, once it has been set forth, the case for [iii] is weakened. But in order to understand the notions of object and existence in mathematics we have to put more flesh on the bare form given by formal logic. We need to fill out the logic-based conception by looking at cases. ... [C]onsiderations proper to mathematics will not lead us to favour [i] over [ii]. General as the notion of object in mathematics is, there is still a constraint of possibility, coherence, or consistency that objects postulated in Meinongian theories are allowed to violate.

The talk here of having to "fill out the logic-based conception" might initially seems surprising given what has gone before. But I take it that the thought is simply this. The Fregean thesis (read the Wright way, if not the right way) is that objects are whatever we have to construe singular terms in true sentences as referring to, if the sentences are indeed to come out true (where being a singular term is a matter of interacting with quantifiers and the identity predicate in the right way). Hence, to “fill out” this general template view about objects, we have to say what kinds of sentences we do in fact accept as being true. If we e.g. take statements like “Sherlock Holmes is more famous than any living detective” and “There’s a fictional detective who is more famous than any living detective” at face value – i.e. construe their logical form as given in their surface structure – and take them moreover as true claims, then (the suggestion goes) we have to accept (i) the Meinongian line that there are objects that do not exist. If we paraphrase away apparent talk of fictional objects and the like, but accept that there are true mathematical statements talking of numbers, sets, etc., then (ii) we are not committed to non-existent objects, but have to accept that there are abstract objects which aren’t “actual”. If we insist on also paraphrasing away apparent straight talk of numbers (e.g. construing it as governed by an operator “in the arithmetical fiction ...”), then perhaps (iii) we may only be committed to actual objects.

Parsons is pretty sceptical about whether we have any need “to admit into the range of our quantifiers such objects as the golden mountain, the round square, Pegasus and Sherlock Holmes”, though it is not his concern to argue for this here. But he *does* argue that “considerations proper to mathematics” don’t give any impetus for preferring the Meinongian views (i) over (ii). For mathematics doesn’t countenance impossibilia like the round square, or present itself as fictional discourse. And that’s right and important. As to (iii), I assume Parsons’s thought is that a critic of our common-or-garden standards of mathematical truth on the basis of a metaphysical repudiation of abstract objects is (in danger of) getting things upside down, at least by the lights of the truth-first, “logic-based conception” of objects, according to which we don’t have a handle on the notion of an object except via a prior grip on the notion of truth for the relevant object-referring statements.

If this reading of Parsons is right, then I agree with him.

§7 Abstract objects and their concrete representations Back in §1, Parsons says “Roughly speaking, an object is abstract if it is not located in space and time and does not stand in causal relations.” In the last section of the first chapter, he returns to question of characterizing abstract objects, and suggests a distinction among them between pure abstract objects (e.g. pure sets) and those which “have an intrinsic relation to the concrete” – Parsons calls the latter quasi-concrete.

As a paradigm example of the quasi-concrete, Parsons takes the example of sentence types:[4] “what a sentence [type] is is a matter of what physical inscriptions are or would be its tokens”. But how should we generalize from this case? Parsons writes “What makes an object quasi-concrete is that it is of a kind which goes with an intrinsic, concrete ‘representation’”. The scare quotes are there in Parsons – and you can see why. Should we really say, for example, that a sentence token is a *representation* of its type? Your first response might be: the token isn’t *about* the type, so isn’t a representation of it. But, reading on, it becomes clear that Parsons doesn’t mean representation so much as *representative*. And then, yes, I suppose we might say that the token is a representative of the type. Parsons also writes “Although sets in general are not quasi-concrete, it does seem that sets of concrete objects should count as such; here the relation of representation

[4]Just as an aside, I suppose we might then wonder for a moment whether sentence types might be a counter-example to the claim that abstract objects lack temporal location. We might ask: did the sentence type “the cat is on the mat” really exist in 2000 BC before anyone spoke English? Or what about that other quasi-concrete thing(?) the Earth’s equator – does this only exist when the Earth is spinning?

would be just membership.” (no scare quotes!). Again, we might say the spoon in my coffee cup is a representative of the set of cutlery – though it surely can’t be said to be a representation.

Now these cases might suggest that Parsons’s quasi-concrete objects are those formed by abstraction from equivalence classes of concreta. The type-exemplified-by-token-S_1 is the same type as the type-exemplified-by-token-S_2 just if S_1 is equiform to S_2. The set-of-cutlery-of-which-s_1-is-a-representative-spoon is the same set as set-of-cutlery-of-which-s_2-is-a-representative-spoon just if s_1 belongs together with s_2. But if this is what Parsons has in mind, why not put it this way, instead of in terms of representations/representatives?

But let’s agree, it *is* plausible to say that some abstract objects are more directly tied to the concrete than others: and let’s suppose that we have suitably tidied Parsons’s notion of such quasi-concrete abstracta. Then he raises the question, are *numbers* quasi-concrete in this sense? We might be tempted to say yes, suggesting that the number five, for example, has concrete representatives like: ||||| . Parsons, however, makes two good Fregean points against this. First, to take that block as representative, we have already to take it *as* a set of strokes (rather than as a single grid, for example). So the representative here is not concrete. But second, it isn’t even that numbers have only quasi-concrete representatives, as numbers can number anything, including the purely abstract. (Parsons is going to return to talk about related matters in Chapter 6, so I’ll say no more for the moment.)

2 Eliminative Structuralism and Nominalism

§§8–11 Introducing eliminative structuralism The first four sections of this chapter covers some rather familiar territory, and I don’t have much to add by way of commentary. So I’ll confine myself with a quick overview summary.

§§8, 9: Parsons says that he himself thinks that “something close to the structuralist view is true”. But structuralist in what sense? It is often said, perhaps in a Bourbachiste spirit, that mathematics is the study of structures. But *that* claim taken by itself leaves it wide open what picture we should adopt of the ontology of mathematical objects. So Parsons stresses that he is more concerned with varieties of structuralism(s) which come with an ontological manifesto – something along the lines suggested by “the objects of mathematics are positions in structures, [and] have no identity or features outside of a structure” (to quote from Michael Resnik’s well-known 1981 *Noûs* paper). Though, Parsons notes, *that* can’t be the whole story about quasi-concrete mathematical objects if such there be (“because the representation relation is something additional to intrastructural relations”)

But if objects are “positions in structures”, what are structures? The Bourbachiste can – indeed typically does – takes structures to be sets (domains with distinguished elements and equipped with relations and/or functions which are themselves treated as more sets). So it looks as though an account of mathematical objects as “positions in structures” already presupposes familiar kinds of abstract objects (sets) to build structures out of. But then going on to explain the nature of sets in their turn in structuralist terms threatens circularity. But Parsons puts this worry for a global structuralism on hold for the moment, and so let’s start – at any rate – by exploring a version of structuralism that uses a background set-theoretic conception of structure.

§10: In particular, then, consider as an exemplar Dedekind’s treatment of the natural numbers (read in a way that is not true to the historical Dedekind, but let's not fuss about that now, particularly as Dedekind's own view, which we'll return to touch on in discussing §18, is rather murky).

Dedekind defines what it is for a set N, with distinguished element 0, and a mapping $S : N \to N - \{0\}$, to be 'simply infinite'. Abbreviate those (categorical) conditions $\Omega(N, 0, S)$. With some effort, an ordinary statement of arithmetic can be correlated with a version $A(N, 0, S)$ whose primitives are again $N, 0, S$. And on one reading of Dedekind – the eliminative reading – the suggestion is that the ordinary statement can be treated as elliptical for

For any $N, 0, S$, if $\Omega(N, 0, S)$ then $A(N, 0, S)$.

This is 'eliminative' in that a statement apparently about one kind of thing, numbers, is treated as in fact a disguised generalization about other kinds of things. And it might be said to be structuralist, since it's what is in common to all simply infinite systems – not the objects occupying positions but the relations between the positions themselves – which matters for arithmetical truth.

This eliminative structuralism neatly sidesteps 'multiple reduction' problems for more straightforward attemps to reduce arithmetic to set theory. But (on the face of it) it faces the serious worry that if there are no simply infinite systems then any ordinary arithmetical statement comes out as vacuously true. True, this first worry won't be very pressing if we already buy into a rich enough background universe of sets: but it will become more urgent if and when we try to repeat the trick and give an eliminative structuralist account of really big mathematical universes.

Parsons also raises a more immediate worry. $\Omega(N, 0, S)$ will involve quantification over sets (likewise indeed for a typical $A(N, 0, S)$, as we give explicit definitions of e.g. recursive arithmetical functions). We might ask: do we really want a structuralist account of a particular familiar kind of mathematical object, numbers, to tell us that we've been generalizing about some other rather less familiar kind of object all along? I'm not sure, however, how telling this thought is. For are we to take eliminative structuralism 'hermeneutically', as a story about what we've been meaning all along, or in a more 'revolutionary' spirit, as supposedly offering us the best chance to preserve the truth of common-or-garden arithmetical claims while keeping our ontology respectable? Parsons doesn't explore the distinction.

§11: Now, Parsons suggests, we can perhaps sidestep *some* worries by trading in an explicitly set-theoretic presentation of eliminative structuralism for a version couched in second-order logical terms. We get a new second-order definition of being simply infinite, $\Omega'(N, 0, S)$, a new correlate of an ordinary arithmetical claim, $A'(N, 0, S)$, and correspondingly a new suggestion that the ordinary statement can be treated as elliptical for

For any $N, 0, S$, if $\Omega'(N, 0, S)$ then $A'(N, 0, S)$

where now 'any N' and 'any S' are treated as second-order. If we are relaxed enough about second-order quantification, we might find this easier to swallow than the previous version of structuralism that traded explicitly in sets (though that's quite a big 'if').

However, this kind of 'if-thenism' is *still* threatened by the possibility of vacuity. What to do? One option is to read the conditional as stronger-than-material, e.g. by discerning a governing modal operator. But that opens up a whole new set of problems. What kind of modality is involved here?

Some of these issues are to be pursued in the following chapter. But we might wonder first if we can perhaps sidestep issues about modality by giving a very modest possibility-as-consistency reading. Perhaps "we interpret the theories in an if-thenist way, but deal with the problem of possibility by appealing to consistency, nominalistically interpreted." Well, this suggestion is to be pursued critically in the following section (but I should say that I found it, and the rest of this chapter, something of a jumble of concerns).

§12 Nominalism Parsons understands 'nominalism' Harvard-style – no surprise there, then! – to mean the rejection of abstract entities and the eschewing of (ineliminable) modality. What hope, then, for giving a response to the potential-vacuity problem for eliminative structuralism about arithmetic (say) which meets nominalist constraints? We can't, by hypothesis, go ineliminably modal: so what to do?

Well, as the physical world actually is (or so we might well now believe), there are in fact enough physical things – e.g. space time points – and suitable physical orderings on them to give us physically realized simply infinite structures. But Parsons is unhappy with this way of bluntly rejecting the vacuity worry, and for familiar reasons: "[S]hould it be taken as a presupposition of elementary mathematics that the real world instantiates a mathematical conception of the infinite? This would have the consequence that mathematics is hostage to the future possible development of physics."

But (although I have no particular nominalist sympathies myself), I'm not sure quite how worried the hard-core nominalist should be about giving such hostages to fortune if he treats arithmetic, say, in the eliminative structuralist way. For as things are, given how we believe the world actually to be, such a nominalist can believe that there are physical instances of omega sequences, and can hence continue to speak with the vulgar and treat arithmetical claims (as he construes them) as non-vacuously true or false. And even if the worst happens, so we change our minds and come to believe the world is in fact ultimately grainy and finite in all respects, it's not that the 'school-room' arithmetic of feasibly computable numbers is going to get undermined. At most, it is the idealizing rounding out of school-room arithmetic which insists on an infinitude of numbers which is in trouble. And if it indeed should emerge that this rounding out, construed the eliminative-structuralist way, makes any proposition of idealized arithmetic vacuously *true* without discrimination, that doesn't make the mathematician's game of seeing what follows from what in idealized arithmetic vacuous or trivial – it just means that the game doesn't track the truth, any more than the wilder reaches of set theory do. (Parsons says "a great deal of the historically given mathematics would have to be jettisoned in this case [i.e. on the eliminative structuralist reading, if e.g. there are no simply infinite systems]". But talk of 'jettisoning' covers over a slide. For no longer thinking of arithmetic as construable as literally true is not the same as throwing arithmetic into the trash-can, as any fictionalist will be quick to insist.)

What about the other line that offered to the nominalist at the end of §11? – i.e. sidestep the vacuity problem by going modal but in a nominalistically tolerable way ("interpret the theories in an if-thenist way, but deal with the problem of possibility by appealing to consistency, nominalistically interpreted"). Well, again Parsons sees trouble, this time arising from the fact that there might be physical limitations in how big a proof-token could be, and so a theory could count as (nominalistically) consistent – because no proof of an inconsistency could be tokened – even if we can show that there is a process which, if only there were world enough and time, would produce an inconsistency.

At the end of this section, Parsons revisits the question of how to frame an eliminative structuralism for arithmetic. He looked at a move from a set-theoretic formulation to a more 'logical', second-order formulation. But now he asks: could we in fact go first-order, in a way more congenial no doubt to those of nominalist inclinations? The trouble is, of course, that we won't get categoricity (whatever we build into the axioms), so the eliminative structuralist who goes first-order runs up against the intuition that the natural numbers have a unique structure. But how secure, we might wonder, is that intuition? Parsons raises the question only to shelve it until Ch. 8. So we'll have to return to that later.

§13 Nominalism and second-order logic This long section falls into two parts. First, Parsons offers some remarks on the Fieldian project of using mereology to do the work of second-order logic. The key thought is this. For mereology to do all the work Field wants, it needs an (impredicative) comprehension principle: "Given a predicate of individuals that is true of at least one individual, there is a sum of just the individuals of which the predicate is true, and moreover, the admissible predicates will be closed under quantification over all individuals, including those very sums." (Cf. the principle 'Cs' in Field's paper 'On Conservativeness and Incompleteness'.) But what entitles Field to such a strong comprehension principle? Well, Parsons notes that it's not clear that Field can offer any direct a priori argument (but then, I wonder, would he want to?). The justification will be that "the comprehension principle is a hypothesis justified by its consequences in systematizing the geometrical basis of physics". But then "Field's view, on this reading, puts him in a position in which we have found other formulations of nominalism: making the justification of mathematics turn on some hypothesis about the physical world, which is more vulnerable to refutation than the mathematics."

But again, just how troubled should be someone of nominalist inclinations be? Suppose we decide that our physical theory of the world doesn't require such a strong comprehension principle (we can get away with recognizing a less wide-ranging plurality of regions). That's not at all implausible, actually, given that (nearly) all the mathematics required for physics can be reconstructed in a weak second-order arithmetic like ACA_0 with only predicative comprehension. Then we would just demote the full mathematical apparatus of the classical reals from its alleged status as being justified as a required tool for getting more nominalistically acceptable consequences out of our best physics. It turns out that it is no longer so justified. In that sense, for the Fieldian, the "justification" of a bit of mathematics is indeed wrapped up with our hypotheses about the physical world, and Parsons's complaint will seem question-begging.

The second part of this section considers Boolos's attempt to make second-order logic ontologically tame by giving a plural reading to the second-order quantifiers. The thought under scrutiny is that plural quantification is ontologically innocent because, in plurally quantifying over Fs, we are just committing ourselves to the Fs (not to sets or to Fregean concepts). Parsons's discussion – or am I missing something here? – initially advances now rather familiar sorts of worries about this claim of innocence. But Parsons does make one point towards the end of the section that I find very congenial (i.e. I've argued similarly myself!).

Consider (say) the range of second-order arithmetics that Simpson discusses in his *SOSOA*. As we advance through theories with stronger and stronger comprehension principles, then – on a standard platonist construal – we are countenancing more and more sets of numbers. If we reconstrue the second-order quantifiers plural-wise, then, as we go from theory to theory, we are countenancing more and more . . . well, more what? It is tempting to say 'pluralities'. And indeed it is convenient to give an informal gloss of the plural reading using talk of pluralities. But – if this isn't to smuggle back reference to pluralities-as-single-entities, i.e. sets – this convenient way of talking needs to be eliminable.[5] So how do we eliminate it here? We might, I suppose, trade in talk of countenancing more and more pluralities for talk of allowing more and more different *ways* we can take numbers together: but quantifying over ways of taking numbers together seems tantamount to re-instating Fregean concepts as the values of the second-order variables – which is fine by me, but then the supposed ontological gain of interpreting the second-order quantifiers via plurals is lost.

The question then is this: if we accept the pluralist's contention that we can treat second-order numerical quantifiers as ontologically committing just us to numbers, period, then how are we to think of the surely varying commitments we take on with varying strengths of comprehension principle. As Parsons nicely puts it, "If there is no enlargement of ontological commitment as one passes to less restricted versions of the comprehension schema, then perhaps that speaks against the importance of the notion."

§14 Structuralism and application We're considering the schematic idea that an ordinary arithmetical statement is elliptical for something generalizing over structures, along the lines of

For any $N, 0, S$, if $\Omega(N, 0, S)$ then $A(N, 0, S)$,

where $\Omega(N, 0, S)$ lays down the conditions for a set N (equipped with a distinguished element 0, and a mapping $S : N \rightarrow N - \{0\}$) to be 'simply infinite', and $A(N, 0, S)$ is appropriately correlated with the ordinary statement.

Does this eliminative structuralist view have a problem accounting for the application of numbers as cardinals? Recall Frege's remark: "It is applicability alone that raises arithmetic from the rank of a game to that of a science. Applicability therefore belongs to it of necessity." And Frege further takes it that an account of numbers should start from their use in counting (so a structuralist understanding that explains the nature of arithmetical truths prior to explaining their application is going wrong). But, Parsons argues, our structuralist in fact can resist that further thought.

I'm not sure I fully have the measure of Parsons thinking here. Part of the trouble is that he slips back into talking of numbers as objects (e.g. at pp. 74–75), while I thought the attraction of the eliminative structuralism was to get rid of numbers as a special kind of object. But I take it that the idea is something like this. Counting some objects involves putting them into one-one correspondence with an initial segment of some paradigm simply infinite system (of numerals, say). That involves setting up some external relations between some members of the relevant simply infinite system, over an above the internal relations which constitute their being a such a system. But now, via the Dedekind categoricity theorem, we see that these external relations will engender a one-one correspondence with an isomorphic initial segment of any simply infinite system. So, in counting, there is an implicit generalization over simply infinite systems in the offing – which is what, according to the eliminative structuralist, talk of numbers amounts to. Hence, as Frege wanted, even on the structuralist view, we do after all have an essential connection between numbers and their application in counting.

[5] See here Linnebo's nice article on plural logic in the Stanford Encyclopedia.

That, I think, does deal with the supposed general problem. Now, Dummett has raised a more specific problem – roughly, defining a simply infinite system doesn't tell us whether its initial element is to be treated as 0 or 1 (or indeed, I suppose, 42). But Parsons (rightly in my view) doesn't find this worry a telling one for the structuralist. He can regard it as just a matter of pragmatic convention whether, in applications, we start counting at 0 or 1, depending on how much we care about having a number for empty collections.

One final comment on this section. Having quieted worries about the structuralist view, Parsons remarks that as well as the natural number 3, we have the integer 3, the rational 3, the real number 3 and the complex number 3 (not to mention more exotic constructions). And the structuralist can say that the use of '3' each time signifies not the same entity but the same structural role, a point congenial to his general account of the significance of number words. But, contra Parsons, I don't at all see that the multiple use of '3' counts at all against the Fregean view that numbers are specific objects. The Fregean can just say that there are here a range of different terms, ('natural number 3', 'rational number 3', 'real number 3', etc.) which have different objects as their referents. The occurrence of '3' in each of those term is not just a pun: for the denoted objects, intuitively, have analogous roles in the respective families. Or to put it more fancily, the naturals have a canonical embedding into rationals and reals, etc. But note that an embedding is a *mapping* not a literal placing of one structure inside another.

3 Modality and structuralism

§15 Mathematical modality Before turning to discuss modal structuralism in §§16 and 17, Parsons discusses what *kind* of modality a prima facie defensible modal structuralism might involve. Setting aside epistemic modalities as not to the present purpose, he considers (i) physical (or natural) necessity, (ii) metaphysical necessity (truth in all possible worlds), (iii) mathematical necessity, (iv) logical necessity (meant in a narrow sense that can be explicated model-theoretically).

Parsons argues that we don't want to spell out a modal structuralism in terms of (i) natural modalities: "it demands too much to ask that the structures considered in mathematics be physically possible; indeed, in the case of higher set theory, there is every reason to believe that they are not physically possible." Well, that is indeed too much to ask, *if* we want swathes of highly infinitary mathematics to come out true on the structuralist reading: but it isn't so obvious that the structuralist about the mathematics-we-have-to-take-seriously has to be a conservative in that way. But let that point pass for now.

Second, Parsons argues that (iv) logical possibility – at least in the sense standardly explicated model theoretically – reveals itself as itself a mathematical notion, given that models are (at least typically) mathematical entities. So(?), "It is very doubtful that a generous notion of logical possibility would be distinguishable in a principled way from ... mathematical possibility."

But hold on! The idea, to repeat, is that we explicate 'it is logically possible that P' (in the generous sense of allowed-at-least-by-considerations-of-logical-form, which allows 'logical' possibilities that run beyond the metaphysical possibilities) in terms of there being a mathematical model on which P can be interpreted as true. That indeed is a mathematical notion: but it isn't evident that we have a modality in the explanans here – indeed, I thought that the supposed virtue of the model-theoretic treatment of logical necessity is that it enables us to trade in a murky modal notion for a clean set-theoretic one. Note too that Parsons himself remarks on the common view that a mathematical truth (falsehood) is necessarily true (false): and on that view the very idea of a kind of 'mathematical possibility' distinguished from plain mathematical truth evaporates.

I'm left puzzled, then, when Parsons concludes that the two runners for the kind of modality that might be involved in a modal structuralism are (ii) metaphysical modality and (iii) mathematical modality – and when he recommends the latter over the former. For do we really have a grip on the latter notion of mathematical modality?[6]

§16 Modalism Parsons now returns to the problem we left hanging at the end of §11, and considers the stratgegy of rescuing eliminativist structuralism from the vacuity problem by going modal.

To recap once more, the eliminative structuralist's proposal is that an ordinary arithmetical statement is elliptical for something along the lines of

For any $N, 0, S$, if $\Omega(N, 0, S)$ then $A(N, 0, S)$,

where $\Omega(N, 0, S)$ lays down the conditions for a set N (equipped with a distinguished element 0, and a mapping $S : N \to N - \{0\}$) to be 'simply infinite', and $A(N, 0, S)$ is appropriately correlated with the ordinary statement. The idea is, of course, that the quantifications here are restricted to kosher, unproblematic, collections of physical objects N, and mappings on them (such as arrays of space-time points, and a 'go to the next point' map) so that problematic purely abstract entities drop out of the picture. And the vacuity problem is: what if, as a matter of physical fact, ours is a finite, granular, world and there is no infinite physical collection and physically realized mapping for which the condition Ω is true? In that case, the quantified conditional holds vacuously, and all arithmetical statements come out as indiscriminately true.

Now, the obvious modal gambit is to respond: Ah, we should require the quantified conditional to hold necessarily. For, even if in *this* world there are no physically realized simply infinite systems, there could be other metaphysically possible worlds (maybe where the physical laws are very different) which *do* realize simply infinite systems. Why not? So even if the plain unmodalized conditional is vacuously true for any $A(N, 0, S)$, the modalized version won't be.

Put like that, though, surely the *natural* way of construing the modality here is surely as as truth in all metaphysically possible worlds. So, as I remarked above, I'm a bit stumped as to why Parsons says "I will assume that the modal operators are understood either in the sense of mathematical modality or of metaphysical modality." For I'm quite unclear what the first disjunct is doing here, what a distinct notion of mathematical modality would gain us here.

Anyway, Parsons considers two main objections to modal structuralism, one in the present section, one in the next (and he also remarks again on the use of second-order logic). The first main objection, however, turns out not to be an objection to the modal version in particular: the key idea is just that "it is falsifying the sense of discourse about natural numbers [to take] arithmetical statements to be really about every simply

[6] We might wonder whether what's in the background here is the Quine/Putnam contrast between those bits of mathematics which are indispensable for science, and coherent mathematics more generally. Only the former is actually true: and I suppose we *might* say that the latter bits of mathematics (waiting to be recruited for use in talking about the world) express mere "mathematical possibilities". But I have no idea whether that is how Parsons is thinking.

infinite system." But surely many an eliminative structuralist will think that *this* is pretty feeble. The 'revolutionary' structuralist is trying to get the metaphysics right: and even if his account seems prima facie to do some violence to our intuitive model of what we mean, such a structuralist will just say that that shows we have become too deeply gripped by a bad folk-philosophical picture of what we are really up to in doing mathematics. In any case, as Parsons himself puts it, "Putting this generality into the explicit content of the statements will for some be a small price to pay for avoiding an ontology of mathematical objects."

§17 Difficulties of modalism Parsons presses a second objection, this time more specifically against modal structuralism, by querying "whether the modalist's apparatus really does offer an elimination of mathematical objects". But in fact the way he develops the point seems aimed not against a modal structuralist account of arithmetic or indeed applicable analysis (given we can construct applicable analysis in weak systems of second-order arithmetic). Rather the worry seems to be specifically whether there could be structures realized in some sort of alternative 'physical' world which have anything like the richness to be a model of higher set theory.

Well, indeed, maybe they can't be – though I'm not sure how one goes about settling the issue. How do we weigh Parsons's confidence that the modalist can't really hope to find enough to model set theory against Hellman's apparent confidence that we can? (Not for the first time in the book, there seems an odd reluctance here to engage straight on with a key bit of the literature). But in any case, why shouldn't one kind of modal structuralist – if not Hellman! – just bite the bullet and say "so much the worse for higher set theory; it's just a fiction, or a jeux d'esprit, which we can't construe as being literally true"?

To put it bluntly, why should the structuralist about the 'respectable' bits of maths feel that he has to save suspect flights of fancy like higher set theory and allow those too to be true?[7]

§18 A noneliminative structuralism The previous two sections critically discussed a modal version of *eliminative* structuralism (even though, to my mind, the objections raised weren't particularly telling against e.g. modal structuralism about arithmetic, as Parsons seems to concede in §31). Parsons now moves on characterize his own preferred *noneliminative* version, and he responds to some potential obections. I do wish I could give a really sharp characterization of the position Parsons wants to occupy here in the longest section of his book. But I do have to confess some bafflement.

Parsons makes two key initial points. (1) Unlike the eliminative structuralist, the noneliminativist "take[s] the language of mathematics at face value". So arithmetic, say, is indeed about numbers. What characterizes the position as structuralist is that we don't "take more as objectively determined about the objects about which it speaks than [the relevant mathematical] language itself specifies". (2) Then there is "the aspect of the structuralist view stressed by Bernays, that existence for mathematical objects is in the context of a background structure." Further, structures aren't themselves objects, and "[the noneliminativist] structuralist account of a particular kind of mathematical

[7] Of course there are issues about where respectability gives out! As Luca Incurvati stressed in conversation, even full-blown classical analysis – though it sits so low down the set hierarchy – has a continuum with such a rich structure that we might well wonder whether an alternative 'physical' world can realize that structure either.

object does not view statements about that kind of object as about structures at all".

But note, surely there's thus far nothing in (1) and (2) that e.g. the neo-Fregean Platonist need dissent from. The neo-Fregean can agree e.g. that numbers only have numerical intrinsic properties (pace Frege himself, even raising the Julius Caesar problem is a kind of mistake). Moreover, he can insist that individual numbers don't come (so to speak) separately, one at a time, but come all together forming an intrinsically order structured – so in, identifying the number 42 as such, we necessarily give its position in relation to other numbers.[8]

So what *more* is Parsons saying about (say) numbers that distinguishes his position from the neo-Fregean? Well, he in fact explicitly compares his favoured structuralism with what he calls the view that the natural numbers, for example, are *sui generis* in the sort of way, I suppose, that the neo-Fregean holds. He writes

> One further step that the structuralist view takes is to reject the demand for any further story about what objects the natural numbers are [or are not].

The picture seems to be that the neo-Fregean, say, offers a "further story" at least in negatively insisting that numbers are sui generis, while the structuralist refuses to give such a story:

> If what the numbers are is determined only by the structure of numbers, it should not be part of the nature of numbers that none of them is identical to an object given independently.

But of course, neo-Fregeans like Wright and Hale won't agree that their rejection of cross-type identities is somehow an optional extra: they offer arguments which – successfully or otherwise – at least purport to block the 'Julius Ceasar problem' and reveal questions about cross-type identifications as ruled out by our very grasp of the content of number talk. So from this neo-Fregean perspective, we can't just wish into existence a coherent structuralist position that (a) construes our arithmetical talk at face value, as referring to numbers as objects, while (b) insisting that the possibility of cross-type identifications is left open, because – so the story goes – a properly worked out version of (a), and reflection on the ways that objects are identified under sortals, implies we can't allow (b).

Now, on the sui generis view about numbers, claims identifying numbers with sets will be ruled out as plain false. Or perhaps it is even worse, and such claims fail to make the grade for being either true or false (though it is, of course, notoriously difficult to sustain a stable, well-motivated, distinction between the neither-true-nor-false and the plain false – so let's not dwell on this). Conversely, assuming that numbers are objects, if claims identifying them with sets or the like are false (or worse), then numbers are sui generis. So it seems that if Parsons is going to say that numbers *are* objects but are *not* sui generis, he must allow that claims identifying numbers with sets (or if not sets, at least some other objects) might be true. But then Parsons is faced with the familiar Benacerraf "multiple-candidates" problem (if not for sets, then presumably an analogous problem for other candidate objects, whatever they are: let's keep things simple by running the argument in the familiar set-theoretic setting). How *do* we choose e.g. between saying that the finite von Neumann ordinals are the natural numbers and saying that the Zermelo ordinals are?

[8]Hale and Wright say as much in *The Reason's Proper Study*, p. 1, fn. 1.

It seems arbitrary to plump for either choice. Rejecting both together (and other choices, on similar grounds) just takes us back to the sui generis view – or even to Benacerraf's preferred view that numbers aren't objects at all. So that, it seems, leaves just one position open to Parsons, namely to embrace *both* choices, and to avoid the apparently inevitable absurdity that, say, $\{\emptyset, \{\emptyset\}\}$ is identical to $\{\{\emptyset\}\}$ because both are identical to 2 by going contextual. It's only in one context that '$2 = \{\emptyset, \{\emptyset\}\}$' is true; and only in another that '$2 = \{\{\emptyset\}\}$' is true.

And this does seem to be the line Parsons seems inclined to take: "The view we have defended implies that [numbers] are not definite objects, in that the reference of terms such as 'the natural number two' is not invariant over all contexts." But how are we to understand that? Is it supposed to be rather like the case where, when Brummidge United is salient, 'the goal keeper' refers to Joe Doe, but when Smoketown City is salient, 'the goal keeper' refers to Richard Roe? So when the von Neumann ordinals are salient, '2' refers to $\{\emptyset, \{\emptyset\}\}$ and the Zermelo ordinals are salient, '2' refers to $\{\{\emptyset\}\}$? But then, to pursue the analogy, while 'the goal keeper' is sometimes used to talk about now this particular role-filler and now that one, the designator is apparently also sometimes used more abstractly to talk about the role itself – as when we say that only the goal keeper is allowed to handle the ball. Likewise, even if we grant that '2' sometimes refers to role-fillers, it seems that sometimes it is used to talk more abstractly about the role – perhaps as when we say, when no particular ω-sequence of sets is salient, that 2 is the successor of the successor of zero. Well, is *this* the way Parsons is inclined to go, i.e. towards a structuralism developed in terms of a metaphysics of roles and role-fillers?

Well, Parsons does also talk of "the conclusion that natural numbers are in the end roles rather than objects with a definite identity". *But then why aren't roles 'objects' after all, in his official 'logical' sense?* – for we can use "the linguistic devices of singular terms, predication, identity and quantification to make serious statements" about roles (and yes, we surely *can* make claims about identity and non-identity: the goal keeper is not the striker). True, roles are as Parsons might say, "thin" or "impoverished" objects whose intrinsic properties are determined by their place in a structure. But note, Parsons's official view about objects didn't require any sort of "thickness": indeed, he is "most concerned to reject the idea that we don't have genuine reference to objects if the 'objects' are impoverished in the way in which elements of mathematical structures appear to be". And being merely 'thin' objects, roles themselves (e.g. numbers) can't be the same things as 'thick' role-fillers. So now, after all, numbers qua number-roles do look to be sui generis entities – objects, in the broad logical sense, which are not to be identified with any role-filler – in other words, just the kind of thing that Parsons seems *not* to want to be committed to.

Parsons's also briefly discusses Dedekind abstraction, and similar issues arise. To explain, suppose we have a variety of 'concrete' structures, whether physically realized or realized in the universe of sets, that satisfy the conditions for being a simply infinite system. Then Dedekind's idea is that we 'abstract' a new structure $\langle N, 0, S\rangle$ which is – so to speak – a 'bare' simply infinite system without other inessential physical or set-theoretic features, and it is elements of this system which are the numbers themselves. (Erich H. Reck nicely puts it like this:[9] "[W]hat is the system of natural numbers now? It is that simple infinity whose objects only have arithmetic properties, not any of the additional, 'foreign' properties objects in other simple infinities have.") Since the bare

[9]In his very illuminating article on 'Dedekind's structuralism', Synthese, 2003: see also his entry on *Dedekind's Contributions to the Foundations of Mathematics* for the Stanford Encyclopedia.

structure is *all* that is generated by the Dedekind abstraction, "it conforms to the basic structuralist intuition in that the number terms introduced do not give us more than the structure", to borrow Parsons's words. But, he continues,

> This procedure gets its force from the use of a typed language. Thus, the question arises what is to prevent us from later, for some specific purpose, speaking of numbers in a first-order language and even affirming identities of numbers and objects given otherwise.

To which the answer surely is that, to repeat, on the Dedekind abstraction view, the 'thin' numbers determinately *don't* have intrinsic properties other than those given in the abstraction procedure which introduces them: so, by assumption, they are determinately distinct from any 'thicker' object with such further properties. Why not?

So now I'm puzzled. For Parsons, does 'the natural number two' have a fixed reference to a sui-generis 'thin' role-object (or Dedekind abstraction, if that's different), or a contextually shifting reference to a role-filler? Or perhaps both? The latter seems the charitable reading. But it would have helped a lot if Parsons had much more explicitly related his position to an articulated metaphysics of role/role-filler structuralism. Elsewhere, he writes that "the metaphysical tradition is likely to be misleading as a source of ideas about the objects of modern mathematics".[10] Perhaps so. But his own metaphysical musings here seem to be insufficiently worked through to be entirely helpful.

4 A problem about sets

The title of Parsons's book is *Mathematical Thought and Its Objects*. But much of the book is in fact about *arithmetical* thought and *its* objects. And there are of course important strands of thought about the foundations of mathematics according to which what goes for arithmetic doesn't go for more wildly infinitary mathematics. Thus defenders of latter-day versions of Hilbert's programme or of Weyl's predicativism – e.g. those who think that ACA_0 marks a boundary to respectable 'contentual' mathematics – will want to insist that a good metaphysical/epistemological story about at least some arithmetics (e.g. up to predicative second-order arithmetic) needn't be be expected to carry over to deal with those areas of mathematics that can't be seen as arithmetic in disguise. And they will consequently insist that the fact that a certain metaphysical/epistemological story *doesn't* work for full-blown set theory is not in itself a reason to reject it as a story about arithmetic.

Now, this present short chapter – a slightly expanded version of a 1995 paper in a festschrifft for Ruth Barcan Marcus – considers whether there are special problems giving a broadly structuralist account of set theory. However, even if there *are* such problems, it's not clear – given what I've just said – that that counts against a structuralist theory of arithmetic, Parsons's main concern. And further, since the last section of the previous chapter left me puzzled about what, exactly, Parsons counted as a structuralist view, I'm not entirely sure I have the issue in sharp focus anyway. But with those caveats, let's proceed!

§19 An objection To revisit earlier thoughts, it is perhaps clear enough what the problem is for the gung-ho across-the-board *eliminative* structuralist about all mathematics (whether or not he modalizes). His idea is that an ordinary mathematical claim

[10] This is from Parsons's 2004 paper 'Structuralism and Metaphysics'.

A is to be read as disguising a quantified claim of the form for all ..., if $\Omega(\ldots)$ then $A^*(\ldots)$, where Ω is an appropriate set of axioms for the relevant mathematical domain, A^* is a suitable formal rendering of A, and where the quantification is over kosher non-mathematical whatnots, and perhaps possible world indices too. This account escapes making A vacuously true only if Ω is satisfied somewhere (at some index). Now if Ω is suitably modest – axioms for arithmetic say – we might conceive of it being satisfied by some physical realization at this (or at least, at some not-too-remote) world. But if Ω is a rich set theory, then it doesn't seem anywhere near so plausible to say that the relevant structure is realized somewhere. Unless, that is, we allow into our possible worlds *abstracta* to do the job – in which case the point of the *eliminative* structuralist manoeuvre is undermined.

But suppose we do want to endorse ZFC, and yet remain broadly structuralist. Even if we eschew eliminativist ambitions, and allow abstracta, presumably the structuralist thought will be at least that there isn't a given unique universe of determinately identified objects, the pure sets, which set theory aims to describe. And it might be argued that *this* thought runs against the motivating stories told at the beginning of typical set theory texts, for two suggested reasons:

1. The first reason isn't made explicit in Parsons, but is perhaps hinted at. Assume still that we are dealing with the theory of pure sets (without *urelemente*). So now take the empty set – and isn't *that*, at any rate, determinately unique? Now form its singleton (unique again). Next form all the different possible sets whose members are what we have already. Next do that again at the next level. Keep on going .... This iterative conception is a familiar one, and seems (or so the authors of many texts apparently suppose) to fix a *unique* universe, at least at up to any given height of the hierarchy.

2. More explicitly, Parsons notes the standard view that at each stage the sets formed are in some sense totally "constituted" out of their elements – and this seems to "give the membership relation some additional content, still very abstract but recognizably more than a pure structuralism would admit."

Neither (1) nor (2), however, seems at first blush particularly telling as a consideration against a structuralist view of sets. We could imagine resisting the claim in (1) that the empty set is determinately fixed if read, not as a claim about its place as a role in the structure of sets, but as a metaphysical claim that it has only one possible realization as a role-filler. Can't we just choose *any* non-set to play the role of being the empty set, and then build up from there? (We might even imagine Parsons himself remarking, given what he said in the previous section, that "the question arises what is to prevent us from later, for some specific purpose, affirming the identity of the empty-set with some object given otherwise".) And as to (2), it is initially pretty unclear what the "constitution" claim comes to over and above the principle of extensionality, i.e. the principle that the identity of a set is entirely fixed by giving its members.

Still, Parsons thinks that the thoughts (1) and (2) can perhaps be pushed harder. The worry is that without the richer non-merely-structural concept of sets grounded in some notion of a set being "constituted" by its already "available" members, "we cannot know that set theories describe coherent possibilities". So, to make out the non-vacuity of set-theory, we need a notion of set "where a set may still be ontologically impoverished compared to a concrete object but it is still more than structurally determined. We might call this view the 'ontological' conception of the objects of set theory, as opposed to the structuralist one."

This, then, is the line of thought which Parsons wants to focus on in the present chapter.

§§20–23 Ontological conceptions of set, the iterative conception, and the axioms Informal characterizations of sets have used three basic ideas – the idea of a collection (meaning "an object that consists of or is constituted by its elements"), the idea of a plurality, the idea of extension. But talk of pluralities, Parsons suggests, either involves the idea of a collection again or (construed Boolos-style) doesn't introduce new ontology. And he is not minded to put any separate weight on the idea of extensions either. So we are left with the idea of a sort of object which is "constituted by its elements".

What does that mean? Parsons says surprisingly little, though he suggests that this goes with the familiar idea that a set is formed from objects that are already "available" or "given". But those actually look like epistemic ideas, and isn't what we want here some kind of metaphysical notion? The thought must be that sets are in some sense asymmetrically *metaphysically dependent* on their elements.[11] But Parsons surprisingly doesn't really pursue this sort of suggestion – despite its being at the root of the objection to structuralism about sets which he is considering.

But even if some notion of metaphysical dependency is in good order, the iterative story remains problematic (according to Parsons). For a start, the metaphors of "forming" sets at "levels" perhaps don't bear the weight that is put on them: "when we come to [a set] of sufficiently high rank, it is difficult to take seriously the idea that all the intermediate sets that arise in the construction of this set ... can be formed by us". And then there are problems wrapped up in the temporal metaphor of "keeping on going", when the relevant ordinal structure we are supposed to grasp is much richer than that of time. Further, it is aguable that additional thoughts, over and above the basic conception of an iterative hierarchy, are needed if we are indeed to underpin *all* the axioms of ZFC – that's plausibly the case for replacement, and possibly even for the full powerset axiom (see Parsons's §§22, 23).

I'm not going to explore Parsons's arguments further here. For the idea that the iterative story is problematic and doesn't get us everything we want in a uniformly principled way is by now a familiar, though contended, one.[12] Let's suppose for present purposes that Parsons is right. What then?

Parsons writes that his "discussion of the arguments that are actually in the literature should make plausible that there is not a set of persuasive, direct and 'intuitive' considerations in favour of the axioms of ZF that are incompatible with a structuralist conception of what talk of sets is." But, by the end of his chapter, that surely seems far too sanguine – indeed, to get things upside down. For it isn't that there are multiple lines of thought in the literature which, each taken separately, clearly give us a conception of some structure that satisfies the ZF axioms (first or second order), indicating – perhaps – the kind of multiple realizability that is grist to the structuralist argument. No, the putative worry is that *no* single familiar line of thought (neither the iterative conception, nor the idea of 'limitation of size', not to mention e.g. the ideas shaping theories like

[11] See on this idea Michael Potter's *Set Theory and Its Philosophy*.

[12] There are interesting and important discussions by George Boolos, Alex Paseau, Michael Potter and others, and I don't have anything to add.

NF with a universal set) warrants all the axioms. So it isn't, after all, clear that we *do* have a unified intuitive grasp of any structure that satisfies the axioms. Hence, the worry continues, for all we really know maybe there is *no* structure that satisfies them.

Which seems to take us back, with a vengeance, to vacuity worries for structuralism as applied to set theory.

5 Intuition

I should now declare an interest here – or perhaps better, *lack* of interest. I've never really understood talk about intuition (at least, when intuition is supposed to function as some kind of distinctive, and perhaps especially authoritative, source of knowledge). And frankly, I'm not greatly helped either when Parsons writes "I shall be concerned to develop a conception of mathematical intuition that is in a general way Kantian", since Kant is pretty much a closed book to me. So I confess that I rather struggled in this chapter.

§§24, 25 Intuition and perception Parsons starts by distinguishing supposed *intuition of* objects from *intuition that* such-and-such is the case. And he stresses that in his usage, *intuition that* isn't factive. So is an intuition that such-and-such just a non-inferential belief? Well note, for example, that we might hold that we have non-inferential beliefs in e.g. very basic logic truths: but, Parsons suggests "the intuitive is contrasted not just with what is inferred but also with the conceptual" (but does that mean that *intuitions that* should have non-conceptual content? – he doesn't say). And what about "knowledge without observation" of our own bodily movements? This might be argued to have non-conceptual 'analog' content, but seems *not* normally counted to be intuitive. So what differentiates intuition properly so-called?

Well, Parsons writes, "It is hard to see what could make a cognitive relation to objects count as intuition [i.e. intuition *of*] if not some analogy with perception" (cf. e.g. Gödel). Further, he claims that intuition *that* is intimately connected with intuition *of*, rather as perception *that* is surely grounded in perception *of*. So, intuition is quasi-perceptual. But then, so elucidated, an intuitive grasp of mathematical truths would seem to have to based on some quasi-perceptual intuition of mathematical objects. And *this* is highly puzzling idea. Not surprisingly, Parsons immediately turns to consider . . .

§26 Objections to the very idea of mathematical intuition Start with the following point. Ordinary perception is (so to speak) evident to the subject – when I see an object, e.g. my computer screen, "there is a phenomenological datum here". But "it is hard to maintain that the case is the same for mathematical objects . . . [Are] there any experiences we can appeal to in the mathematical cases that are anywhere near as indisputed as my present experience of seeing the computer screen?" The answer is surely 'no', and this seems to undermine any alleged analogy between "intuition of mathematical entities" and ordinary perception. So how are we to defend the analogy, given the radically different phenomenologies? Unfortunately, Parsons next remarks here are Kantian obscurities I can do nothing much with: though in fact Parsons himself doesn't seem to find them particularly helpful either.

There's a further difficulty, Parsons thinks, for the notion of intuition of mathematical objects on the sort of structuralist view that he wants to defend. How can an intuition latch on to a particular object, if mathematical objects are (as it were) 'thin', lacking intrinsic properties other than those fixed by their places in structures. In fact, he's later going to argue that it can't. And even if the objects are not entirely thin because quasi-concrete, such 'intuitions' as we might be supposed to have *seem* to be of their representatives and not of the abstract objects.

Which all leaves me initially stumped as to how to we are going to be able to find much room for the idea of 'intuition' of mathematical objects of any kind. But Parsons, for all that, hopes to offer us a way forward in the next section.

§27 Toward a viable concept of intuition: perception and the abstract This section is intended to soften us up for the idea that we can, after all, have intuitions of abstracta (where these intuitions are somehow quasi-perceptual). But although the section is quite long, it is surprisingly thin on content.

The section starts by backtracking, and reminding us why we might be tempted to suppose (despite the emerging difficulties) that there might be room for a notion of intuition – basically, the claim is that quasi-empiricist accounts of mathematics of a Quinean stamp don't account for the immediate obviousness of elementary mathematical truths. (Is that a good objection? Why shouldn't the Quinean allow that some beliefs 'remote from the periphery', 'having a high degree of theoreticity', are so imbued in us by our education and training, are so entrenched and so resistant to replacement by alternatives, that they strike us as inevitable, as obvious, indeed as 'intuitive': the Quinean just insists that these features aren't marks of a special logical status.)

Next, after that motivational aside, there's a puzzling, and quite inconclusive discussion of supposed intuitions of colours qua abstract objects. But Parsons himself sets this case aside as raising too many complications, so I will too.

Which leaves us in the end with just one supposedly telling illustration of how we might have quasi-perceptual access to abstracta, the putative case of perceptions (or intuitions) of abstract types such as letters. The claim is that "the talk of perception of types is something normal and everyday".

But hold on! It is not enough to remark that we *talk* of e.g. seeing types: we'd need to argue, surely, that we can take our talk here at face value, as indeed reporting a (quasi)-perceptual relation to types. Here I am, looking at a squiggle on paper: I immediately see it as being, for example, a Greek letter phi. And we might well say: I see the letter phi written here. But, in this case, 'perception of the type' is surely a matter of perceiving the squiggle as a token of the type, i.e. perceiving the squiggle and taking it as a phi.

Now, it would be wrong to say that – at an experiential level – 'seeing as' just simply factors into a perception and the superadded exercise of a concept or of a recognitional ability. When the aspect changes, and I see the lines as a duck rather than a rabbit, at some level the content of my conscious perception itself, the way it is articulated, changes. Still, in seeing the lines as a duck, it isn't that there is *more* epistemic input than is given by sight (visual engagement with a humdrum object, the lines) together with the exercise of a concept or of a recognitional ability. Similarly, seeing the squiggle as a token of the Greek letter phi again doesn't require me to have some epistemic source over and above ordinary sight and conceptual/recognitional abilities. There is no need, it seems, to postulate something *further* going on, i.e. quasi-perceptual 'intuition' of the type.

Let's be clear here. (1) It may well be that, as a matter of the workings of our cognitive psychology, we recognize a squiggle as a token phi by comparing it with some stored template. But that of course does not imply that we need be able, at the personal level, to bring the template to consciousness: and even if we were to have some quasi-perceptual access to the *template* itself, it wouldn't follow that we have quasi-perceptual access to the *type*. Templates are mental representations, not the abstracta represented.

(2) Apart from worries about its being a possibly misleading way of speaking, I'm not objecting to introducing talk of 'intuiting' a quasi-concrete type where that can be construed, in a deflationary way, as involving perceiving (or imagining) a concrete token as being of a certain type. The question is whether there is any need for a richer notion of 'intuition' here, as Parsons seems to hold.

§28 Hilbertian intuition So, to summarize, the case of the perception of tokens as being of a type doesn't seem enough to soften us up for any idea of intuition of abstracta as a distinctive potential source for mathematical knowledge. Or at least, the deflationary story we've given about the perception of types is the obvious counter to Parsons's apparent line in the previous section.

But Parsons now offers a critique of our story, when he turns to consider the particular case of the perception of expressions from a very simple 'language', containing just one primitive symbol '|' (call it 'stroke'), which can be concatenated. The deflationary reading

> does not accurately render our perceptual consciousness of strokes. It would make what I want to call intuition of a string an instance of seeing a certain inscription *as* of a type .... But in actual cases, the identification of the type will be firmer and more explicit that the identification of any physical inscriptions that is an instance of the type. That the inscriptions are real physical objects with definite physical properties plays no role in the mathematical treatment of the language, which is what concerns us. An illusory presentation of a string, provided it is sufficiently clear, will do as well to illustrate a mathematical notion as a real one.

Well, there seem to be two points here, neither of which defeats the deflationist.

The first point is that the identification of a squiggle's type may be 'firmer and more explicit' than our determination of its physical properties as a token (which I suppose means that a blurry shape, seen through a glass darkly, may still definitely be a letter phi). But so what? Suppose we have some discrete conceptual pigeon-holes, and have reason to take what we see as belonging in one pigeonhole or another (as when we are reading Greek script, primed with the thought that what we are seeing will be a sequence of letters from forty eight upper and lower case possibilities). Then fuzzy tokens can get sharply pigeonholed. But there's nothing *here* that the deflationist about seeing types can't accommodate.

The second point is that, for certain illustrative purposes, illusory strings are as good as physical strings. But again, so what? Why shouldn't seeing an illusory strokes as a string be a matter of our tricked perceptual apparatus engaging with our conceptual/recognitional abilities? Again, there is certainly no need to postulate some further cognitive achievement, 'intuition of a type'.

Oddly, Parsons himself, when wrestling with issues about vagueness, comes close to making these very points. For you might initially worry that intuitions which are "founded" in perceptions and imaginings will inherit the vagueness of those perceptions or imaginings – and how would that then square with Parsons's claim that "mathematical intuition is of sharply delineated objects"? But Parsons moves to block the worry, using the example of seeing letters again. His thought now seems to be mine above, that we have some discrete conceptual pigeon-holes, and in seeing squiggles as a phi or a psi (say), we are pigeon-holing them. And the fact that some squiggles might be borderline candidates for putting in this or that pigeon-hole doesn't (so to speak) make the pigeon-holes less sharply delineated. Well, fair enough. But thinking in these terms surely does not in fact sustain the idea that we need some basic notion of the "intuition of" the type phi to explain our pigeon-holing capacities.

So, at this point then, I'm still quite unpersuaded that we actually need (or indeed can make much sense of) any notion of the quasi-perceptual 'intuition of types' – and in particular, any notion of the intuition of types of stroke-strings – that resists a deflationary reading. But let's suppose for a moment that we follow Parsons and think we *can* make sense of such a notion. Then what use does he want to make of this idea of intuiting stroke-strings?

Well, despite its title, the present section doesn't take things very far. But Parsons does write

> What is distinctive of intuitions of types [here, types of stroke-strings] is that the perceptions and imaginings that found them play a paradigmatic role. It is through this that intuition of a type can give rise to propositional knowledge about the type, an instance of intuition that. I will in these cases use the term 'intuitive knowledge'. A simple case is singular propositions about types, such as that ||| is the successor of ||. We see this to be true on the basis of a single intuition, but of course in its implications for tokens it is a general proposition.

This passage raises a couple of issues worth commenting on. One concerns the way that the notion of 'intuitive knowledge' is first introduced here, as the notion of propositional knowledge that arises in a very direct (and non-inferential?) way from intuition(s) of the objects the knowledge is about. Such a notion looks very restrictive – there presumably won't be much intuitive knowledge to be had. We'll soon be returning to this point.

The other issue concerns the claim that there is a 'single intuition' here on basis of which we see that that ||| is the successor of ||. I can think of a few cognitive situations which we might agree to describe as grounding quasi-perceptual knowledge that ||| is the successor of || (even if some of us would want to give a deflationary construal of the cases, one which doesn't appeal to intuition of abstracta). For example,

1. We perceive an array of two stroke-strings
   ||
   |||
   and imagine the upper one sliding down over the lower one, leaving us with a stroke left over.

2. We perceive a single sequence of three strokes ||| and flip to and fro between seeing it as a threesome and as a block of two followed by an extra stroke.

But, even going along with Parsons on intuition, neither of those cases seems aptly described as seeing something to be true on the basis of a *single* intuition. In the first case, don't we have an intuition of ||| and a separate intuition of || plus an intuitive(?) recognition of the relation between them. In the second case, don't we have successive intuitions, and again a recognition of the relation between them? It seems that our knowledge that ||| is the successor of || is in either case grounded on intuitions, plural, plus thoughts about their relation. And now the suspicion is that it is the *thoughts* about the relations that really do the essential grounding of knowledge here (generalizing thoughts that could as well be engaging just as well with perceived real tokens or with imagined tokens, rather than with putative Parsonian intuitions that, as it were, reach past the real or imagined inscriptions to the abstracta).

§29 Intuitive knowledge: a step toward infinity The strings of the stroke-string 'language' introduced in the last section can be put into an ω-sequence, giving us a structure isomorphic to the natural numbers. But even if we grant that (some) individual stroke-strings are intuitable, the question remains what aspects of this structure are 'intuitively' knowable.

Well, note again that the notion of 'intuitive knowledge' was introduced in the last section in such a way that "an item of intuitive knowledge would be something that can be 'seen' to be true on the basis of intuiting objects that it is about" – we just 'saw' that ||| is the successor of ||. But Parsons now wants to extend the notion in two ways. First

> Evidently, at least some simple, general propositions about strings can be seen to be true. I will argue that in at least some important cases of this kind, the correct description involves imagining *arbitrary* strings. Thus, that will be included in 'intuiting objects that a proposition is about'.

But even if we now allow intuition of 'arbitrary objects', that still would seem to leave intuitive knowledge essentially non-inferential. And so restricted, there will perhaps be rather little such knowledge.

> I do not wish to argue that the term 'intuitive knowledge' should not be used in that [restrictive] way. Our sense, following that of the Hilbert School, is a more extended one that allows that certain inferences preserve intuitive knowledge, so that there can actually be a developed body of mathematics that counts as intuitively known. This seems to me a more interesting conception, in addition to its historical significance. Once one has adopted this conception, one has to consider case by case what inferences preserve intuitive knowledge.

Two comments about all this. Take the second proposed extension first. That's perhaps fine as a programmatic suggestion. But the obvious question to ask is: *what will constrain our case-by-case considerations of which kinds of inference preserve intuitive knowledge*? To repeat, the concept of intuitive knowledge was introduced by reference to an example of knowledge seemingly non-inferentially obtained. So how are we supposed to 'carry on', applying the concept now to inferential cases? It seems that nothing in our original lesson tells us which such further applications are legitimate, and which aren't. But there must be *some* constraints here if our case-by-case examinations are not just to involve arbitrary decisions! So what are they? I struggle to find any clear explanation in Parsons. We'll return to this matter when discussing Chapter 7.

What about intuiting 'arbitrary' strings? How does this ground the knowledge that every string has a 'successor'? Well, supposedly, (1) "If we imagine any [particular] string of strokes, it is immediately apparent that a new stroke can be added." (2) But we can "leave inexplicit its articulation into single strokes", so we are imagining an arbitrary string, and it is evident that a new stroke can be added to this too. (3) "However, ... it is clear that the kind of thought experiments I have been describing can be taken as intuitive verifications of such statements as that any string of strokes can be extended only if one carries them out on the basis of specific concepts, such as that of a string of strokes. If that were not so, they would not confer any generality." (4) "Although intuition yields one essential element of the idea that there are, at least potentially, infinitely many strings . . . more is involved in the idea, in particular that the operation of adding an additional stroke can be indefinitely iterated. The sense, if any, in which intuition tells us that is not obvious." But (5) "Once one has seen that every string can be extended, it is still another question whether the string resulting by adding another symbol is a different string from the original one. For this it must be of a different type, and it is not obvious why this must be the case. . . . Although it will follow from considerations advanced in Chapter 7 that it is intuitively known that every string can be extended by one of a different type, ideas connected with induction are needed to see it."

There's a lot to be said about all this, though (4) and (5) already indicate that Parsons thinks that, by itself, 'intuition' of stroke-strings won't take us terribly far. But does it take us even as far as Parsons says? For surely it is *not* the case that imagining/intuiting adding a stroke to an inexplicitly articulated string, together with the exercise of the concept of a string of strokes, suffices to give us the idea that any string can be extended. Can't we conceive, at least in principle, of a particularist reasoner, who has the concept of a string, can bring various arrays (more or less explicitly articulated) under that concept, and given a string can recognize that *this* one can be extended – but who can't advance to frame the thought that they can *all* be extended? The generalizing move surely requires a further thought, not given in intuition.

Indeed, we might wonder quite what the notion of intuition is doing here at all. For note that (1) and (2) are a claims about what is *imaginable*. But if we *can* get to general results about extensibility by imagining particular strings (or at any rate, imagining strings "leaving inexplicit their articulation into single strokes", thus perhaps || . . . || with a blurry filling) and then bring them under concepts and generalizing, why do we also need to think in terms of having cognitive access to something *else* which is general, i.e. stroke-string types? It seems that Parsonian intuitions actually drop out of the picture.

Another point to highlight, for future discussion, is that the thought that ideas "connected with induction" can still be involved in what is 'intuitively known" is evidently going to be problematic. What integrity is left to the notion of intuitive knowledge once it is no longer tightly coupled with the idea of some quasi-perceptual source and allows even non-logical inference to preserve intuitive knowledge? We'll return to this worry.

§30 The objections revisited The final section in Parsons's chapter on intuition aims to revisit the worries in §26 about the very idea of intuition. But I found this rather opaque.

But one thing I became clearer about is that Parsons resists what I called the deflationist view of 'intuition of types' because he aligns it with a nominalist position which regards types as equivalence classes of tokens (and he thinks issues e.g. about vagueness cause grave difficulty for that position). But surely that's wrong: the issue here concerns epistemology, not ontology. The deflationist idea is that seeing the type phi instantiated on the page is a matter of seeing the written squiggle as a phi – and this involves bring to bear the concept of an instance of phi. And, the suggestion continues, having such a concept is *not* to be explained in terms of a quasi-perceptual cognitive relation with an abstract object, the type. If anything it goes the other way about: 'intuitive knowledge of types' is to be explained in terms of our conceptual capacities, and is not a further epistemic source. But note, the deflationist who resists the stronger idea of intuition as a distinctive epistemic source isn't barred from taking Parsons's permissive Fregean line on objects, and can still allow the introduction of talk via abstraction principles of abstract objects such as types. He needn't have a nominalist horror of talk of abstracta.

6 Numbers as objects

§31 What are the natural numbers? We are now about halfway through Parsons's book. So where have we got to? We have yet to see how much arithmetic is accessible to 'intuition': but have we perhaps made some progress on the question of the nature of the numbers themselves?

"One part of an answer to the question what the natural numbers are is quite uncontroversial. They are a structure satisfying the Dedekind-Peano axioms . . . a 'progression'." Actually, if we are going to be pernickety, we might wonder how uncontroversial the claim is when put quite like *that*. It looks like a category mistake just to say that some objects (the numbers) *are* a structure. It's objects together with relations and/or functions between them that make for a structure. However, let that pass.

Still, asks Parsons, "might we distinguish one progression as being *the* natural numbers, or at least uncover constraints such that some progressions are eligible and others are not?".

> The structuralist view of numbers explored in Chapters 2 and 3 offers an answer to our question. If we are prepared to help ourselves to second-order logic, then the objection made in §17 to the eliminative version of structuralism is not fatal in the particular case of natural numbers, and the modal version has been worked out thoroughly in Hellman's *Mathematics without Numbers*. The non-eliminative version proposed in §18, however, has the advantage of not helping itself to second-order logic, and it seems to me to do better justice to the actual role of second-order principles in mathematics. Could we simply end our enquiry at this point and say that we have said what reference to the natural numbers is?

At the end of our earlier discussion of §18, however, we were in fact left puzzled about the exact position being defended there. Further – revisiting that section – I'm not clear either why Parsons now thinks that non-eliminative structuralism "does better justice" to the way we actually use second-order principles in mathematics (that's not a thought that was highlighted before). But again, let that pass. For Parsons himself argues that we need to say more about the nature of numbers: in brief, "our discussion of the natural numbers will be incomplete so long as we have not gone into the concepts of cardinal and ordinal".

So, let's proceed: cardinals first . . .

§32 Cardinality and the genesis of numbers as objects This section outlines a project which is close to my heart – roughly, the project of describing a sequence of increasingly sophisticated arithmetical language games, and considering just what we are committed to at each stage. (As Parsons remarks, "The project of describing the genesis of discourse about numbers as a sequence of stages was quite foreign to [Frege]" – yet such projects, I agree, can be highly illuminating.)

We start, let's suppose, with a grasp of counting and a handle on 'there are n Fs'. It would seem over-interpreting to suppose that, at the outset, grasp of the latter kind of proposition involves grasping the second-order thought 'there is a 1-1 correspondence between the Fs and the numerals from 1 to n'. Parsons – reasonably enough – takes 'there are n Fs' to carry no more ontological baggage than a first-order numerical quantification '$\exists_n xFx$' defined in the familiar way. Does that mean, though, that we are to suppose that counting-numerals enter discourse as indices to numerical quantifiers? Even if ontologically lightweight, that *still* seems conceptually too sophisticated as an account of our entry-point into numerical talk. And in fact Parsons has a rather attractive little story that introduces numerals as special *demonstratives* (in counting the spoons, I point to them in turn, saying 'one', 'two', 'three' and so on), and then takes the competent counter as implicitly grasping principles which imply that, if the demonstratives up to n are correctly applied to all the Fs in turn, with no double-counting, then it will be true that $\exists_n xFx$.

So far so good. But thus far, numerals refer (when they do refer, in a counting context) to the objects being counted, and then recur as indices to quantifiers. Neither use refers to *numbers*. So how do we advance to uses which are (at least prima facie) apt to be construed as so referring?

Well, here Parsons's story gets far too sketchy for comfort. He talks first about "the introduction of variables and quantifiers 'ranging over numbers'" – with the variables replacing quantifier indices, and where the quantifiers can initially be construed substitutionally. But how are we to develop this idea? He mentions Dale Gottlieb's book *Ontological Economy* and also refers to the approach to substitutional quantification of Kripke's well-known paper:[13] but he doesn't develop the idea here. And then there's the key step – as Parsons himself notes – of moving from a story where number-talk need only be construed substitutionally to a story where numbers do genuinely appear as objects, objects that themselves are available to be counted. So, as he asks, "in what would this further conceptual leap consist?". A crucial question: but one that Parsons singularly fails to answer (see the middle para on p. 197). Another issue concerns *at what stage* in the sequence of language games and *how* a commitment to an infinity of natural numbers is taken on. This too is a crucial question left unanswered (except for some too-short remarks in §36). So this section is at best merely programmatic – though as I said, the programme is indeed an important and illuminating one.

§§33, 34 Finite sets, finite sequences, and intuitions of them As we saw, the previous section outlines rather incompletely one sequence of increasingly sophisticated but purely arithmetical language games. §33 considers a different way of introducing numbers, by first giving a theory of hereditarily finite sets, and then explaining how a theory of numbers can naturally be implemented as an adjunct to such a theory.

Parsons, then, outlines the required theory of hereditarily finite sets, taking as basic the membership relation and the dyadic function $x+y$ (intuitively, $x \cup \{y\}$). The resulting theory proves the axioms of ZF without infinity and foundation. I won't reproduce it here. In such a theory, we can define a relation $x \sim y$ that holds between the finite sets x and y when they are equinumerous. We can also define a 'successor' relation between sets along the following lines: Syx iff $(\exists z)(z \notin x \land y \sim x + z)$.

[13]But aren't Gottlieb's and Kripke's treatments significantly different?

We can next conservatively add (finite) 'cardinal numbers' to our theory by introducing a functor **C**, using an abstraction axiom $\mathbf{C}x = \mathbf{C}y$ iff $x \sim y$ – so here, Parsons says, "numbers are types, where the tokens are sets and the relation $\sim$ is that of being of the same type". Then we can define a successor function on cardinals in terms of the non-functional relation S in the obvious way (and go on to define addition and multiplication too).

Well, so far so good. But quite how far does this take us? We'd expect the next step to be a discussion of just how much arithmetic can be constructed like this. For a start – another key question – are we entitled cheerfully to quantify over these newly defined cardinals? We don't get the answer here, however. Which is disappointing. Rather Parsons first considers a variant construction in which we start not with the hierarchical structure of hereditarily finite sets but with a 'flatter' structure of finite sequences, and where we introduce ordinals to 'count' these sequences. But I'm not sure anything much is gained by this diversion. And then – in §34 – he turns to consider whether one or other story about grounding an amount of arithmetic in the theory of finite sets/sequences might give us reveal a way that arithmetic can get an intuitive grounding via intuitions of sets.

Well, I suppose we can indeed wonder whether we "might reasonably speak of intuition of finite sets under somewhat restricted circumstances" (i.e. where we have the right kinds of objects, the objects are not too separated in space or time, etc.). And Penelope Maddy, for one, did at one stage argued that we can not only intuit but perceive some such sets – see e.g. the set of three eggs left in the box. But in fact, while it may be the case that we, so to speak, take in the eggs in the box as a threesome, that fact in itself gives us no reason to suppose that this cognitive achievement involves "seeing" something other than the eggs (plural). As Parsons remarks, "it seems to me that the primary elements of a story [to rival Maddy's] would be the capacity to classify what one sees . . . and to recognize identities and differences" – capacities that could underpin an ability to judge small numerical quantifications at a glance, and "it is not necessary [at least to explain knowledge such as that there are three eggs in the box] to attribute to the agent perception or intuition of a set as a single object". I agree.

§35 Intuition of finite sets This section is something of an aside from the discussion of numbers. But suppose we accept the conclusion of the last section, that we don't need a notion of intuiting sets in order to ground arithmetical beliefs about the number of things in a collection. Still, we might wonder whether intuitions of sets-as-objects might be needed to serve at least to "give an intuitive foundation to theories of finite sets". But Parsons finds problems with this suggestion too.

One difficulty can be introduced like this. Suppose I perceive the following array:

```
$$$$$$
```

Then do I 'intuit' six dollar signs, a single set of six dollar signs, a set of three elements each a set of two signs, or even a set containing the empty set together with a set of six signs? Which way do I 'bracket things up'?

```
$$$$$$
{$$$$$$}
{$$}{$$}{$$}
{}{$$$$$$}
```

The possibilities are many – indeed literally endless, if we are indeed allowed the empty set and sets containing it (and what are our intuitions of *those*?). So it seems that the "intuition" here has to involve some representational ingredient to play the role of the brackets in the various possible bracketings. But then we seem to losing our grip on any putative analogy between intuition and perception (as Parsons himself puts it, "in a perceptual situation involving the application of certain concepts, we not expect that a linguistic or other embodiment of the concepts should be perceptually present in that very situation").

Here's a second difficulty. Start by noting that we can in fact give a theory of those "bracket terms" which we've just been using – terms putatively for hereditarily finite sets constructed from a given domain D of individuals – which uses a relative substitutional semantics. That is to say, we can start with a first-order language for which D is the domain, add terms as if for hereditarily finite sets of elements from D, and variables and quantifiers for them, which we then interpret substitutionally relative to D. Parsons spells this out in an Appendix, but the general idea will be familiar to readers of his old paper on 'Sets and Classes'. And the upshot of this, Parsons says, "is that if we take the relative substitutional semantics as capturing a speaker's understanding of the language of hereditarily finite sets . . . then we largely remove the motives for characterizing awareness of such sets as initution". The discussion here isn't ideally clear – but the point seems a good one.

Note that this isn't to say that we have entirely eliminated a role for intuition. For even on the relative substitutional interpretation we still need the idea of sequences of individuals from D. And we might suppose that *that* notion is grounded in intuition. But even if true, Parsons still seems right in holding that this still falls well short of the original thought that intuitions of sets-as-objects are what underpin our grasp of theories of finite sets.

§36 Well, then, what are the numbers? Structuralism put in its place The present chapter started with Parsons reiterating his commitment to a non-eliminativist version of structuralism. But now we've met, at least programmatically, a couple of just-so stories about the genesis of number-talk. The first goes through a series of language games starting with games involving numerals-as-demonstratives and numerals-as-quantifier-subscripts and works up, via the addition of substitutional quantifiers, to a fully referential language involving objectual quantifiers; the second starts from a theory of hereditary finite sets, and adds number talk to that. The obvious question is: what's the relation between the official structuralism and these two just-so stories?

Well, Parsons says of the first story:

> The account has the feature that numbers first arise as objects of a distinctive type. That means that without being structuralist, the account is congenial to structuralism because it introduces numbers in a way that at least leaves entirely open the question what identities might hold between numbers and other objects."

But recall, Parsons hasn't spelt out in any detail how the first story is to go. I suppose that one way it could be developed would have us saying, at a certain stage, 'And now, let's postulate some entities – call them pro tempore "numbers", without prejudice as to what more familiar things they might turn to be – which satisfy the following conditions . . . .' So indeed, we then leave it open "what identities might hold between numbers and other objects". But we can also and perhaps more plausibly imagine the first story being pursued e.g. in a logicist spirit, where at a certain stage rules for introducing and eliminating number-terms are given, and where (just as there is no more content to other 'logical' expressions than is revealed in their introduction and elimination rules) there is supposed to be no more to the numbers that is settled thereby – a thought, of course, very much in keeping with that other structuralist idea that numbers "have no identity or features" other than those given by specifying the kind of structure they form.

So Parsons's quoted remark about the first just-so story is surely far too quick – for it is far from obvious that this story does have to leave it open what the numbers are. Likewise for the second story. For according to *that*, terms for numbers are introduced by an abstraction principle: and it is, to say the least, again far from obvious that abstracta so introduced can be identified with other types of objects. But we've touched on such matters before, in discussing §18, and I won't repeat the point.

Parsons adds:

> However, common to all the versions ... is the following: We have choices as to whether to proceed so that terms and variables for numbers are of the same type as others, and, assuming that mature mathematics does involve regarding numbers as objects among others, at what point to unify the type of numbers with others. This implies that the questions giving rise to the structuralist account of mathematical objects can be postponed until far along in the genesis of discourse about numbers. ... The place of a structuralist view of numbers is as an account of numbers in mature mathematics, and we need not assume that such an understanding of numbers is what the child learns or what our distant ancestors acquired over a long period of time.

But this seems to be doubly misguided. First, Parsons is seemingly over-impressed (here as before) by some mathematicians' casual practice of apparently identifying across types – e.g. identifying numbers with sets[14] – and he seems to want to frame his account of numbers in such a way as to preserve this way of talking as legitimate. Yet many mathematicans are insistent on talking of the mathematical universe as being 'strongly typed', and when on their best behaviour will speak of structure-preserving embeddings (or functors between categories, etc.) rather than ever commit themselves to cross-type identities. The philosopher of mathematics, commenting from the sidelines, doesn't need to be as accepting as Parsons seems to be of apparent identifications across types – and we don't need to adopt structuralism just to accommodate them. But second, we presumably *do* need an account of what is going on in apparently object-referring number talk in relatively elementary arithmetic: and the structuralist idea that numbers are 'thin', positions in a certain abstract structure, which "have no identity or features outside of [that] structure", is surely *already* attractive as an account of the commitments of immature number talk, if it is attractive anywhere. So structuralism doesn't come into play just as an account of numbers in 'mature' mathematics.

§37 Intuition of numbers denied So – Parsons asks finally in this chapter – given a structuralist view of numbers, can numbers be objects of intuition in anything like the sense developed in §28 onwards?

There's an evident problem – one already trailed. Grant for the sake of argument that we indeed might have intuitions of the elements of some progressions (if these are not

[14] In the next section, indeed, he explicitly talks of the *integration* of arithmetic with set theory.

too complex or abstract – stroke-strings, for example). Still, while an 'intuitive model' of arithmetic might therefore in some sense be possible, "the intuition in question need not be intuition of *numbers*". We surely can perceptually or quasi-perceptually only access "a structure that is physical, or mental, or intuitive-geometrical, in a way in which, on [the] structuralist view, numbers are not". To put the challenge as we did before in discussing §26: how can an intuition latch on to mathematical objects like particular numbers, if such objects are (as it were) 'thin', lacking intrinsic properties other than those fixed by their places in structures?

But note, what makes *this* line of thought rather compelling is precisely the conception here of numbers as 'thin' and correspondingly remote from anything quasi-perceptual. So it seems that here we are back to a structuralism that thinks of terms like 'the natural number two' as primarily referring to definite sui-generis objects like roles. (Yet contrast again Parsons's remark in §18 which instead characterizes his preferred kind of structuralism as holding that numbers are *not* "definite objects, in that the reference of terms such as 'the natural number two' is not invariant over all contexts" – for *that* position apparently *could* allow, in some contexts, the number term to denote some intuitable object which in the context fills the appropriate role.)[15]

7 Intuitive arithmetic and its limits

The sequence of expression-types from the Hilbertian stroke-string 'language' described in §28 gives us a model of arithmetic: as Parsons puts it, "we can easily satisfy ourselves that it satisfies the Dedekind-Peano axioms". But he wants to say more: the system of strings is in some sense an *intuitive model* of arithmetic. Why so? "Because it consists of objects of intuition in the sense that there is actual intuition of strings sufficiently early in the sequence and it is possible to draw some conclusions about an arbitrary string intuitively". But what conclusions *can* we draw intuitively? "How far does intuitive knowledge in arithmetic extend, when arithmetic is understood with reference to this model of strings?" That question is the topic of this chapter.

We'd better start by reminding ourselves of the way Parsons introduces his notion of intuitive knowledge. As first explained in §28, it seems that "an item of intuitive knowledge would be something that can be 'seen' to be true on the basis of intuiting objects that it is about" – as in the case were we supposedly just 'see' that ||| is the successor of ||. But in §29 Parsons quickly wants to extend the notion in two ways. First, as well as intuiting particular stroke strings, say, Parsons wants to allow cases which can be described as intuiting "arbitrary" strings. And second, he "allows that certain inferences preserve intuitive knowledge, so that there can actually be a developed body of mathematics that counts as intuitively known." The first extension raises issues about what counts as intuiting arbitrary objects. But our main worry in discussing the present chapter will concern the second extension. The concept of intuitive knowledge was, as we noted before, first introduced by reference to an example of knowledge seemingly non-inferentially obtained. So just how are we supposed to 'carry on', broadening the concept to cover inferential cases as well? It might seem that nothing in the way the concept was introduced tells us which such further applications are legitimate. But there must be *some* constraints here if our discussions about which inferences preserve intuitive knowledge are not just to involve arbitrary decisions. So what are they?

[15]This chapter finishes with two semi-technical appendices, numbered §38 and §39, which I won't talk about here: hence the jump below in the section-numbering.

§40 Arithmetic as about strings: finitism The question of how far intuitive knowledge in arithmetic extends of course relates intimately to discussions by Hilbert, Bernays and others about how to characterize what Hilbert called the finitary method. To make the connection, recall Gödel's remark, quoted by Parsons, that "finitary mathematics is defined as that of intuitive evidentness". Which suggests a two-way link: (1) if a proposition can be established by approved finitary methods, then it is intuitively evident; and conversely (2), if a proposition is intuitively evident, then it can be established by approved finitary methods.

Let's concentrate on (1), and combine it with the familiar thought – which Parsons in this section urges is indeed a Hilbertian thought – that at least proofs in Primitive Recursive Arithmetic are finitary. Then we get (part of) what Parsons calls 'Hilbert's Thesis', namely that proofs in PRA indeed yield intuitive knowledge. But is that thesis defensible? In particular, can we use our supposed intuitive grasp of the stroke-string model at least to show that, indeed, PRA is intuitive?

Well, given the character of PRA, whether we can do that will depend on five subtheses "interpreted with respect to the intuitive model of strings":

I1 Successor can be seen intuitively to be well defined.

I2 The elementary successor axioms can be known intuitively.

I3 In each case of introduction of a function symbol by primitive recursion, if the assumed functions have been intuitively seen to be well defined, then this is so of the new function introduced, in such a way that the recursion equations are known intuitively.

I4 Logic inference preserves intuitive evidence.

I5 Inference by induction preserves intuitive evidence.

Here, I've followed Parsons's formulations and his numbering. The subtheses (I2) to (I5) are to be taken up in the ensuing sections. Parsons, however, briskly says that at least (I1) has "effectively been argued for in §29".

But actually not really so. In §29, the argument was that, given any particular string, we can 'see' that it can be extended with an additional stroke. Grant that. Grant too that we might only be given a string rather indeterminately – which we might picture thus, $||\ldots||$ – and yet we still 'see' that the string can be extended. But it takes some conceptual thought to make the generalizing move from this or that individual case (whether determinate or vague) to seeing that we can *always* add a stroke, whatever the string, so that 'successor' applied to stroke strings is well-defined. So, at best, it is intuition of strings as types *plus* the application of generalizing concepts which gives us knowledge that successor is well defined.

But then, as before, we might begin to wonder whether intuition of *types* is actually doing any essential work in this sort of story. Won't perception or imagination of *token* strings plus generalizing thought about *them* do the work as well? Do we have to postulate a sense in which the types themselves are present to the mind, that gives us a route to new knowledge that perception/imagination of tokens plus thought can't supply? As far as I can see – though I'm not going here to revisit the discussion of Chapter 5 – Parsons has given us no good reason to suppose so. In which case, while successor might evidently be well-defined for strings – in the sense that the exercise of some imagination plus elementary reflections convinces us of its truth – there's no reason to say that it is (or can be) seen *intuitively*, if that means on the basis of a quasi-perceptual cognitive relation to types.

§41 The elementary axioms Consider next the subthesis (I2), that the elementary successor axioms can be known intuitively. The most difficult case for (I2) is presumably the axiom that different numbers have different successors: interpreted in the string model, this is the claim that if strings x and y differ, then so do their successors. Now, Parsons writes,

> What is the "successor" of a string x but something obtained by attaching one more stroke to x? This "attaching" takes places in the space outside that occupied by x, so that if x differs from y, then there is no way in which one could, by adding a stroke to each of x and y, obtain identical results. If we think of sameness of type in terms of one-one correspondence then clearly a correspondence between s and t can be extended if a | is added to each, by making these |s correspond to each other.

Now, there seem to be two different thoughts here. But the *second* one is the wrong way round (since it only shows that, if strings are of the same type, then so are their successors). I take it that what Parsons should have written instead is something like this: Suppose there is a one-one correspondence the successor of x – i.e. x plus an end-added | – with the successor of y – i.e. y plus an end-added |. By swapping a couple of pairings of elements, this can be trivially mutated into a one-one correspondence between the successors which matches the 'end-added' strokes. This correspondence then induces a one-one correspondence between x (without the end-added stroke) and y (without the end-added stroke). Which is fine, establishes the needed axiom, but doesn't on the face of it involve any direct appeal to intuition at all.[16]

Does Parsons's *first* quoted remark gives us an alternative route to intuitive knowledge that, if x and y differ, then so do their successors? Well, I found that remark a bit opaque. But, in any case, an analogue of the comment I made about seeing that the successor function is well-defined would seem to apply. Maybe we indeed can come to 'see' that different strings have different successors by manipulating strings in spatial imagination and bringing to bear generalizing conceptual thought: but it isn't at all clear why that route has to be construed as involving (in addition to imagining tokens and conceptual reflection) anything akin to a quasi-perceptual relation to abstracta.

So again, it hasn't been adequately shown that the elementary successor axioms can be known *intuitively*, if this is supposed to signal a distinctive route to knowledge dependent on an irreducible cognitive relation to types.

Perhaps, though, it will be said that I've been repeatedly fussing unduly. Perhaps Parsons would in fact be almost as happy with the claim that some arithmetic knowledge – e.g. of the elementary successor axioms – can be grounded in *imagination* plus thought (so it isn't so much the claim about Kant-style intuition as a special route to knowledge that is central for him, as the claim that more than pure reason is or can be involved). But if so, you would surely expect him to engage with the literature on arithmetic and the imagination,[17] and perhaps also the recent literature on necessary truth and the imagination more generally. He doesn't.

[16] Even if we need intuition to grasp the concepts deployed in the argument, it seems we need no further appeal to follow the argument.

[17] Starting e.g. with Edward Craig's well-known 1985 paper 'Arithmetic and Fact'.

§42 Logic and intuition Subthesis (I4) says that logical inference preserves intuitive evidence. This claim doesn't square with Parsons's assertion that "Hilbert's remarks about quantification in finitist mathematics would suggest that already reasoning in which unbounded quantifications enter into logical combinations, even within the limits of intuitionistic logic, does not preserve intuitive knowledge." But the discrepancy is merely apparent, I take it. The idea we need is just that *the amount of inference required for PRA* preserves intuitive knowledge – and that's (at most) propositional logic and laws of identity.

Yet still, even propositional logic and/or substitutions can take you from intuitively evident premisses by finitistically kosher means to quite intractably complex propositions. In just what sense can these remote consequences be *intuitively* evident? Given that the notion of intuitive knowledge seemed to be introduced on the model of a rather direct quasi-perceptual engagement with abstracta, on what principle are we now supposed to allow conclusions rather remote from their quasi-perceptual basis to also count as intuitive?

To press the point: note that in the case of *perceptual* knowledge, it is commonly said e.g. that "perceptual knowledge that p is . . . knowledge that p which arises immediately from current perception, that is, without inference from prior assumptions".[18] So understood, perceptual knowability will only be preserved under such logical inferences as take the perceiver to truths that can also be e.g. seen to be true straight-off, without conscious inference. Which presumably will be a very constrained class of inferences. Hence, if *intuitive* knowledge is quasi-perceptual, we might expect that only a similarly restricted amount of inference will preserve intuitive knowability. What principle do we use to determine how far those restrictions suggested by the analogy with perception should now be lifted?

Parsons writes:

> Some basic forms of reasoning are going to have to be allowed as preserving intuitive evidence [if any body of mathematics is to count as intuitively evident], and some will count as logical. The general principle should be that what is allowed is to be absolutely basic to reasoning in general, or to reasoning about objects or about the particular kind of objects with which we are concerned.

But that doesn't really answer our question. To be sure, if even 'absolutely basic' logic (whatever that is) doesn't count as preserving intuitive knowledge then *very* little is intuitively knowable. Parsons recommends modus tollens on that conditional – but one man's modus tollens is another's modus ponens. What's to choose? He says the use of propositional logic was held by Hilbert and Bernays to preserve intuitive knowledge because we can just compute what follows from what. But that begs the question again: why should lengthy computations deliver what is properly to be understood as *intuitive* knowledge?

Parsons also says: "[l]ogical inference has the character that, generally, no new intuition of objects is needed to attain the conclusion that was not already needed to attain the premises". But putting it that way still doesn't give him quite what he wants. For if the feature of needing no new intuitions is indeed a *general* feature of logical inference, it is a feature shared by inferences involving unbounded quantifications of the kind that Parsons says don't preserve intuitive knowledge.

[18] The quotation is from Alan Millar's 'The scope of perceptual knowledge', *Philosophy* 2000.

What to do?[19] Let's stipulatively say that knowledge is intuitively grounded if it can be inferred from the intuitive knowledge of particular objects, without appeal to further information. Then yes, at least the propositional reasoning together with substitution of identicals used in PRA will keep us within the bounds of intuitively grounded knowledge. So what Parsons says here about *intuitive knowledge* simpliciter applies at least to what I've just called *intuitively grounded knowledge.*

What's in a label? Perhaps nothing. But on the other hand, as noted before, Parsons certainly doesn't write as if – in moving from his first introduction of the notion of intuitive knowledge in §28 to then allowing intuition-plus-inference to deliver intuitive knowledge – he is merely going in for a stipulative extension of his original notion. He writes as if the same notion is in play all the way along. The best reconstruction I can offer of *why* is as follows: Parsons in fact wants to define intuitive knowability by reference not to what is quasi-perceptually graspable by us as we are, but by reference to what is in principle so graspable, allowing for arbitrary but still finite extensions of the sorts of capacities we actually have. Then what I've called intuitively grounded knowledge – even the results of long inferences – would arguably still be quasi-perceptually graspable in principle. For, arguably, any result of propositional reasoning and substitution in identities can in principle be grasped straight off, i.e. without conscious inference, if not by us then by a smarter but still only finitely improved version of us.

That reconstruction *is* very speculative, though. And you might well worry that it builds into Parsons's position aspects he'd be better off without (talking about 'in principle' extensions of our cognitive capacities can be dangerous!). So you might think that Parsons's full notion of intuitive knowledge is after all better regarded as a stipulative extension of the originally introduced quasi-perceptual notion. But no matter. I agree that the real issue for Parsons surely is: how much arithmetical knowledge can be regarded at least as intuitively grounded (in my sense)?

§43 Induction Parsons is going to return in Chapter 8 to write at length about induction in general. But for the moment, the issue is (I5) applied to strings, and the question whether induction – or at least, induction over quantifier-free predicates, which is all that we require for PRA – preserves intuitive evidence.

Parsons stresses that the question is whether *applications* of induction preserve intuitive evidence – "there is no claim to the effect that a principle of induction is intuitively evident". But then he writes:

> If we admit some conception of the domain of strings as intuitive, it seems we ought to admit induction as preserving intuitive knowledge.

Why? Parsons just doesn't say.

> What is claimed by the finitist is that instances of induction, provided that their content does not involve any non-finitist notions, become evident one at a time.

Now, you might initially worry: don't instances of inductive reasoning prove universal generalizations – and haven't we agreed that universal generalizations fall outside the reach of intuition? However, that would be a misunderstanding. For the finitist can take induction as the following schematic rule (where the filling for φ does not involve any non-finitist notions):

[19] I've been much helped here by conversations with Steven Methven and Luca Incurvati.

$$\frac{\varphi(0) \qquad \begin{array}{c}[\varphi(a)]\\ \vdots \\ \varphi(Sa)\end{array} \qquad Nt}{\varphi(t)}$$

where 'N' of course means 'is a number' (see §47 for an explicit presentation of this as the preferred form of induction). Instances of this rule take us from the particular (intuitively accessible) claim Nt to the claim $\varphi(t)$; and the thought is that this conclusion will be intuitively available too, so long as – as we required – the filling for φ is finitistically acceptable.

But is that right? Well, the inference requires us to recognize the reasoning from $\varphi(a)$ to $\varphi(Sa)$, *with 'a' a parameter*, as a correct subproof. And can *that* really count as intuitively grounded knowledge by Parsons's lights? Again, can't we conceive of what I earlier called a particularist reasoner, who grasps claims about particular objects (grounded perceptually/intuitively), and who can reason from claims about particulars to more claims about particulars, using propositional logic and the laws of identity, but who is stumped by implicitly generalizing reasoning using parameters?

Perhaps Parsons thinks that the kind of generalizing move that supposedly takes us from (i) 'seeing' that an 'inexplicitly articulated' string can be extended to (i′) the thought that all strings can be extended, can equally take us from (ii) seeing that a particular inference from $\varphi(a)$ to $\varphi(Sa)$, with 'a' inexplicitly articulated, to (ii′) the thought that the inference goes through with a parametric. But we complained before that the former generalizing move in (i) isn't in fact available just on the basis of intuition; nor likewise for (ii). And in any case, on second thoughts, the cases (i) and (ii) don't seem comparable. Perhaps we can 'see' straight off that a stroke can be added to a given string, in a way that doesn't require pattern-matching, and so it doesn't matter if the string is 'inexplicitly articulated'. But to see an array of wffs as a proof from $\varphi(a)$ to $\varphi(Sa)$ does require, surely, some pattern-matching between the former and latter wffs, and in particular between their component 'a's and 'Sa's – and how on earth can that be done if those component strings are not explicitly articulated?

Which might all suggest that following an instance of the induction rule takes more than the sort of reasoning that is "absolutely basic" to reasoning about intuited objects at all. On what principle, then, does inductive reasoning still count as preserving intuitive groundness?

§44 Primitive recursion Finally, from the subtheses (I1) to (I5), what about (I3), the claim that definition by primitive recursion preserves intuitive knowledge?

Hilbert and Bernays claim that primitive recursion is finitistically acceptable. Their argument seems essentially this. Suppose we are given the simplest equations

$$\varphi(1) = a$$
$$\varphi(n+1) = \psi(\varphi(n)).$$

Then after $m > 1$ substitution steps we'll get an equation

$$\varphi(m) = \psi(\psi(\psi(\ldots\psi(a)\ldots)))$$

without φ on the right. So assuming ψ is already well-defined, φ is.

Now, as Parsons notes, we can tidy up that argument, by making explicit a proof by induction that the computation of $\varphi(m)$ terminates. But note that the claim that a computation terminates is essentially Σ_1 ('there is a number which is the output . . .). So Hilbert and Bernays's argument in effect appeals to Σ_1-induction – which, by *their* lights at any rate, goes beyond finitarily acceptable reasoning, and so for them doesn't preserve intuitive knowledge. Parsons agrees. He oddly says that the Hilbert and Bernays argument for (I3) is *circular*: but that's surely wrong. Granted, the argument depends on a principle which they perhaps officially should not accept; but arguing for the well-definedness of recursively specified functions by using 'too much' induction isn't arguing in a circle, since they aren't here arguing for the legitimacy of PRA's induction schema. But leave that aside: Parsons's thought is presumably that Σ_1-induction involves propositions that go beyond those than can be intutively known. And for this reason, in general, it isn't available to intuition to 'see' that a primitive recursion definition well-defines a function.

§45 The limits of intuitive knowledge Where does all that take us? I'm dubious about the claims (I1), (I2) and (I5). And even Parsons is 'inclined to the negative conclusion, that the arithmetic that is intuitively known does not include exponentiation", so he too is inclined to reject at least (I3) and hence reject what he earlier called Hilbert's Thesis. But

> Rejection of Hilbert's Thesis would still leave open the question whether the functions that can be seen intuitively to be well-defined have weaker closure properties [than closure under primitive recursion], such as being closed under *bounded* primitive recursion. . . . I am far from sure about questions of this kind. It is possible that the notion of intuitive knowledge is not precise enough to decide them.

Well, indeed so! And I'd certainly also wonder whether the general claim that a certain function is well-defined is the sort of thing that can be known quasi-perceptually.

One last point. The claim that members of some class of arithmetical truths are available to be known intuitively is a modal claim: and Parsons in this final section in the chapter now gets more explicit about that. Take a very simple class of truths – propositions of the form $m + n = k$. Small instances like $7 + 5 = 12$ are presumably available to be known intuitively if any arithmetical truths are. But can we intuitively know truths about the addition of numbers e.g. of the order of 2^{65536}? If we say 'no', then only a small fragment of arithmetic is intuitively knowable. Parsons though seems minded to say e.g. that the results of addition (if not exponentiation) are always intuitively knowable. Plainly the 'can' here is not the can of practical possibility. But then what? It's 'mathematical possibility', says Parsons. But that's prima facie a very odd thought. Discussions of mathematical possibility – in any sense I can understand – speak e.g. to the question of what mathematical entities (or: what mathematical truths) there can be: they are simply silent on the question of what our epistemic access is to those entities (or truths). So what's going on here? Earlier, in discussing §42, we wondered whether ideas about finite extensions of our intuitive capacities are what might be in play. But Parsons doesn't explicitly say this: instead he just notes that critics are unhappy about "the application of mathematical modality in combination with epistemic notions". He does not then further explore the worry here – which is very puzzling given the seeming centrality of the notion of intuitive knowability to the overall project of his book.

8 Mathematical Induction

§47 Induction and the concept of natural number Why does the principle of mathematical induction hold for the natural numbers? Well, arguably, "induction falls out of an explanation of the meaning of the term 'natural number'".

How so? The thought can of course be developed along Frege's lines, by simply *defining* the natural numbers to be those objects which have all the properties of zero which are hereditary with respect to the successor function. But it seems that we don't *need* to appeal to impredicative second-order reasoning in this way. Instead, and more simply, we can develop the idea as follows.

Put 'N' for '... is a natural number'. Then we have the obvious 'introduction' rules, (i) $N0$, and (ii) from Nx infer $N(Sx)$, together with the extremal clause (iii) that nothing is a number that can't be shown to be so by rules (i) and (ii).

Now suppose that for some predicate φ we are given both $\varphi(0)$ and $\varphi(x) \to \varphi(Sx)$. Then plainly, by repeated instances of modus ponens, φ is true of $0, S0, SS0, SSS0, \ldots$. Hence, by the extremal clause (iii), φ is true of *all* the natural numbers. So it is immediate that the induction principle holds for φ – e.g. in the form of the finitistically acceptable elimination rule for N that we displayed in discussing §43. Thus far, then, Parsons.

So: two initial issues about this, one of which Parsons himself touches on, the other of which he seems to ignore.

First, as an *argument* warranting induction doesn't this go round in a circle? Doesn't the observation that each and every instance $\varphi(SS\ldots S0)$ is derivable given $\varphi(0)$ and $\varphi(x) \to \varphi(Sx)$ itself depend on an induction? Parsons says that, yes, "As a proof of induction, this is circular. ... Nonetheless, ... it is no worse than arguments for the validity of elementary logical rules." This of course doesn't count against the claim that "induction falls out of an explanation of the meaning of the term 'natural number'" – it is just that the "falling out" is immediate, fully grasping the fundamental rules governing deployment of the concept of a natural number makes inductive arguments 'primitively compelling' in something like Parsons's sense. I'm minded to agree with Parsons here.

Second, some will complain that Parsons's preferred way of seeing induction as given to us in the very notion of 'natural number' is actually not significantly different from Frege's way, because the extremal clause (iii) is essentially second order. It will be said: the idea in (iii) is that something is a natural number if belongs to all sets which contain 0 and are closed under applications of the successor function – which is just Frege's second-order definition put in set terms. Parsons doesn't address this familiar line of thought. But his implicit assumption seems to be that his preferred defence of induction does *not* presuppose full second-order quantification, and his account is indeed different from Frege's.

And if that *is* Parsons's assumption, I'm again minded to agree. In headline terms, just because the notion of transitive closure *can* be defined in second-order terms, that doesn't make it a second-order notion (compare: we can define identity in second-order terms, but that surely doesn't make identity a second-order notion!). It certainly seems arguable that the child who picks up e.g. the notion of an ancestor doesn't thereby exhibit a grasp of second-order quantification – and more generally, grasping how to form the ancestral of a relation is a conceptual leap all right, but it is far from clear that it is the conceptual leap as far as understanding full second-order quantification. But more really needs to be said about this.[20]

[20]For just a little more, see my *Introduction to Gödel's Theorems*, §23.5.

Parsons now takes up three more issues:

1. His first question is: "What is the range of the first-order variables?" over which we can apply the rules which ground his self-styled "justification" of induction? Some domain of entities, presumably, that can be given to us prior to our specifying its "numbers", i.e. the zero and its successors. "However," says Parsons, "this is . . . to assume that some infinite structure is given to us independently of our knowledge of the kind of structure the natural numbers instantiate."

   But I'm not sure why Parsons says this is required for the argument from induction. After all, take *any* domain which contains a zero element 0 and for which a function S is defined. Then, whether the function S is injective or otherwise, whether the domain is finite or infinite, we'll be able to similarly define N – meaning 'is 0 or one of its successors' – and the induction rule will hold for the Ns. To be sure, in the case of arithmetic, we'll have further rules governing S to ensure that the Ns form an infinite progression: but Parsons's "justification of induction" *seems* to work equally well whether they do and whether they don't. If he thinks that there is something special about the infinite case, then he doesn't bring the point out clearly here.

2. Second, Parsons comments on "the schematic character of the induction rule. . . . the applicability of the rule is not limited to predicates defined in some particular first-order language such as that of first-order arithmetic. But we must not take it as implying the unavoidability or even the legitimacy of full second-order logic." The target here, I suppose, is Kreisel's well known contrary claim that we mentioned before in discussing §5: we accept instances of a schematic form of the induction rule because we *already* accept the full second-order induction axiom. I take it that Parsons's contrary argument is that the reasoning that led us to accept the induction rule was silent on the particular character of the filling for φ – that, it seems, was left entirely open ended (the permitted fillings will be whatever we can make sense of, as wide or as narrow a class as that is): but silence doesnt mean agreeing to the coherence of the full second-order notion of quantifying over arbitrary properties, where these are conceived of as being in effect arbitrary subsets of the domain of the first-order variables (when that domain is infinite). In fact, I agree, though the matter surely needs more discussion than Parsons gives it.[21]

3. "A third question is whether and in what sense is induction an analytic or conceptual rule or truth." Parsons's line is that "The explanation of the number concept by rules makes induction follow from an explanation of that concept: it is certainly in some sense 'conceptual'." But then what of someone who does not accept induction across the board – say, a finitist who doesn't countenance Σ_1 induction? Is he then guilty of failing to acknowledge a conceptual truth?

   No, says Parsons, and surely rightly. We should take the finitist objection to be not against the schematic induction rule itself but rather against the admission of certain [say, Σ_1] predicates as fully kosher and hence available for substitution into the schema.

§48 The problem of the uniqueness of the number structure: Nonstandard models "There is a strongly held intuition that the natural numbers are a unique

[21] Cf. my *An Introduction to Gödel's Theorems*, §22.2.

structure." Parsons now begins to discuss whether this intuition – using 'intuition', of course, in the common-or-garden non-Kantian sense! – is warranted. He sets aside until the long §49 issues arising from arguments of Dummett's: here he makes some initial points on the uniqueness question, arising from the consideration of nonstandard models of arithmetic.

It's worth commenting first, however, on what looks at first blush to be a certain 'disconnect' between the previous section and this one. For recall, Parsons has just been discussing how we might introduce a predicate 'N' ('... is a natural number') governed by the rules (i) $N0$, and (ii) from Nx infer $N(Sx)$, plus the extremal clause (iii) that nothing is a number that can't be shown to be so by rules (i) and (ii). Together with the rules for the successor function, the extremal clause – interpreted as intended – ensures that the numbers will be unique up to isomorphism. Conversely, our naive intuition that the numbers form a unique structure is surely most naturally sustained by appeal to that very clause. The thought is that any structure for interpreting arithmetic as informally understood must take numbers to comprise a zero element, its successors (all different, by the successor rules), *and nothing else.* And of course the numbers in each such structure will then have a natural isomorphism between them (which matches zeros with zeros, and n-th successors with n-th successors). So the obvious issue to take up at this point is: what does it take to grasp the intended content of the pivotal extremal clause? Prescinding from general worries about rule-following, is there any special problem about understanding that clause which might suggest that, after all, different arithmeticians who deploy that clause – passing all the ordinary tests for understanding – could still be talking of different, non-isomorphic, structures? However, obvious though these questions are given what has gone before, Parsons doesn't directly raise them in quite that expected form[22] – although the next section *does* take up closely related matters.

Let's ask, though: given the ready availability of the sort of informal argument just sketched, why ever should we doubt uniqueness? Ah, the skeptical response will go, we can't take the understanding of the extremal clause to be secure: after all, regiment our arithmetical thinking however we like, there can still be rival interpretations (thanks to Löwenheim-Skolem theorems). Even if we dress up the uniqueness argument – by putting our arithmetic into a set-theoretic setting and giving a formal treatment of the content of the extremal clause, and then running a full-dress version of the informal Dedekind categoricity theorem – that still can't be used settle the uniqueness question. For the requisite background set theory[23] itself, presented in the usual first-order way, can itself have nonstandard models: and we can construct cases where the unique-up-to-isomorphism structure formed by 'the natural numbers' inside such a nonstandard model won't be isomorphic to the 'real' natural numbers. And going second-order doesn't help either: we can still have non-isomorphic "general models" of second-order theories, and the question still arises how we are to exclude *those.* In sum, the skeptical line runs, someone who starts off with worries about the uniqueness of the natural-number structure because of the possibilities of non-standard models of arithmetic, won't be mollified by categoricity arguments, for these presuppose uniqueness elsewhere, e.g. in our background set theory.

That skeptical line of thought will, of course, be met with equally familiar responses

[22] He reports that §§47, 50 and 51 in this chapter draw on a 1983 paper, while §§48 and 49 draw on a 1992 paper – which possibly explains the disconnect?

[23] Let's consider for the moment the usual setting for the theorem as conventionally presented: in §49 Parsons presents a version which requires less.

(familiar, that is, from discussions of the philosophical significance of the existence of nonstandard models as assured us by the L-S theorem). For example, it will be countered that things go wrong at the outset. We can't keep squinting sideways at our *own* language – the language in which we do arithmetic, express extremal clauses, and do informal set theory – and then pretend that more and more of it might be open to different interpretations. At some point, as Wittgenstein insisted and Dummett reiterates, there has to be understanding without further interpretation (and at that point, assuming we are still able to do informal arithmetical reasoning at all, we'll still be able to run the informal argument for the uniqueness of the numbers we *are* talking about).

How does Parsons stand with respect to this sort of dialectic? He outlines the skeptical take on the Dedekind argument, in particular explaining in some detail how to parlay a certain kind of nonstandard model of set theory into a nonstandard model of arithmetic. But his first response, the one in the present section, isn't the very general line of thought just mooted (though he does take that up in the next section). Rather, he here claims that the way his described construction works "witnesses the fact the model is nonstandard" – and this seems to show that a grasp of his constructed model which provides a deviant interpretation of arithmetic has to piggy-back on a prior grasp of the standard interpretation. If that's right, then the idea that we might have deviantly cottoned onto *that* constructed nonstandard model from the outset is surely undermined.

Parsons next makes an interesting generalizing point about nonstandard models, noting the difference between (1) those other cases where we get deviant interpretations that we can in some sense grasp but where our understanding piggy-backs on a prior standard understanding of the theory in question, and (2) those cases where we just that there exist alternative models because of the countable elementary submodel version of the L-S theorem. Since the existence of the latter submodels is typically given to us via the axiom of choice, these resulting interpretations are, in a sense, unsurveyable by us, hence – for a different reason – they are again arguably not available as alternative interpretations we might have cottoned onto from the outset. And if we can argue that the cases of non-standard models fall into either type (1) or type (2), then we'd have the beginnings here of a line of response to skeptical arguments based on the L-S theorem. But, Parsons doesn't in the end push the idea. Indeed, slightly puzzlingly, he says he isn't after all going to attempt to directly answer skeptical arguments based on the L-S theorem. And he finishes the section by saying the theorem "seems still to cast doubt on whether we have really 'captured' the 'standard' model of arithmetic". So I'm left rather puzzled just what he thinks he has achieved here.

§49 Uniqueness and communication Parsons now takes another pass at the question whether the natural numbers form a unique structure. And this time, he offers something like the broadly Wittgensteinian line which we mooted above as a riposte to skeptical worries – though I'm not sure that I have grasped all the twists and turns of Parsons's intricate discussion.

We'll start by following Parsons in considering the following scenario. Michael uses a first-order language for arithmetic with primitives $0, S, N$, and Kurt uses a similar language with primitives $0', S', N'$. Each accepts the basic Peano axioms, and each also stands ready to accept any instances of the first-order induction schema, or equivalently stands ready to deploy the rule displayed above (in discussing §43), for predicates formulable in his respective language (or in an extension of that language which he can come to understand). And we now ask: how could Michael determine that his 'numbers' are isomorphic to Kurt's?

We'll assume that Michael is a charitable interpreter, and so he thinks that what Kurt says about *his* numbers is in fact true. And we can imagine that Michael recursively defines a function f from his numbers to Kurt's in the obvious way, putting $f(0) = 0'$, and $f(Sn) = S'f(n)$ (of course, to do this, Michael has to add Kurt's vocabulary to his own, while shelving detailed questions of interpretation – but suppose that's been done). Then trivially, each $f(n)$ is an N' by Kurt's explicit principles which Michael is charitably adopting. And Michael can also show that f is one-one using his own induction principle.[24]

Thus Michael can show that f is an injection from the Ns into the N's, whatever exactly the latter are. But, at least prescinding from the considerations in the previous section, that so far leaves it open whether – from Michael's point of view – Kurt's numbers are non-standard (i.e. it doesn't settle for Michael whether there are also Kurt-numbers which aren't f-images of Michael-numbers). How could Michael rule that out? Well, he could show that f is onto, and hence prove it a bijection, *if* he could borrow *Kurt's* induction principle – which he is charitably assuming is sound in Kurt's use – applied to the predicate $\exists m(Nm \wedge fm = \xi)$. But now, asks Parsons, what entitles Michael to suppose that *that* is indeed one of the predicates Kurt stands prepared to apply induction to? Why presume, for a start, that Kurt can get to understand *Michael*'s predicate N so as to bring it under the induction principle?

It would seem that, so long as Michael regards Kurt 'from the outside', trying to 'radically interpret' him as if an alien, then he has no obvious good reason to presume that. But on the other hand, that's just not a natural way to regard a fellow human being. The natural presumption is that Kurt *could* learn to use N as Michael does, and so – since grasping meaning is grasping use – could come to understand that predicate, and likewise grasp Michael's f, and hence come to understand the predicate $\exists m(Nm \wedge fm = \xi)$. Hence, taking for granted Kurt's common humanity and his willingingness to extend the use of induction to new predicates, Michael *can* then complete the argument that his and Kurt's numbers are isomorphic. Parsons puts it like this. If Michael just takes Kurt as a fellow speaker who can come to share a language, then

> We now have a situation that was lacking when we viewed Michael's understanding of Kurt as a case of radical interpretation; namely, he will take his own number predicate as a well-defined predicate according to Kurt, and so he will allow himself to use it in induction on Kurt's numbers. That will enable him to complete the proof that his own numbers are isomorphic to Kurt's.

And note, the availability of the proof here "does not depend on any global agreement

[24] Let's abbreviate '$\forall m$(if $f(m) = f(\xi)$, then $m = \xi$)' as '$F(\xi)$', where the quantification is, of course, over Ns. Then Michael wants to show that $\forall nF(n)$. With induction to hand, it is enough for Michael (a) to prove $F(0)$, and (b) to show that the assumption $F(n)$ implies $F(Sn)$.

(a) Assuming Nm, if $m \neq 0$, then $m = Sp$, so $f(m) = f(Sp) = S'f(p) \neq 0'$, with the last inequality given by Michael's charitably taking over Kurt's principles for S'. Hence, contraposing and generalizing in the obvious way, $\forall m$(if $f(m) = f(0)$, then $m = 0$) – i.e. $F(0)$.

(b) Assume $F(n)$, i.e. $\forall m$(if $f(m) = f(n)$, then $m = n$). And now suppose $f(m) = f(Sn)$. Then $f(m) = S'f(n)$, hence $f(m) \neq 0'$, whence $m \neq 0$, so $m = Sp$ for some p. Hence, since $f(m) = S'f(n)$, we have $f(Sp) = S'f(p) = S'f(n)$. Hence by Kurt's rules for S' which Michael charitably accepts as sound, $f(p) = f(n)$, so by our general assumption, $p = n$, hence $m = Sp = Sn$. So: the supposition that $f(m) = f(Sn)$ implies $m = Sn$. Whence, by conditional proof and generalizing, $F(Sn)$. So we are done.

And that indeed is Parsons's argument, slightly tidied up – inter alia to avoid an m/n fumble.

between them as to what counts as a well-defined predicate", nor on Michael's deploying a background set theory.

So far, then, so good.[25] Things would seemingly have gone easier if Michael and Kurt had both been introducing number talk as Parsons discusses in §47, each deploying an extremal clause that says that their numbers are a zero, its successors, *and nothing else*. But let that pass, and ask instead: how far does this all take us? You might say: if Michael and Kurt in effect can come to belong to the same speech community, then indeed they might then reasonably take each other to be talking of the same numbers (up to isomorphism) – but that doesn't settle whether what they share is a grasp of a standard model. But again, that is to look at them together 'from the outside', as aliens. If we converse with them as fellow humans, presume that they stand ready to use induction on our predicates which they can learn, then we can use the same argument as Michael to argue that they share *our* conception of the numbers. You might riposte that this still leaves it open whether we've *all* grasped a nonstandard model. But *that* is surely confused: as Dummett for one has stressed, in order to formulate the very idea of models of arithmetic – whether standard or nonstandard – we must already be making use of our notion of 'natural number' (or notions that swim in the same conceptual orbit like 'finite', or stronger notions like 'set'). To put that notion into doubt is to saw off the branch we are sitting on when describing the models. Or as Parsons says, commenting on Dummett,

> [I]n the end, we have to come down to mathematical language as *used*, and this cannot be made to depend on semantic reflection on that same language. We can see that two purported number sequences are isomorphic without strong set-theoretic premisses, but we cannot in the end get away from the fact that the result obtained is one "within mathematics" (in Wittgenstein's phrase). We can avoid the dogmatic view about the uniqueness of the natural numbers by showing that the principles of arithmetic lead to the Uniqueness Thesis . . .

So, there is indeed basic agreement here with the Wittgensteinian observation that in the end there has to be understanding without further interpretation. But Parsons continues,

> . . . but this does not protect the language of arithmetic from an interpretation completely from the outside, that takes quantifiers over numbers as ranging over a non-standard model. One might imagine a God who constructs such an interpretation, and with whom dialogue is impossible, and with whom dialogue is impossible. But so far the interpretation is, in the Kantian phrase, "nothing to us". If we came to understand it (which would be an essential extension of our own linguistic resources) we would recognize it as unintended, as we would have formulated a predicate for which, on the interpretation, induction fails.

Well, yes and no. True, *if* we come to understand someone as interpreting us as thinking of the natural numbers as outstripping zero and its successors, then we would indeed

[25]'Hold on! How does Michael in effect get a categoricity argument using first-order reasoning when first-order arithmetic isn't categorical?' But look at it this way: Michael's result isn't a categoricity result in the model-theoretic sense – he isn't squinting sideways at his language and asking how many kinds of models it has, but arguing that his and Kurt's numbers come to the same.

recognize him as getting us wrong – for we could then formulate a predicate 'is-zero-or-one-of-its-successors' for which induction would have to fail according to his interpretation, contrary to our open-ended commitment to induction. And further dialogue will reveal the mistake to the interpreter who gets us wrong. However, perhaps contra Parsons, we surely don't have to pretend to be able to make any sense of the idea of a God who constructs such an interpretation and 'with whom dialogue is impossible': Davidson and Dummett, for example, would both surely reject *that* idea.

In sum, then, we might put things like this. Parsons has defended an 'internalist' argument – an argument from "within mathematics" – for the uniqueness of the numbers we are talking about in our arithmetic, whilst arguing against the need for (or perhaps indeed, against the possibility of) an 'externalist' justification for our intuition of uniqueness.

Can we rest content there? Parsons takes Hartry Field as thinking that we can appeal to a 'cosmological hypothesis' together with an assumption of the determinateness of our *physical* vocabulary to rule out non-standard models of our applicable arithmetic. And about *that* idea, Parsons reasonably enough worries: "If our powers of mathematical concept formation are not sufficient [to rule out nonstandard models], then why should our powers of physical concept formation do any better?" But I'm not sure that Parsons has got Field quite right here: isn't Field more concerned to argue the connection, that if we think our arithmetical practice allows e.g. non-standard interpretations of finiteness, then there will be the same worries about interpreting physics.

He also discusses Shaughan Lavine's argument that our arithmetic can be regimented as a "full schematic theory" which is in fact stronger than the sort of theory with open-ended induction that we've been considering, and for which a categoricity theorem can be proved. But Parsons finds some difficulty in locating a clear conception of exactly what counts as a full schematic theory – a difficulty on which, indeed, I've commented elsewhere. But Parsons's discussions of Field and Lavine are very brief, and more really needs to be said (though not here).

§50 Induction and impredicativity Parsons now takes up another topic that he has written about influentially before, namely impredicativity. He describes his own earlier claim like this: "no explanation [of the predicate 'is a natural number'] is in sight that is not impredicative". That claim has been challenged by Feferman and Hellman in a couple of joint papers, and Parsons takes the present opportunity to respond. As the title of this section indicates, Parsons links claims about impredicativity to thoughts about the scope of induction.

But how much is an 'explanation' of the concept natural number supposed to explain? Parsons doesn't explicitly tell us (though, as we'll see in a moment, he has already said things that would imply that he in fact recognizes two levels of task here). And what exactly does Parson *mean* by impredicativity? Oddly, he doesn't explicitly tell us that either. Nor does he really explain why it might *matter* whether definitions of the natural numbers have to be impredicative. So there's quite a lot to sort out here, and I can only make a start here.

What is to be explained? Suppose someone (a) grasps that natural numbers comprise zero and its successors, and nothing more, and grasps that induction holds for any kosher predicate that expresses a genuine property of numbers (and, lets add, understands the cardinal and ordinal uses of numbers in counting, and understands addition and multiplication too), but (b) she hasn't yet come to a view about just which *are* the kosher predicates, even among those expressed in a purely arithmetical language. Do we say that she has a determinate understanding of the predicate 'is a natural number' or not?

We could answer 'yes' – in getting to level (a), she can count as grasping the notion of being a natural number. What she is wavering about is something else, e.g. whether it is legitimate to define predicates of numbers in terms of quantification over 'the completed infinity' of numbers and assert propositions involving such predicates. This response would in some ways chime with what Parsons says in §47 about introducing the notion of the natural numbers via the rules he describes, for he explicitly remarks there that "the concept of natural number cannot determine what counts as a well-defined predicate".[26]

But we could answer 'no' – a fully formed concept of the natural numbers should, inter alia, fix which predicates in the language of arithmetic make determinate sense. Thus it might be said that a radical constructivist – who limits the acceptable arithmetical predicates to those which express decidable properties of numbers – has one concept of the natural numbers; and a realist who allows arithmetical predicates with arbitrarily complex embedded numerical quantifiers must have a different concept.[27]

Let's not fuss about which answer is right (or indeed whether the question is entirely well-posed). For present purposes the point is this: 'explaining' the concept natural number could be taken to be a matter of explaining what we grasp at what I'll call the *basic* level; or the explanatory task could be more ambitious, explaining enough to determine which are the well-defined predicates. Which is Parsons after?

What is impredicativity? The usual sort of account of impredicativity, in the same vein as Russell's original (or rather, as *one* of Russell's originals), runs roughly like this: 'a definition ... is impredicative if it defines an object which is one of the values of a bound variable occurring in the defining expression', i.e. an impredicative specification of an entity is one 'involving quantification over all entities of the same kind as itself'.[28] Thus Weyl, famously, argued against the cogency of some standard constructions in classical analysis on the grounds of their impredicativity in this sense.[29]

And why should we *care* about avoiding impredicative definitions for Xs? Why should such definitions, at least in certain circumstances, lack cogency? Well, here's *one* strand of thought: suppose we think that X's are in some sense (however tenuous) 'constructed by us' and not determined to exist prior to our mathematical activity. Then, it is surely illegitimate to give a recipe for constructing a particular X which requires us to take as already given as constructed a totality of Xs which includes the very one that we

[26]Note though that Parsons's given reason is that "through application or through the further development of mathematics" we can keep introducing relevant new predicates. And that thought would strictly speaking be compatible with claiming, after all, that the concept of natural number fixes at least which predicates in purely arithmetical language are kosher.

[27]This perhaps accords with Michael Dummett's thought when he seems to suggest – in his paper 'The Philosophical Significance of Gödel's Theorem', p. 194 – that in one sense of 'meaning', grasping the meaning of 'natural number' involves more than grasp at the basic level (a).

[28]The first quotation is from Fraenkel, Bar-Hillel and Levy, *Foundations of Set Theory*, p. 38, one of a number of very similiar Russellian definitions quoted by Alexander George in his 'The imprecision of impredicavity' (*Mind*, 1987); the second much more recent quotation is from John Burgess *Fixing Frege*, p. 40.

[29]And because ACA_0 bans impredicative specifications of sets of numbers, it provides one possible framework for developing those portions of analysis which should be acceptable to someone with Weyl's scruples. Now, as Parsons in effect notes, ACA_0 which lacks an impredicative comprehension principle is often described as being, unqualifiedly, a predicative theory of arithmetic: but that description takes it for granted that its first-order core – Peano Arithmetic – isn't impredicative in some other respect.

are now attempting to define. So at least any definition which is to play the role of a recipe-for-construction had better not be impredicative. Given Weyl's constructivism about sets, then, it is no surprise that he rejects impredicative definitions of sets. I'll not pause to assess this line of thought any further here: but I take it that it is a familiar one.[30]

Now, on the Russellian understanding of the idea, a definition of the set of natural numbers will count as 'impredicative' if it quantifies over some totality of sets including the set of natural numbers. Modulated into property talk, we'd have: a definition of the property of being a natural number will count as impredicative if it quantifies over some totality of properties including the property of being a natural number. Some familiar definitions are indeed impredicative in this sense: take, for example, a Frege/Russell definition which says that x is a natural number iff x has all the hereditary properties of zero. Here, the quantification is over a totality which includes the property of being a natural number, and the definition is indeed impredicative in a Russellian sense.

So far, so familiar. But in talking about arithmetic, some authors – including Parsons – bring another, non-Russellian notion of impredicativity into play. Thus Edward Nelson, at the beginning of his book, *Predicative Arithmetic*, says that the usual induction principle "involves an impredicative concept of number":

> A number is conceived to be an object satisfying every inductive formula; for a particular inductive formula, therefore, the bound variables are conceived to range over objects satisfying every inductive formula, including the one in question.

Similarly Dummett, in his 'The Philosophical Significance of Gödel's Theorem' (quoted approvingly by Parsons), writes:

> [T]he notion of 'natural number' . . . is impredicative. The totality of natural numbers is characterised as one for which induction is valid with respect to any well-defined property, . . . the impredicativity remains, since the definitions of the properties may contain quantifiers whose variables range over the totality characterised.

So the claim *isn't* that "a characterization of the natural numbers that includes induction as part of it will be impredicative" because (i) the totality of natural numbers is being defined in terms of a quantification over some domain which has as a member the totality of natural numbers itself. Or putting that in terms of properties, the claim *isn't* that we have impredicativity here because (ii) the property of being a natural number is being defined in terms of a quantification some domain which already includes that property. Rather, the impredicativity arises because (iii) the property of being a natural number is being defined by reference to quantification(s) over things that *fall under* that property. To quote Parsons:

> Because the number concept is characterized as one for which induction holds for any well-defined predicate or property, there is impredicativity if those involving quantification over numbers are included, as they evidently are.

[30]By the way, I don't want to imply that constructivist thoughts are the *only* ones that might make us suspicious of impredicative definitions: though as Ramsey and Gödel pointed out, it is far from clear why a gung-ho realist should be too worried about impredicative definitions.

In sum, Nelson, Dummett and Parsons are here using a non-Russellian notion of impredicativity (iii), as opposed to the original Russellian brand (i)/(ii).

Now, at first blush, it might seem that the idea is that an explanation of the natural numbers will be impredicative (in the NDP sense), just if – explicitly or tacitly – it somehow involves generalizations over what we are characterizing, i.e. the naturals. But that reading is surely too sweeping. Consider again the constructivist who, as we saw, has reasons to eschew definitions that are impredicative in Russell's sense. By contrast, it doesn't offend at all against constructivist principles to set out to characterize the natural numbers by saying such things as 'for any number, its successor is also a number', or 'any number can be reached by repeatedly applying the successor function starting from zero'. Prescinding from worries about our limited capacities, such explanations tell us, precisely, how each and every number can be 'constructed', at least in principle, and tell us not to worry about there being any 'rogue cases' which our construction rules can't reach. Yet the explanations involve generalizations which range over what we are characterizing, i.e. the naturals. And *why* are such generalizations constructively acceptable? Presumably because grasping *those* does not require conceiving of the numbers as a 'completed infinite totality'. Such uses of generalizations over numbers to explain the concept are surely harmless (and not 'impredicative' in any interesting sense).

So what *is* at stake in the NDP characterization of impredicativity? The focus is on predicates of numbers which embed quantifiers. Such a predicate will in general express a condition which is satisfied by a given number n if something obtains of *all* numbers – so directly determining whether n satisfies the property would involve a non-constructive supertask. And the concern here is not that deploying such a predicate in itself directly offends against some vicious circle principle, but that it surely involves a non-constructive conception that goes beyond what is given to us by a basic level grasp of the numbers. And it is *this* point that the NDP notion of impredicativity seems really to be picking up on: allowing the comprehensibility of unrestrictedly complex instances of the induction schema involves already making sense of a richer conception of the numbers than is given us at the basic level – and therefore trying to explain that richer conception of the numbers by saying that it allows unrestricted induction takes us in a very tight conceptual circle.

Predicative explanations of the concept of number? Take again the kind of account which Parsons outlines in §47. We explain that (i) zero is a natural number, (ii) if n is a natural number, so is Sn, and (iii) whatever is a natural number is so in virtue of clauses (i) and (ii). That gives us the basic structure of the number-sequence. Now augment the story with definitions of addition and multiplication, and whatever other primitive recursive functions we want. This basic level characterization of the natural numbers does not *explicitly* involve a quantification over a class of properties including that of a natural number. It might be claimed, of course, that understanding of the crucial extremal clause (iii) does *implicitly* require a grasp of impredicative uses of second-order quantification. But I've urged before that this view is contentious; and indeed the contentious view doesn't seem to be one that Parsons endorses (since he seems to recommend his §47 characterization of the numbers as having the virtue of *not* being lumbered with the weighty baggage of the impredicative Frege/Russell account).

So here we have, arguably, a basic level explanation of the concept of number which *isn't* impredicative in the Russellian sense. Nor is it impredicative in the NDP sense. For this explanation doesn't presuppose the availability of arbitrarily complex inductions.

Rather, as Parsons himself argues, while it underpins induction for whatever are well-defined predicates, it takes more to determine what counts as a well-defined predicate for inductive purposes.

So, at the basic level, there are impredicative explanations of number to be had. It is unclear that Parsons would dissent: for his concern is probably with explanations of a richer conception of numbers, which *do* purport to justify a generous class of predicates for induction. Do *they* have to be 'impredicative' in some sense?

On Feferman and Hellman Well, perhaps we can try making play with ideas about sets to get somewhere. Suppose we help ourselves to the notion of a finite set, and start by saying that x is a number if (i) there is at least one finite set which contains x and if it contains Sy contains y, and (ii) every such finite set contains 0. This definition isn't impredicative in the strict Russellian sense.[31] Nor is it overtly impredicative in anything like the NDP sense. We might argue that it is still covertly impredicative if elucidating the very notion of a finite set already presupposes a rich conception of the naturals. But is *that* right?

This is where Feferman and Hellman enter the story. For, as Parsons remarks, they aim to offer in their theory EFSC a grounding for arithmetic in a theory of finite sets that is predicatively acceptable and that also explains the relevant idea of finiteness in a way that does not presuppose the notion of natural number. Though now things get a bit murky (and I think it would take us too far afield to pursue the discussion any further here). But Parsons's verdict is that

> EFSC admits the existence of sets that are specified by quantification over all sets, and this assumption is used in proving the existence of an N-structure [i.e. a natural number structure]. For this reason, I don't think that ... EFSC can pass muster as strictly predicative.

Here 'predicative' is used again in NDP sense: so the worry is that EFSC explication requires grasping the same kind of move from the grasp potential to actual infinities that we are trying to explain for arithmetic. This seems right, if I am following.

Where have we got to? We noted before that Parsons's §47 rule-based explanation of the notion of natural number sustains induction for well-defined properties but leaves it wide open *which* are the well-definined properties. So it seems a further thought, going beyond what is given in that explanation, to claim that any predicate involving first-order quantifications over the numbers is in fact well-defined. There are surely arithmeticians of finitist or constructivist inclinations, who fully understand the idea that the natural numbers are zero and its successors and nothing else, and understand (at least some) primitive recursive functions, but who resist the thought that we can understand predicates involving arbitrarily complex quantifications over the totality of numbers, since we are in general bereft of a way of determining in a finitistically/constructively acceptable way whether such a predicate applies to a given number. To put it in headline terms: it is a significant conceptual move to get from grasping PRA to grasping (first-order) PA – we might say that it involves moving from treating the numbers as a potential infinity to treating them as a completed infinity. But we need an argument that someone who balks at the move has not grasped the concept *natural number* – indeed, it as I've put, they

[31] As Alexander George points out in his 'The imprecision of impredicativity'.

surely do have a basic grasp.[32] And the NDP challenge, if I'm reading its import right, is to ask whether there is a non-circular explication of the notion of natural number that justifies us in making the move. Parsons's conjecture is that there isn't.

Crispin Wright has written

> Ever since the concern first surfaced in the wake of the paradoxes, discussion of the issues surrounding impredicativity – when, and under what assumptions, are what specific forms of impredicative characterizations and explanations acceptable – has been signally tangled and inconclusive.

Indeed so! But given that tangled background, any discussion really ought to go more slowly and more explicitly than Parsons does here.

§51 Predicativity and inductive definitions The final section of Ch. 8 sits rather uneasily with what's gone before. The preceding sections are about arithmetic and ordinary arithmetic induction, while this one briskly touches on issues arising from Feferman's work on predicative analysis, and iterating reflection into the transfinite. It also considers whether there is a sense in which a rather different (and stronger) theory given by Paul Lorenzen some fifty years ago can also be called 'predicative'. There is a page here reminding us of something of the historical genesis of the notion of predicativity: but there is nothing, I think, in this section which helps us get any clearer about the situation with arithmetic, the main concern of the chapter (rather it discusses what else you might be argued into accepting if you go along with full-strength ordinary arithmetic induction). So I'll say no more about it.

9 Reason

§§52–53, Reason, "rational intuition" and perception In the first section of this final chapter, Parsons rehearses five features of our practice of supporting our claims by giving reasons (occasionally, he talks of 'features of Reason' with a capital 'R': but this seems just to be Kantian verbal tic which lacks any particular significance).

(a) Reasoning involves logical inference (and "because of their high degree of obviousness and apparently maximal generality, we do not seem to be able to give a justification of the most elementary logical principles that is not in some degree circular, in that inferences codified by logic will be used in the justification"). (b) In a given local argumentative context, "some statements . . . play the role of principles which are regarded as plausible (and possibly even evident) without themselves being the conclusion of arguments (or at least, their plausibility or evidence does not rest on the availability of such arguments)." (c) There is a drive towards systematization in our reason-giving – "manifested in a very particular way [in the case of mathematics], through the axiomatic method". (d) Further, within a systematization, there typically is a to-and-fro dialectical process of reaching a reflective equilibrium, as we play off seemingly plausible local principles against more over-arching generalizing claims. (e) Relatedly, "In the end we have to decide, on the basis of the whole of our knowledge and the mutual connections

[32] How much arithmetic can we get if we *do* balk at the extra move and restrict induction to those predicates we have the resources to grasp in virtue of grasping what it is to be a natural number (plus grasping addition and multiplication, say)? Well, arguably we can get at least as far as $I\Delta_0$, and Parsons talks a bit about this at the end of the present section.

of its parts whether to credit a given instance of apparent self-evidence or a given case of what appears to be perception".

Now, that final Quinean anti-foundationalism is little more than baldly asserted. And how does Parsons want us to divide up principles of logical inference from other parts of a systematized body of knowledge? His remarks about treating the law of excluded middle "simply as an assumption of classical mathematics" suggest that he might want to restrict logic proper to some undisputed core – though he doesn't tell us what that is. Still, quibbles apart, the general drift of Parsons's reflections about Reason will strike most readers nowadays as unexceptionable.

In the next section, he goes on to say a bit more to compare and contrast intuitions in the sense of statements found in a given context of reasoning to be intrinsically plausible – call these "rational intuitions" – and intuitions in the more Kantian sense that has occupied Parsons in earlier chapters. As he says, "intrinsic plausibility is not strongly analogous to perception [of objects]", in the way that Kantian intuition is supposed to be. But he suggests that we *might* argue that analogies with perception remain. This will be so if, in particular, we endorse a Gödelian view that intrinsic plausibility for some mathematical propositions involves something like perception of *concepts* (though Parsons is not minded here to endorse that view). And there is perhaps is another analogy too, suggested by George Bealer: reason is subject to illusions that, like perceptual illusions, persist even after they have been exposed.

But Parsons only briefly floats those last ideas here, and the section concludes with a quite different thought, namely there is a kind of 'epistemic stratification' to mathematics, with propositions at the lowest level seeming indisputably self-evident, and as we get more general and more abstract, self-evidence decreases. And at the level of some brisk descriptive remarks, Parsons is of course right: but his remarks go no way towards settling how we are best to articulate and explain the phenomenon.

§54 Arithmetic How does arithmetic fit into the sort of very rough picture of the role of reason and so-called "rational intuition" drawn in the preceding two sections?

The bald claim that some basic principles of arithmetic are "self-evident" is, Parsons thinks, decidedly unhelpful (what is evident to the enthusiast for full second-order arithmetic is not evident for the very strict finitist, for example). So how should we think of the epistemology of arithmetic?

Reason, in the sense of connection-making, plays its part: "in mathematical thought and practice, the axioms of arithmetic are embedded in a rather dense network ... [which] serves to buttress [their] evident character . . . so that in that respect their evident character does not just come from their intrinsic plausibility." True, we don't get much top-down revisionary pressure on elementary arithmetical beliefs from new more 'theoretical', claims. But supposedly, once we get away from basics, we do get a subtle interplay between arithmetical claims and other principles – a dialectic "between attitudes towards mathematical axioms and rules and methodological or philosophical attitudes having to do with constructivity, predicativity, feasibility, and the like".[33] Further, arithmetic isn't a free-floating game: we have ties between arithmetical claims and their applications. Which, as Parsons himself notes, is all sounding rather Quinean. How is his position distinctive?

[33] Is that right? It doesn't look to be so much a dialectic as a matter of methodological and philosophical attitudes fixing how much of a rather fixed structure of arithmetic we are supposed to take seriously. But let that pass.

Not, it turns out, by making further play with talk of "rational intuition". That made its temporary appearance in Sec. 53 just as a way of talking about what is intrinsically plausible, and Parsons seems to want give the notion no more epistemological weight than that. Indeed, he says that the idea that the axioms of arithmetic derive a special status from being grounded in rational intuition is said to be "in an important way misleading". Where Parsons does depart from Quine, then – and by now it is no surprise to be told this! – is in holding that some elementary arithmetic principles are 'intuitively evident', can be intuitively known in the Hilbertian sense he discussed in earlier chapters, and "an intuitive domain witnesses the possibility of the structure of numbers". As we move to more sophisticated areas of arithmetic and beyond, areas which cannot directly be so grounded, "the conceptual or rational element in arithmetical knowledge becomes much more prominent", but the web of arithmetic keeps its grounding in intuitions of stroke strings and the like.

Of course, quite how impressed we are by that story will depend on how well we think Parsons defended his conception of intuitive knowledge in earlier chapters (and I'm not going to go over that ground again now, nor indeed does Parsons in this chapter). But set that aside. There's another question: how we are to marry the holism with the dash of foundationalism in the claims that there are after all epistemically fixed points that are pretty securely given to us in intuition? Relatedly: what warrants the parts of arithmetic that *don't* get rooted in Hilbertian intuition? To be sure, those more advanced parts can get tied closely to other bits of mathematics, notably set theory, so there is that much rational constraint. But that just shifts the question: why make the ties we do? and what grounds those related theories as reasonable? (There are some remarks in the next section, but as we'll see they are not very unhelpful.)

So where have we got to? Parsons's picture of arithmetic retains a role for Hilbertian intuition. And unlike an "all-in" holism, he wants to emphasize the epistemic stratification of mathematics and the place of arithmetic at or near the bottom of the heap (though his remarks on that stratification really do little more than point to the phenomenon). In sum

> [W]hat most distinguishes the view advocated here from the holistic empiricist view is that the smaller, accessible objects of the domain are given in intuition, there is intuitive knowledge of certain ground-level propositions, and the conceptual or rational element is limited by the very low place the mathematics involved occupies on the hierarchy of levels of mathematical theories.

Still, he says,"our view does not differ *toto caelo* from holism". But then – to repeat the question – what, in the end, is Parsons's view of the status of the overall web of arithmetical belief, including the non-intuitive parts? Presumably it's not going to be *toto caelo* different from on Quine's story. Yet, in the next and final section of the book, Parsons seems to break with another key Quinean thought, by having no truck with indispensability considerations as constraining how we fill in what lies beyond the intuitive. Parsons notes that applicable mathematics can be reconstructed in very weak subsystems of ZFC, but remarks that "there is no convincing ground on which the part left out should be ruled out of court as genuine mathematics". Well, of course the Quinean can agree that the wilder reaches of set theory are mathematics in the sense of being a delightful exploratory game (and there will be lots of truths there about what follows from what). But he'll insist that we can't just assume that that it is genuine in the sense of about genuine mathematical objects in the way that elementary arithmetic seems to be.

So we have holism, plus some limited role for intuited anchor-points, but – importantly – minus indispensability arguments. A possibility, given that Parsons seems happy for mathematicians to freely exercise their Reason in exploring beyond what is fixed by the anchor-points, Parsons will end up not a hundred miles from a Maddy-like naturalism on the question of what constrains the game. But since he doesn't triangulate his position with respect to hers, it is difficult to tell.

§55 Set theory The book ends with one of its briefest sections, whose official topic is about the biggest – the question of the justification of set-theoretic axioms. But, reasonably enough, Parsons just offers here some remarks on how the case of justifying set theory fits with his remarks in the preceding sections.

First, on "rational intuition" again. We can work ourselves up to sufficient familiarity with ZFC for its axioms to come to seem intrinsically plausible – but such rational intuitions (given the questions than have been raised, by mathematicians and philosophers) "fall short of intrinsic evidence". Which is true but not very helpful.

What about Parsons's modified holism? In the case of set theory, is there "a dialectical relation of axioms and their consequences such as our general discussion of Reason would suggest"? We might suppose otherwise, given that (equivalents of) the standard axioms were already "essentially in place in Skolem's address of 1922". Nonetheless, Parsons suggests, we do find such a dialectical relation, historically in the reception of the axiom of choice, and perhaps now in continuing debates about large cardinal axioms, etc., where "the role of intrinsic plausibility" is much diminished, and having the right (or at least desirable) consequences are an essential part of their justication. But, Parsons concludes – the final sentence of his book – "apart from the purely mathematical difculties, many problems of methodology and interpretation remain in this area". Which is to end, to say the least, on a rather disappointing note of anti-climax!

Envoi

It's been a long and bumpy ride. Has it been worth it? Indeed so. The topics that Parsons is wrestling with here – how we are to regard the objects of arithmetic, how our knowledge of at least the simplest arithmetical truths is bound up with what we can imagine, the kind of thinking that takes us beyond the simplest cases – are absolutely central. Even if I find myself puzzled or positively disagreeing, I've learnt a good deal from the exercise of trying to follow the twists and turns of his thinking. Whether I've learnt enough is, of course, for others to decide!