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# A Generic Russellian Elimination of Abstract Objects

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**ABSTRACT**

In this paper I explore a position on which it is possible to eliminate the need for postulating abstract objects through abstraction principles by treating terms for abstracta as ‘incomplete symbols’, using Russell’s no-classes theory as a template from which to generalize. I defend views of this stripe against objections, most notably Richard Heck’s charge that syntactic forms of nominalism cannot correctly deal with non-first-orderizable quantifcation over apparent abstracta. I further discuss how number theory may be developed in a system treating apparent terms for numbers using these definitions.

## 1. INTRODUCTION: ABSTRACT OBJECTS AND ABSTRACTIONISM

As their name implies, we might describe abstract objects as those arrived at through a process of abstraction. An informal description of abstraction might be that it is a process whereby we recognize what is in common among a group of things sharing a certain kind of relevant similarity despite certain irrelevant differences. We overlook, ignore or ‘abstract away’ those differences, and what is left is then reified and identified as an abstract ‘thing’. This informal characterization is open to several criticisms. It almost makes it seem as if we could turn one thing, a specific, possibly concrete thing, into an abstract thing just by ignoring certain features of it, or perhaps even make more than one thing ‘the same’ by ignoring their differences. A thing is what it is regardless of what aspects of it we choose to focus on or pay attention to. Frege was particularly harsh in criticizing those who liked to describe a number such as three as arrived at by ignoring or putting aside the differences among different collections of three objects by thinking of the objects as ‘bare units’, indistinguishable from the units in any other collection of three. He accused them of adopting such absurd-on-their-face principles as the ‘principle of the non-differentiation of the different' (see, *e.g.*, [Frege, 1984]), whereby we simply 'decide' that different things are the same.

Some post-Fregean philosophical literature portrays abstract objects instead as those introduced, or at any rate introduce-able, by means of abstraction principles, those of the form:

$$\forall\tau\forall\mu(\rho(\tau) = \rho(\mu) \leftrightarrow E(\tau, \mu)). \tag{AP}$$

Here, $\tau$ and $\mu$ are variables of the same logical type (individuals, concepts, or properties of individuals, relations, *etc.*), and $E$ abbreviates some equivalence relation ascribable to values of that type — that is, a reflexive, symmetric, and transitive relation, which thereby 'partitions' the type into equivalence classes or groups all of which bear $E$ to each other and none of which bear $E$ to something outside the group. The functor $\rho$ here maps entities of the type of $\tau$ and $\mu$ to abstract objects; the abstraction principle guarantees that $\rho$ will map *relata* bearing $E$ to each other to the same abstract object, and *relata* not bearing $E$ to different abstract objects. Examples of principles of this form might be *the direction of line l and the direction of line m are the same iff l and m are parallel*, which introduces the abstract notion of a direction, or *the shape of object o and the shape of object p are the same iff o and p are congruent*, which introduces shapes as abstract objects. It is these sorts of abstract objects which I would like to focus on in what follows. For the purposes of this paper, I bracket the question as to whether or not other abstract objects must be recognized.

Interest in abstraction principles among philosophers of mathematics dates back at least to the 1880s; they are discussed in Frege's *Grundlagen* and in the writings of the Italian school led by Giuseppe Peano. More recently, Crispin Wright and his associates have advocated an 'abstractionist' form of logicism (see, *e.g.*, [Hale and Wright, 2001; Wright, 1983]). On their view, the core of mathematics rests on our acceptance of abstraction principles making use of purely logically specifiable equivalence relations. They hold that these principles can roughly be taken to have the logical and epistemological status of definitions: they merely explicate what certain terminology means. The most important example is what has come to be known as HP or Hume's Principle:

$$\forall F\forall G(\#(F) = \#(G) \leftrightarrow F \cong G). \tag{HP}$$

The number of $F$s is the number of $G$s iff the $F$s are equinumerous with the $G$s. 'Equinumerosity' is the equivalence relation holding between $F$ and $G$ just in case there is a 1–1 correlation between the $F$s and $G$s, which can be defined purely logically as follows:

$$F \cong G =_{\mathrm{df}} (\exists R)\{\forall x\forall y\forall z\forall w[Rxy \wedge Rzw \rightarrow (x = z \leftrightarrow y = w)] \wedge \\ \forall x[Fx \rightarrow \exists y(Gy \wedge Rxy)] \wedge \forall y[Gy \rightarrow \exists x(Fx \wedge Rxy)]\}.$$

In standard second-order logic, it is possible to derive all of the principles of Peano arithmetic from HP as the sole axiom over and above the usual logical axioms, with the functor $\#(\ )$ as the sole addition to the usual logical primitives. According to Wright, because (HP) can be seen as an analytic truth, specifying what we mean by talking of 'numbers' in logical terms, and this is evidence that arithmetic itself is an analytic outgrowth of logic. Further research has confirmed that similar abstraction principles may be used to derive the basic principles of real analysis and other areas of mathematics.

This kind of attitude towards abstraction principles and abstract objects, however, is not without its share of worries. HP, for example, is not a definition in the usual sense, and merely from the fact that Peano arithmetic can be derived from it shows that it has serious ontological consequences. It entails the existence of infinitely many things, infinitely many numbers. Is it really possible, by means of an analytic principle, to become aware of new objects merely by considering whether or not a higher-order relation holds between concepts? Such considerations naturally lead us back to general and ancient philosophical worries about *abstracta*: how can we become aware of, refer to, or discriminate, the causally inert? How can laws or principles about *abstracta* be brought to bear, as mathematics clearly can, on the concrete world? Is not postulating a realm of Platonic entities unnecessary, redundant, or puzzling? Abstractionist forms of Platonism have other, more specific, objections to consider as well. There are principles of the form AP, very similar to HP, which are self-contradictory or inconsistent. Most notoriously, there is the principle which states that the extension of $F$ is the extension of $G$ iff $F$ and $G$ bear to each other the equivalence relation of coextensionality:

$$\forall F \forall G(\text{ext}(F) = \text{ext}(G) \leftrightarrow \forall x(Fx \leftrightarrow Gx)). \qquad \text{(BLV)}$$

For all intents and purposes, this is Frege's Basic Law V, and it easily gives rise to the contradiction from Russell's paradox. A similar principle postulating order-types the same for all and only isomorphic relations gives rise to the Burali-Forti paradox. Even abstraction principles that are consistent on their own, such as HP, can often be shown to be inconsistent with other principles also consistent on their own.

It is not my purpose here to evaluate fully these forms of Platonism about abstract objects. There is nonetheless motivation for considering rival views that do not take abstraction principles, or the abstract objects they apparently introduce, at face value. This is not to say that all apparent talk about abstract objects should be rejected out of hand. It is clearly intelligible at a certain level to speak of directions, or shapes, or numbers — even extensions in most cases. Such discourse is clearly also in some way closely related to speaking of parallel lines, congruent objects, and so on. I here examine an alternative proposal for how to understand that relationship.

## 2. SYNTACTIC REDUCTIONISM AND HECK'S CRITICISM

A nominalist is one who denies the existence of abstract objects, or at least eschews commitment to them. If a nominalist is prepared to admit, as I just have, that not all discourse *apparently* about abstract objects is unintelligible, (s)he must be prepared to provide an explanation for how such discourse is to be understood. Nominalists may disagree with one another about how to understand the syntax and semantics of what appear to be terms for abstract objects. Following Richard Heck,[1] we may divide possible nominalist approaches into three groups:

> **Syntactic reductionism** is the view that what appear to be terms for abstract objects are not genuine syntactic terms at all; in a full or correct representation of the syntax of sentences in which they appear they can be shown not to be genuine individual terms at all.
>
> **Semantic reductionism** is the view that what appear to be terms for abstract objects are in fact genuine syntactic terms, and they do refer, but the objects they refer to are not abstract objects.
>
> **Nominalist fictionalism** is the view that what appear to be terms for abstract objects are genuine terms from a syntactic point of view, but they do not refer at all to anything, much like names in fictional discourse.

For the purposes of the present paper, I wish to put the latter two types of positions to the side. Nominalisms of these stripes deserve careful consideration and scrutiny, but we cannot discuss them fully here. The view I wish to explore is a form of syntactic reductionism, specifically one modeled on a generalization of Russell's no-classes theory. Although I am very sympathetic with this approach, I am not confident enough to endorse it definitively. I shall mention my reservations later on. Nevertheless, I think it is worth developing in greater detail than, to my knowledge, it has been, and I also wish to respond to certain, to my mind inconclusive, objections against syntactic reductionism, including one of Heck's.

In discussing this issue, Heck uses as example the type/token distinction for word inscriptions. ('Types' here is not being used in the sense of logical types.) It should be uncontroversial, and in some sense true, that the following line contains three word-inscription tokens of the same word type:

banana banana banana.

The tokens we may take to be concrete objects. We also postulate a relation, which I shall call $W$, which holds between tokens when and only when they inscribe what it is tempting to call 'the same word', *i.e.*, the same type. A Platonist abstractionist then, might endorse an abstraction principle such as the following:

$$\forall x \forall y(\text{type}(x) = \text{type}(y) \leftrightarrow Wxy). \tag{T}$$

I assume here that the relation $W$ can be characterized or defined without appeal to types as abstract objects. In this particular case, this assumption

[1] See [Heck, 2011, chap. 8]; Heck calls the third view simply 'fictionalism'; however, there are views in the philosophy of fiction according to which fictional names refer to abstract objects, or even to concrete objects. I have added the modifier 'nominalist' to discourage the suggestion that such a view is under consideration here.

might seem dubious. It is certainly difficult in English to attempt to state the exemplification conditions for $W$ without mentioning word types. This difficulty might be overcome with careful consideration of the nature of words and their inscriptions. However, nothing important for what follows turns on this particular example. A Platonist abstractionist assumes it is possible to have independent grasp of the equivalence relations involved in abstraction principles; this is necessary if postulating the principles can legitimately be seen as *introducing* the notion of the kind of abstract object in question. For abstraction principles most important for the philosophy of mathematics, such as HP, it would be difficult to deny that $\cong$ could be grasped without the notion of number, since it can be defined in terms of logical constants that do not seem to have a specifically mathematical meaning. If the same is not true for $W$, then the points made here nominally about $W$ would nonetheless hold for other equivalence relations.

Instead of postulating an abstraction principle such as T as a means of introducing a new kind of term, a syntactic reductionist would deny that expressions of the form 'type($a$)' (where $a$ is a particular word inscription, say the second occurrence of 'banana' above) need to be taken as genuine terms. If principle T is acceptable, the left half of the biconditional must have a more complicated form than it appears. Heck cites Russell's theory of descriptions [Heck, 2011, p. 181] as an example of a theory that denies that what appear to be genuine terms really are terms at all. While a sentence of the form '$F$(type($a$))' might appear to contain a term 'type($a$)' naming a type, the true logical form of '$F$(type($a$))' is more complicated, and perhaps should be interpreted as making a claim about all, some, or most of the tokens bearing $W$ to $a$. For example, perhaps to say that the word type 'Paris' has five letters is really to say that *all* (most? some?) word tokens bearing $R$ to a given token of 'Paris' have five letter inscriptions, and so on. Ideally, a syntactic reductionist should be able to provide a translation of all *apparent* discourse about types into more complicated discourse about tokens and relation $W$.

In the next section, I sketch such a theory. It is worth first considering one of Heck's objections to any form of syntactic reductionism that does not collapse into semantic reductionism. Heck concedes that for every first-order statement about types, the syntactic reductionist can provide a translation eliminating terms for types. His objection to syntactic reductionism involves statements about types using 'non-first-orderizable' quantifiers such as 'most', 'more', 'just as many', *etc.* It is perhaps plausible that for every predicate for types, there is a corresponding predicate for tokens; following Heck I shall use capitalized words for type predicates (*e.g.*, 'Short') and their uncapitalized counterparts for token predicates ('short'). Notice, however, that the claim that 'most word types are Short' cannot be analyzed in a way that makes it basically mean that 'most word tokens are short'. It is easy to imagine Short word types having more tokens than Long word types so that there might be more short word tokens even if there are fewer Short word types. Heck conjectures that the best (only?) recourse for the syntactic reductionist would be to count types by choosing representative tokens, one for each type, and analyze the claim that most word types are Short as claiming not that most tokens are short, but that most *representative* tokens are. But doing this in effect requires there to be genuine terms of the form '$\rho(a)$,' which for each token $a$, names the representative token which bears $W$ to $a$, and such terms in effect play the role of terms for types, at least formally speaking. The function $\rho$ maps tokens bearing $W$ to each other to the same thing, and tokens not bearing $W$ to each other to distinct things, and so this function would satisfy principle T. The reductionist may insist that these terms do not refer to abstract objects, but to take this line is to switch to semantic reductionism and to abandon syntactic reductionism. This is Heck's worry.

I believe that the most plausible forms of syntactic reductionism have other, not only more plausible, but in many ways more natural ways, to deal with the problem of 'non-first-orderizable' quantification over types. Those I have in mind would be formulated in terms of a higher-order base language. This complicates matters and deserves some further discussion. Through the bulk of his discussion, Heck seems to assume that the syntactic reductionist would be making use of a first-order language, perhaps supplemented by some additional not usually first-order quantifiers taken as primitive. This is perhaps in part because the nominalists Heck cites while setting up the issue are Quine and Goodman, who of course prefer their logic first-order only. And certainly a case could be made that higher-order logic and nominalistic philosophies make an uneasy partnership. But there are many possibilities within this logical space. A nominalist might prefer higher-order languages but have in mind a substitutional account of higher-order quantification, or perhaps have in mind the kind of plural understanding of higher-order variables suggested by Boolos [1984], and in neither case is it clear that the use of a higher-order language is by itself incompatible with nominalism. Or perhaps she is only a first-order nominalist, and is happy to admit *abstracta* as values of higher-order variables, content only to establish that there are no abstract *objects* or *individuals*. In any case, further argumentation would be needed to rule out the combination.

If we agree to consider approaches in this area of logical space, it unfortunately blurs the distinction between semantic and syntactic reductionism. Consider someone who holds that apparent terms for abstract objects are, despite their appearance, really substituends of higher-order variables, or higher-order 'terms'. There is much to be said about views in this vicinity. Types, perhaps, are really properties of, or concepts applicable to, tokens. Number terms, despite appearing to be singular terms for objects, are really quantifiers, or second-level concepts. Heck suggests that such a view would amount to a kind of semantic reductionism.[2] The issue is perhaps purely one of terminology — how precisely we delimit 'semantic reductionism' — but it seems misleading to

[2]See [Heck, 2011, p. 198, note 29]. There he mentions a view on which, *e.g.*, types would be understood as second-level concepts, restricted quantifiers of the form $\forall x(Wbx \rightarrow \ldots x \ldots)$; another natural view might identify the type of an inscription $b$ with the first-level concept which $x$ falls under just in case $Wbx$, though this may require

me to claim that this is just that. True, this position does not involve claiming that terms for abstract objects are not terms in any sense of the word 'term', but insofar as terms of different logical types are syntactically different (which would be the case for many, if not most, higher-order logics) then it seems that, to the extent that such a theory is a *first-order* nominalism, that nominalism takes the form of a syntactic nominalism, which is then combined either with a semantic higher-order nominalism, or perhaps even a higher-order *realism*. The first-order nominalism is syntactic, however.

The form of syntactic reductionism that most interests me is not one I am most tempted to describe as holding the simple view that apparent terms for abstract objects 'just are' higher-order terms. What I have in mind is modeled much more closely on Russell's theory of descriptions, whereupon certain 'apparent' terms are 'defined away' in the context of their use. The problem, however, is that even Russell's theory of descriptions, when applied in the context of a higher-order language (as of course Russell himself did!), is susceptible to being redescribed as a view according to which descriptions are terms all right, only not first-order terms. That is, Russell's theory of descriptions is not obviously different from the view that a description 'the $\varphi$' should be seen as standing for a certain kind of second-level concept or special restricted quantifier, true of first-level concept $G$ just in case there is one and only one $\varphi$ and $G$ holds of that thing, or formally:

$$(\imath x)\varphi x =_{\mathrm{df}} \lambda f.(\exists x(\forall y(\varphi y \leftrightarrow y = x) \wedge fx)).$$

Reading the $f$ in $f((\imath x)\varphi x)$ as the *argument* to this second-level function, along with $\beta$-reduction, yields Russell's familiar contextual definition:

$$f((\imath x)\varphi x) \leftrightarrow \exists x(\forall y(\varphi y \leftrightarrow y = x) \wedge fx).$$

Of course, not all proponents of the theory of descriptions would welcome this redescription.[3] One possible reason (but not the only one), would be the kind of first-order purism espoused by Quine. But if we are working in a higher-order base language, it becomes difficult to separate out the syntactic elimination of descriptive terms from their redescription as falling in a different syntactic and thus semantic category. If the theory of descriptions cannot be pointed to as a model for the kind of syntactic elimination of terms the syntactic reductionist is aiming for, then it becomes difficult even to understand what syntactic

taking on an extensional view of concepts to get something like T to work out. But the point made here applies either way.

[3] I here bracket the question as to whether or not Russell himself would accept this redescription. It does seem, however, that Russell's understanding of incomplete symbols as symbols which appear to be of one type but in fact are not really symbols of that type at all is important for understanding his diagnosis of the paradoxes. The fact that we are tempted to accept the kinds of reasoning that leads to the paradoxes shows that there are ways of writing certain symbols as if they were of one type whereas *either* they really have a different type *or* are not really unified symbols at all explains why something which is not well formed at all might at times appear to be. Thanks to a referee for this observation.

reductionism could possibly be. It seems as if we have simply defined syntactic reductionism out of existence rather than addressing it in any serious way. Therefore, I shall proceed as if theories modeling their elimination of terms on something like the theory of descriptions can count as syntactic reductionisms, even if such proposals are *technically* equivalent to ones whereupon apparent singular terms for abstract objects are taken instead as complex higher-order terms.

In my introduction, I sketched what a natural view about what the process of abstraction amounts to. The informal description of the process suggests to me a natural informal response a nominalist might give to Heck's worry. For the nominalist, when we think about a type, we are really thinking about tokens, but we 'abstract away' the differences between relevantly similar tokens. We treat tokens as counting 'as the same' when they bear $W$ to each other. In effect, we substitute $W$ for identity in our thinking. This is again loose talk, and cannot be taken too seriously. Identity is identity, and $W$ is $W$. But as an equivalence relation, perhaps $W$ is formally enough like identity that many of the concepts that can be defined or specified in terms of identity will have *analogues* formulated with $W$ instead. Heck concedes that the syntactic reductionist will be able to provide a replacement for first-order-analyzable claims about numbers of types. One way of doing this, would be to take the corresponding statements from a first-order treatment quantifying over types, replace the quantifiers with quantifiers for tokens, replace any predicates for types with the corresponding predicates for the tokens ('short' *vs.* 'Short', *etc.*) and in line with the present viewpoint, replace identity with $W$. So instead of writing 'there are at least two word types that are Short' as:

$$\exists x \exists y(\text{Short}(x) \wedge \text{Short}(y) \wedge x \neq y),$$

we instead get:

$$\exists x \exists y(\text{short}(x) \wedge \text{short}(y) \wedge \neg Wxy);$$

*i.e.*, there are at least two tokens which are short, and do not bear $W$ to each other. Perhaps because he is assuming that the syntactic reductionist would be making use of a first-order language, Heck seems to think that statements sensitive to the cardinalities of types that are not first-order-analyzable cannot be given a similar treatment. If we assume that the syntactic reductionist *can* make use of the higher-order *definitions* of such quantifiers as 'most', 'just as many', 'more', *etc.*, rather than taking these as primitive, matters are not so clear. For the sake of simplicity, I shall take 'just as many' to mean simply being equinumerous, or standing in a 1–1 correlation. Corresponding to this relation is a similar one where the equivalence relation $W$ between tokens of the same type takes over the role of identity:

$$F \cong_W G =_{\text{df}} (\exists R)\{\forall x \forall y \forall z \forall w[Rxy \wedge Rzw \rightarrow (Wxz \leftrightarrow Wyw)] \wedge \\ \forall x[Fx \rightarrow \exists y(Gy \wedge Rxy)] \wedge \forall y[Gy \rightarrow \exists x(Fx \wedge Rxy)]\}.$$

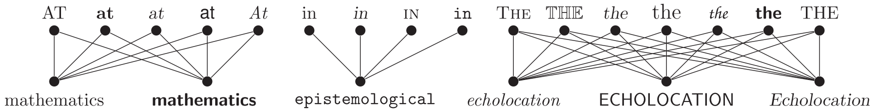


**Fig. 1.** A correlation between tokens of three long and three short word types.

We can read this as saying that the $F$s and the $G$s are alike in number when 'counting by $W$', *i.e.*, when using $W$ in the place of strict identity when determining what counts as 'the same'. The correlation $R$ here need not be 1–1 in the strict sense, but it must 'keep within type', so to speak. Each $F$-token may be paired up with more than one $G$-token, but all those $G$-tokens with which it is paired up must bear $W$ to each other, and *vice versa*. This is illustrated in Figure 1. At the top we have short word tokens, and at the bottom with have long word tokens. Although there are more short tokens than long ones, there are equally many types, as whenever the long same token on the bottom is linked with more than one token on the top (or *vice versa*), the tokens it is linked with bear $W$ to each other. This contrasts with Heck's solution in which one would ignore any but one (say, the first) of each type and focus only it. My approach poses no need for a mechanism whereby one token is chosen as 'representative' or especially important. It, and not Heck's, seems to me to be *the natural* way for a nominalist to make cardinality comparisons for types, and as near as I can tell, it works out simply to replace the claim that there are just as many Short types as Long types with the claim that the short tokens and long tokens are alike in number when 'counting by $W$', or '$\lambda x.\text{short}(x) \cong_W \lambda x.\text{long}(x)$'. Other non-first-orderizable quantifiers can be tackled in similar fashion. 'There are more $F$s than $G$s' can be taken to mean that the $G$s can be put in 1–1 correspondence with a subconcept of $F$, but the $F$s cannot be put in 1–1 correspondence with a subconcept of $G$. Simply replacing the '$=$'s in these definitions with '$W$'s would appear to me to do the trick for saying, *e.g.*, that there are more Long types than Short. 'Few' and 'most' are perhaps somewhat vague, but if we were to take 'most $F$s are $G$s' to mean that there are more $F$s-that-are-$G$ than $F$s-that-are-not-$G$, we have already sketched enough to make it clear how we might handle it.

This is not yet a complete answer to Heck. Still, it is the *kind* of response to his worry that I think it is most promising for a syntactic reductionist to give. In the next section I want to provide a more complete systematic methodology for eliminating reference to, and quantification over, types and other abstract objects for which a solution of this kind simply falls out of more general rules for eliminating apparent terms for abstract objects.

## 3. GENERALIZING ON RUSSELL'S NO-CLASS THEORY

An approach for eliminating terms for abstract objects similar to Russell's theory of descriptions can be found in Russell's own work. I refer of course to the other half of Russell's doctrine of 'incomplete symbols': his no-classes theory, the parallels of which to the theory of descriptions are stressed by Russell himself (in Chap. 3 of the introduction to *Principia Mathematica* (*PM*), for example). According to this theory, just as one cannot provide an explicit definition of the description 'the $F$' on its own, one cannot provide an explicit definition of an apparent term 'the class of $F$s' on its own. However, one *can* eliminate such apparent terms in the context of their use as follows:

$$\mathscr{A}(\hat{x}Fx) =_{\text{df}} \exists G(\forall x(Gx \leftrightarrow Fx) \wedge \mathscr{A}(G)). \quad \text{(NoClasses1)}$$

Putting aside for the moment complications brought on by ramification in Russellian logic — we are free to adopt simple type theory instead — it is useful to contrast this with introducing classes or extensions by the abstraction principle BLV. In both cases, we give pride of place to a certain equivalence relation between concepts (in Frege's terminology) or propositional functions (in Russell's), coextensionality. Frege suggests that when $F$ and $G$ bear this relation to each other, there is some 'thing' they have in common, and this thing is what the term for the class or extension refers to. The Russellian instead suggests that the appearance of a 'term' is illusory, and to make a claim apparently about a class is just to say that *there is* a coextensive function of which what we are saying about the class is true. Notice that what is really effected here, as Russell himself explains [*PM*, p. 75], is to take the context $\mathscr{A}(\ldots)$, which may not have been an extensional context for functions, and from it *manufacture* a context for $F$ to go into which is definitely extensional, thus guaranteeing that it holds regardless of which coextensive function is used to define the class. Class talk is thus a method for redescribing claims about functions in a way that makes what we are saying extensional.

I think perhaps it is likely that Russell chose existential quantification simply to increase the syntactic similarity of this contextual definition with the one for descriptions. As Carnap [1956, pp. 147–149] has stressed, Russell might just as well have interpreted the claim about the class of $F$s as asserting something about *all* functions coextensive with $F$ instead:

$$\mathscr{A}(\hat{x}Fx) =_{\text{df}} \forall G(\forall x(Gx \leftrightarrow Fx) \rightarrow \mathscr{A}(G)). \quad \text{(NoClasses2)}$$

This too has the effect of 'manufacturing' an extensional context from a (possibly) non-extensional one. (And since the mathematical project of *PM* made use of only extensional language anyway, swapping one contextual definition for another would not have damaged his project.)

When it comes to the other kinds of abstract objects a Platonist might be tempted to introduce by abstraction principles, Russell's own practice was to replace reference to them with apparent terms for the associated equivalence classes. Since, on the no-classes theory, terms for such equivalence classes are only 'apparent terms', it seems to me that Russell's approach counts as a syntactic reductionism (despite his higher-order logic). His form seems to me to be under-appreciated and often misunderstood, and definitely still worthy of further scrutiny and study.[4]

What I wish to explore here is another Russell*ian*, though not Russell's own, tack for syntactically eliminating reference to abstract objects. This approach takes the no-classes theory, and its dispatch of the need for an abstraction principle like BLV as programmatic or exemplary as an instance of a generic method for providing contextual definitions for doing away with abstract objects and their associated abstraction principles. For every abstraction principle AP, one may make use instead of a contextual definition that defines away apparent terms of the form '$\rho(\mu)$' by means of existential claims to the effect that something bears the appropriate equivalence relation to $\mu$ and reinterprets the claim as about it, or schematically (— I dub this 'ES' for 'elimination schema'):

$$\mathscr{A}(\rho(\mu)) =_{\mathrm{df}} (\exists\tau)(E(\tau,\mu) \wedge \mathscr{A}(\tau)). \tag{ES}$$

In the case of cardinal numbers as introduced by the abstraction principle HP, a neo-Russellian might instead provide a contextual definition of '$\#(\dots)$' as follows:

$$\mathscr{A}(\#(F)) =_{\mathrm{df}} \exists G(G \cong F \wedge \mathscr{A}(G)). \tag{NoNumbers}$$

Just as Russell's contextual definition of apparent terms for classes/extensions took a context and manufactured an extensional context, what this does is take a context and manufacture a cardinality-congruent context. Something holds of *the number of $F$s* just in case there is a equinumerous concept of which that something holds. (Or we might give the universally quantified version instead.) Clearly, then whatever is true of the number of $F$s will also be true of the number of $F'$s if the $F$s and $F's$ are equinumerous.

It should be noted then that the symbol '$\#(F)$' could only appear in a position that would allow a first-level function name. There is then no possible interpretation for a sentence such as 'Caesar $= \#(F)$', dissolving the so-called 'Julius Caesar problem' in the philosophy of mathematics. However, as '$\#(F)$' may appear to be of the same *shape* as a term for an object, one has an explanation of why some might be tempted to raise the issue, despite its meaninglessness when fully analyzed.

In the case of 'types' from the type/token distinction, where $W$ again is the equivalence relation sorting the tokens into types, the associated contextual definition of 'type(...)' runs as follows:

$$\mathscr{A}(\mathrm{type}(x)) =_{\mathrm{df}} \exists y(Wyx \wedge \mathscr{A}(y)). \tag{NoTypes1}$$

This takes a context, and manufactures a type-congruent one. An apparent claim about 'the type of $x$' is really a quantified claim about tokens bearing $W$

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[4]And indeed, I think Russell's views possess some distinct advantages over more recent logicisms, as I argue in my [2012]; in [Klement, 2010], I argue that certain widespread objections to Russell's views are based on misunderstandings.

to $x$. Again, we would be free to use a universally quantified version instead.[5]

$$\mathscr{A}(\text{type}(x)) =_{\text{df}} \forall y(Wyx \to \mathscr{A}(y)). \tag{NoTypes2}$$

There may be some superficial reasons to prefer (NoTypes2), which I will discuss in Section 5 below. There are also some additional possibilities (involving the addition of modal operators, perhaps) in the vicinity also worth considering. However, in order to preserve an easy comparison with Russell's no-classes theory, I shall proceed assuming we have adopted (NoTypes1). One annoying complication is that, since we have not defined 'type($x$)', but only '$\mathscr{A}(\text{type}(x))$', as in both Russell's no-classes theory and the theory of descriptions, this notation gives rise to scope ambiguities. For example, '$\neg F(\text{type}(a))$' may be read where the 'incomplete symbol' has narrow or secondary scope: '$\neg\exists y(Wya \wedge Fy)$', or it may be read with primary or wide scope '$\exists y(Wya \wedge \neg Fy)$'. Care will thus need to be taken to disambiguate. I shall adopt the convention, similar to Russell's, that unless otherwise indicated, the scope of 'type($a$)' is to be the narrowest possible, and if there is more than one such 'incomplete symbol' in what would otherwise be an atomic statement, the scope of those on the left are greater than those on the right.

How then do we analyze type($x$) = type($y$)? Given the scope convention I just adopted, how we read this depends on whether '=' is a primitive predicate or a defined one. If it is primitive, then we have:

$$\exists z(Wzx \wedge \exists w(Wwy \wedge z = w)). \tag{ID1}$$

If $x = y$ is defined as, say, $\forall F(Fx \to Fy)$, then narrower scopes are possible; so we get:

$$\forall F(\exists z(Wzx \wedge Fz) \to \exists z(Wzy \wedge Fz)). \tag{ID2}$$

Either way,[6] from the assumption that $R$ is an equivalence relation, we will be able to *derive* what *looks like* the abstraction principle T:

$$\forall x \forall y(\text{type}(x) = \text{type}(y) \leftrightarrow Wxy). \tag{T$'$}$$

Of course, however, this has a more complicated syntax than it appears to have: the left side of the biconditional is not an identity statement between genuine terms, but is unpacked as (ID1) or (ID2) depending on how identity is treated. This gives us the desired identity conditions for types, making it clear in what sense $W$ can operate similarly to identity.

We also want ways of quantifying over types. We can take the way Russell introduces defined quantifiers for classes in *PM* *20.07 as our guide. He introduces what look like new variable letters $\alpha$, $\beta$ (and associated quantifiers) to

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[5]This would bring us very close to a suggestion once made by Goodman [1972].

[6]If we were to opt for (NoTypes2), there would be some pressure to adopt something more like the second method for dealing with identity, since $\forall F(\forall z(Wzx \to Fz) \to \forall z(Wzy \to Fz))$ is equivalent with $Wxy$ when $W$ is an equivalence relation, but $\forall z_1(Wz_1x \to \forall z_2(Wz_2y \to z_1 = z_2))$ would not be.

range over classes. But all such notation is in principle eliminable.

$$\forall \alpha \mathscr{A}(\alpha) =_{\mathrm{df}} \forall F \mathscr{A}(\hat{x}Fx).$$

The corresponding move for our no-types theory would be to use, say $t$ and $s$ as special variables for types, eliminable as follows:

$$\forall t \mathscr{A}(t) =_{\mathrm{df}} \forall x \mathscr{A}(\mathrm{type}(x))$$
$$\exists t \mathscr{A}(t) =_{\mathrm{df}} \exists x \mathscr{A}(\mathrm{type}(x)).$$

Care needs to be taken with scope ambiguities here too. The $\mathscr{A}(\ldots)$ here is schematic, and $\mathscr{A}(t)$ may contain more than one occurrence of $t$. My intention is that one would first unpack this definition, and then, for *each* occurrence of 'type($x$)', unpack it with the narrowest scope possible. So if $\mathscr{A}$ is complex, even while adoping (NoTypes1), $\forall t \mathscr{A}(t)$ is not necessarily the same as:

$$\forall x \exists y (Wyx \wedge \mathscr{A}(y)).$$

For example, if we use $\forall F(Fx \rightarrow Fy)$ as our definition of identity, then:

$$\forall t(t = t)$$

first becomes:

$$\forall x \forall F(F(\mathrm{type}(x)) \rightarrow F(\mathrm{type}(x))),$$

and then that becomes:

$$\forall x \forall F(\exists y(Wyx \wedge Fy) \rightarrow \exists y(Wyx \wedge Fy)).$$

(Similar snafus in the order of unpacking contextual definitions are familiar to serious scholars of *PM*.) 'There are at least two Short types' can be written in an abbreviated form in a way that looks very similar to how the Platonist might write it:[7]

$$\exists t \exists s(\mathrm{short}(t) \wedge \mathrm{short}(s) \wedge t \neq s).$$

However, the simplicity is only apparent. This unpacks to:

$$\exists x \exists y(\exists z(Wzx \wedge \mathrm{short}(z)) \wedge \exists z(Wzy \wedge \mathrm{short}(z)) \wedge \mathrm{type}(x) \neq \mathrm{type}(y)).$$

The last conjunct here, written in full, will be the negation of something either like (ID1) or like (ID2) depending on the treatment of identity. This conjunct will be equivalent, by T$'$, to $\neg Rxy$, and so assuming both that $W$ is an equivalence relation, and that whenever $x$ is short, so is anything to which it bears $W$, the above is equivalent to our earlier, more 'natural':

$$\exists x \exists y(\mathrm{short}(x) \wedge \mathrm{short}(y) \wedge \neg Wxy).$$

Non-first-orderizable quantification seems to be no problem either. I am again here presupposing that the syntactic nominalist can appeal to the standard

[7] There is no need to use 'Short' here, the contextual definition will produce the needed switch to a type-congruent context.

higher-order definitions of such quantifiers as *just as many* and *most*. We can use our special *eliminable* variables $t$, $s$ as before. We state a version of equinumerosity changed from the normal definition only to use these variables:

$$F \cong^{*} G =_{\mathrm{df}} (\exists C)\{\forall t \forall s \forall t' \forall s' [Cts \wedge Ct's' \rightarrow (t = t' \leftrightarrow s = s')] \wedge \\ \forall t[Ft \rightarrow \exists s(Gs \wedge Cts)] \wedge \forall s[Gs \rightarrow \exists t(Ft \wedge Cts)]\}.$$

With the above treatment of $\forall t$ and $\exists s$, etc, this could also be stated:

$$F \cong^{*} G =_{\mathrm{df}} (\exists R)\{\forall x \forall y \forall z \forall w[R(\mathrm{type}(x), \mathrm{type}(y)) \wedge R(\mathrm{type}(z), \mathrm{type}(w)) \rightarrow \\ (\mathrm{type}(x) = \mathrm{type}(z) \leftrightarrow \mathrm{type}(y) = \mathrm{type}(w))] \wedge \\ \forall x[F(\mathrm{type}(x)) \rightarrow \exists y(G(\mathrm{type}(y)) \wedge R(\mathrm{type}(x), \mathrm{type}(y)))] \wedge \\ \forall y[G(\mathrm{type}(y)) \rightarrow \exists x(F(\mathrm{type}(x)) \wedge R(\mathrm{type}(x), \mathrm{type}(y)))]\}.$$

And to write it even more fully, each 'apparent term' of the form 'type($x$)' should be eliminated using the smallest possible scope.

The adequacy of this as a way of comparing the cardinalities of 'types', as a response to Heck's worry, is evident from the fact that this way of comparing cardinalities for types is equivalent to the 'counting by $W$' method mentioned in the previous section as a 'natural' response for the syntactic nominalist to give. That is, we can prove:

$$F \cong^{*} G \leftrightarrow F \cong_{W} G.$$

A proof of this result is briefly sketched in Appendix A. It uses nothing other than standard second-order logic, the definitions given earlier, and the fact that $W$ is an equivalence relation. $\cong^{*}$ can then be used to analyze 'more'-type, 'most'-type, *etc.* quantifiers for types. Nothing in the result makes use of anything peculiar to (NoTypes1); the same result would hold for any instance of elimination schema ES as a means for eliminating abstract objects, so long as $W$ above is replaced with whatever equivalence relation $E$ is involved in the instance of ES (and, if needed, the variables replaced by variables for the appropriate logical type for *relata* to $E$).

## 4. NUMBER-ELIMINATIVE ARITHMETIC

If what I have been arguing is correct, the form of syntactic eliminativism that uses contextual definitions for apparent terms for abstract objects of the form of ES can provide a general response to Heck's worries about non-first-orderizable quantification. At this point, I would like to sketch how Peano arithmetic can be developed in a system that makes use of (NoNumbers) to eliminate apparent terms for numbers. This is meant as an example to test how far such an approach can go in providing a full replacement for realist or Platonist accounts of abstract objects. Assuming we have a second-order identity relation between concepts, $F = G$, which we could perhaps define using third-order quantification, *i.e.*,

$\forall Q(Q(F) \rightarrow Q(G))$, (NoNumbers) allows us to derive what *looks like* HP:

$$\forall F \forall G(\#(F) = \#(G) \leftrightarrow F \cong G). \tag{HP$'$}$$

However, the left side of the biconditional is not a simple identity between two genuine terms, even higher-order terms. Assuming identity between concepts '$F = G$' is defined as '$\forall Q(Q(F) \rightarrow Q(G))$', '$\#(F) = \#(G)$' unpacks as:

$$\forall Q(\exists H(H \cong F \wedge Q(H)) \rightarrow \exists H(H \cong G \wedge Q(H))).$$

We will need special eliminable quantifiers for numbers. We use $n$, $m$ (with primes if need be) in much the same way that we used $t$, $s$, *etc.*, for types. Such quantification is in principle eliminable in favor of second-order quantification:

$$\forall n \mathscr{A}(n) =_{\text{df}} \forall F \mathscr{A}(\#(F))$$
$$\exists n \mathscr{A}(n) =_{\text{df}} \exists F \mathscr{A}(\#(F)).$$

Again, here the pseudo-term '$\#(F)$' may occur in $\mathscr{A}(\#(F))$ any number of times; after unpacking this definition, we unpack each of these occurrences using the narrowest scope possible in the context. We may define 0 as the number of non-self-identical things:

$$0 =_{\text{df}} \#(\lambda x.x \neq x).$$

Of course, '0' is no more a genuine term than '$\#(\lambda x.x \neq x)$'; it would be more explicit to put:

$$\mathscr{A}(0) =_{\text{df}} \mathscr{A}(\#(\lambda x.x \neq x)) =_{\text{df}} \exists F(F \cong \lambda x.x \neq x \wedge \mathscr{A}(F)).$$

Next we define the relation of immediate predecessor:

$$m\ P\ n =_{\text{df}} \exists F[\#(F) = n \wedge \exists x(Fx \wedge \#(\lambda y.(Fy \wedge y \neq x)) = m)].$$

The '$n$' and '$m$' here are placeholders for pseudo-terms of the form $\#(F)$; in the context of application they will also be replaced by such expressions, or by variables eliminable by their use. To say that $m$ precedes $n$ then means that there is a concept $F$ where $n$ is the number of things that are $F$, something $x$ is $F$ and the number of things that are $F$ but not $x$ is $m$. A natural number can be defined as a number having all properties $Q$ held by 0 and hereditary with respect to the relation $P$.

$$\text{N}(n) =_{\text{df}} \forall Q[Q(0) \wedge \forall m \forall m'(Q(m) \wedge m\ P\ m' \rightarrow Q(m')) \rightarrow Q(n)].$$

The variable $Q$ here is a (third-order) variable for concepts of concepts; this is required, given the way number pseudo-terms are eliminated: the context $Q(n)$ will become something of the form $\exists G(G \cong F \wedge Q(G))$.

With these definitions, four of the five Peano 'axioms' can be derived as theorems in standard third-order predicate logic with no non-logical axioms at all, *viz*:

$$\mathrm{N}(0); \tag{PP1}$$

$$\neg\exists n\ n\ P\ 0; \tag{PP3}$$

$$\forall m \forall m' \forall n (m\ P\ n \wedge m'\ P\ n \rightarrow m = m'); \tag{PP4}$$

$$\forall Q[Q(0) \wedge \forall m \forall m' (\mathrm{N}(m) \wedge Q(m) \wedge m\ P\ m' \rightarrow Q(m')) \rightarrow \forall n(\mathrm{N}(n) \rightarrow Q(n))]. \tag{PP5}$$

These state, roughly and respectively, that zero is a natural number, that no number precedes 0, that no two distinct numbers precede the same number, and that whatever is true of 0 and true of the successor of a natural number whenever it is true of that number is true of all natural numbers (the principle of mathematical induction). The remaining Peano 'axiom' informally asserts that every natural number has a unique successor which is also a natural number. Two 'pieces' of this assertion can be proven purely logically, *viz.*, that successors of natural numbers are always natural numbers, and that no natural number has more than one successor.

$$\forall m \forall n (\mathrm{N}(m) \wedge m\ P\ n \rightarrow \mathrm{N}(n)); \tag{PP2a}$$

$$\forall m \forall n \forall n' (m\ P\ n \wedge m\ P\ n' \rightarrow n = n'). \tag{PP2b}$$

All that remains to be proven is that every natural number has at least one successor:

$$\forall m(\mathrm{N}(m) \rightarrow \exists n\ mPn). \tag{PP2c}$$

The quantifiers here for numbers are eliminated in terms of quantifiers for concepts, concepts applicable to genuine individuals, the values of the genuine first-order variables. If there is no concept of a given cardinality, then the corresponding 'number' cannot be proven to exist. Because number terms are not genuine terms, and not substituends of first-order variables, we cannot use numbers themselves to prove the existence of concepts of the appropriate cardinalities. Thus we must assume that there are concepts of every finite cardinality; in effect, we must assume an axiom of infinity. This can take the form of assuming that no concept whose number is a natural number applies to all things; for such a concept, there will always be something *not* falling under it.

$$\forall F(\mathrm{N}(\#(F)) \rightarrow \exists x \neg Fx). \tag{Inf}$$

(PP2c) can be proven from (Inf), and thus the whole of Peano arithmetic is interpretable in the system containing standard third-order predicate logic with (Inf) as the sole non-logical axiom. Sketches of proofs of all these results are given in Appendix B.

The need for a principle of infinity here is quite similar to the need for it in Russell's own construction of numbers as classes of classes in the no-classes theory, or in the sort of theory that defines numbers as numerically definite quantifiers, or concepts applicable to concepts just in case those concepts have a certain cardinality. On these ways of proceeding, numbers are not values of first-order quantifiers, the 'terms' flanking the identity on the sides of HP′ (or similar) are not valid substituends of such variables. Hence, they cannot be used to prove the existence of concepts applicable to more and more things, in the 'bootstrapping' method suggested by Frege in *Grundlagen* (§79) and endorsed by contemporary proponents of the stronger version of HP. The bootstrapping approach is able to prove that for every natural number $n$, there is a concept whose cardinality is the next natural number, *viz.*, the concept of being a number in the number sequence up to and including $n$. There is 1 number up to and including 0, 2 numbers up to and including 1, 3 numbers up to and including 2, and so on. But if numbers are not 'things', not values of the individual variables, this method cannot be used. This certainly may seem like a disadvantage to those attracted to logicism, and (Inf) does not seem like a logically necessary principle. However, it may also be seen as an advantage, as the consequence that the domain must be infinite is also often given as a reason for rejecting HP as a logical or analytic principle.

It might be argued, however, that the failure of the bootstrapping method in the treatment of numbers considered here points to another weakness: that it is simply too weak to 'count' numbers, or at least too weak to count numbers using the 'same' numbers as used to count individuals. This objection is too quick. First, it should be noted that numbers are generally apiece with quantification. Anyone employing a standard higher-order logic accepts that quantifying over individuals is distinct from quantifying over concepts, and this is usually not seen as problematic, at least not by proponents of higher-order logic. By employing the methodology discussed for types, we can similarly make non-first-orderizable quantified judgments for numbers, and even develop numbers of numbers, and so on. Indeed, by making use of heterogeneously typed relations, we can even state that a concept of one type is equinumerous with that of another type.[8] Suppose that $Q$ is a variable for concepts of concepts. By using heterogeneous functions, we might introduce a pseudo-term '$\#(Q)$' defined using the same type quantifiers as those used for pseudo-terms of the form '$\#(F)$', *i.e.*, instead of:

$$\mathscr{A}(\#(Q)) = \exists Q'(Q' \cong Q \wedge \mathscr{A}(Q')),$$

we could use the definition:

$$\mathscr{A}(\#(Q)) = \exists G(G \cong' Q \wedge \mathscr{A}(G)),$$

where the '$\cong'$' here postulates a heterogeneous 1–1 function between the $Q$s and the $F$s:

$$G \cong' Q =_{\mathrm{df}} (\exists R)\{\forall x \forall x' \forall F \forall F'[R(x, F) \wedge R(x', F') \to (x = x' \leftrightarrow F = F')] \wedge \\ \forall x[Gx \to \exists F(Q(F) \wedge R(x, F))] \wedge \forall F[Q(F) \to \exists x(Gx \wedge R(x, F))]\}.$$

[8]For more on this general strategy, see [Landini, 2006].

Notice that pseudo-terms of the form '$\#(Q)$' would be interpretable when placed in the same positions as those of the form '$\#(F)$'. This would allow us to interpret identity statements between numbers formed from concepts of different types:

$$\#(F) = \#(Q).$$

Expanding only slightly further would allow us to speak of numbers of concepts applicable to numbers and even compare them with numbers of concepts applicable to things:

$$\mathscr{A}(\#_n(F(n))) = \exists G(G \cong^{*\prime} F \wedge \mathscr{A}(G)).$$

The relation $\cong^{*\prime}$ involves partly making the kinds of changes we made to $\cong$ to get $\cong'$ and partly making the kind of change involved in moving from $\cong$ to $\cong^*$ discussed in Section 3 (except with number quantifiers instead of type quantifiers).

$$G \cong^{*\prime} F =_{\text{df}} (\exists R)\{\forall x \forall x' \forall n \forall n'[R(x,n) \wedge R(x',n') \rightarrow (x = x' \leftrightarrow n = n')] \wedge \\ \forall x[G(x) \rightarrow \exists n(F(n) \wedge R(x,n))] \wedge \forall n[F(n) \rightarrow \exists x(G(x) \wedge R(x,n))]\}.$$

This allows us to speak of numbers of numbers in a way that permits their being identified and/or distinguished from numbers of anything else. It still does not provide a method of 'bootstrapping' without an explicit postulation of infinity, but this is perhaps how it should be. One might object that without being able to provide a logical proof that there is an infinity of objects, one is left without a logical proof of the usual principles of number theory, and thus one is forced to abandon logicism. One response might be to accept this: if a correct philosophical account of abstract objects is incompatible with logicisms, perhaps so much the worse for logicism. Another response, however, would be to question whether logicism requires that we obtain the principles of number theory in their usual form. This raises issues we cannot fully settle here.[9]

## 5. ORDINARY LANGUAGE AND RELATED PHILOSOPHICAL CONCERNS

Philosophical and reductive analyses of numbers or other abstract objects are sometimes rejected on the grounds that they validate claims which seem clearly false, or perhaps meaningless, according to our ordinary-language intuitions about numbers and their properties. Famously, Paul Benacerraf [1965, p. 54] tells a story about two people, having learned two different set-theoretic reductions of the natural numbers, quibbling over whether or not 3 belongs to 17. Dedekind, apparently, (see his [1932, pp. 489–490]) rejected a set-theoretic definition of real numbers of the grounds that questions one could ask of sets

[9] For a defense of a logicist position which calls into question the need for a principle of infinity, and for my own thoughts, see [Landini, 2007] and [Klement, 2012].

seemed out of place when asked of the reals.[10] Clearly, the analysis I have offered for discourse about abstract objects in terms of elimination schema (ES) may give rise to similar kinds of concerns, and might form the basis for an objection that this is 'clearly not what we mean' when we speak of numbers, or of word types, and so on. It is plausible to suppose that we do not really mean that 'something bearing $W$ to such-and-such a token is three letters long' when we inquire whether or not the word type 'cat' is three letters long. Indeed, someone might argue that the present analysis clearly provides the wrong truth conditions for say 'Green(type($w$))' and 'Red(type($w$))', both of which might come out as true for the same word type if some tokens of that type are green and some red.

To those who worry about such things, matters might be improved if we adopted (NoTypes2) instead, so that all tokens would have to be red for 'Red(type($w$))' to come out as true. Still, that might be accidentally true for words rarely written. Someone concerned with more closely matching our ordinary intuitions might suggest adding a modal operator into the mix, so that 'F(type($w$))' would only come out as true if *necessarily* all the appropriate tokens were $F$. Indeed, a rough sketch of what we seem to be willing to attribute to types, or other abstract objects, are those traits which follow *necessarily* about their tokens in virtue of the defining equivalence relation. Depending on how stringent we take the exemplification conditions for $W$ to be, it may or may not be required that all tokens of 'cat' be properly spelled; if so, then all tokens will have three-letter inscriptions. This is perhaps why we seem willing to accept that it is a property of the word type 'cat' that it be three letters long, whereas being red would never be considered a property of the type, since clearly the color of the inscription is not going to feature into the conditions for the holding or non-holding of $W$. Teasing out exactly how our thought and ordinary-language discourse about types works would be a very difficult empirical endeavor, one which I surmise has hardly begun.

However, I think there are limits to how relevant such investigations would be for evaluating the importance of syntactic reductionisms of the sort considered in the previous sections. The philosophically interesting issue — at least to my mind — is not the question of how ordinary-language and mathematical and other abstract cognitions *do* work, but how a language in general *could* work. I would propose that we look upon the kinds of analyses here as a kind of *simplified model* of how it is that talk about the abstract could be introduced or made possible. In science, one often creates simplified

[10] For discussion of this and related issues, see [Shapiro, 2006]. Russell himself at times rejected the importance of such concerns for his own logicist analyses — see, *e.g.*, [Russell, 1931, §242] — though as his career developed he gave varying weight to the importance of matching ordinary thought or language when giving philosophical analyses. Forthcoming secondary work by James Levine nicely sketches the different conceptions of analysis, sometimes in tension with one another, used by Russell throughout his career.

environments — those without friction and air resistance — to test a general theory without the full complications of the physical environments we are used to. Ordinary language, with everything going on in it, with the rich diversity of syntactic and semantic structures it offers, with its tight integration with complicated cognitive processes, including perhaps an equally complicated 'language of thought', is for purely practical reasons a difficult context in which to test general metaphysical theses. Predicate logic, first- or higher-order, is by comparison a much simpler and better-governed system, and yet one known to have rich expressive power. By showing how things that look like terms for abstract objects can be introduced by means of syntactic conventions, but without officially adding any genuinely new syntactic or semantic types, we show something about the possibilities implicit in any language rich enough to quantify over, identify, and differentiate, things and concepts applicable thereto. So long as certain equivalence relations can be captured, the ability, as it were, to speak about mathematical or other abstract objects emerges naturally. What is more, it does so without the need to postulate any new semantic values, or entities in the world, corresponding to the syntactic conventions.

It is only of an artificial language that one could definitely claim that it embodies a form of syntactic reductionism. In the above, I *stipulated* how to unpack pseudo-terms such as 'type($w$)' and '$\#(F)$' contextually, and thereby stipulated that these are not to be taken as genuine object-language terms. In the study of natural language, where one is not free to 'stipulate' one's syntactic conventions, it is very unclear under what conditions someone could admit that a phrase 'behaves' very much like a term, and yet is not one. Nonetheless, the question that seems of philosophical interest to me is not whether or not ordinary language embodies syntactic reductionism, but whether or not anything important is lost (and indeed, perhaps, whether anything is gained) in number theory or other abstract science if we choose to employ an artificial language that decidedly does. That such a language may produce 'extra baggage' such as making 'Red(type($w$))' come out as true seems uninteresting so long as the theory is capable of capturing what is theoretically important about a theory of word types.

We are of course also free (along the lines of a proposal found in the work of Hodes — see his [1990]) to construct a language in which there are numerals which are syntactically of the same type as other terms, but interpret sentences containing such terms in a way that takes them not as referring to objects, but instead, as expressing higher-order truths in a syntactically different way. And if it is possible to develop an object language with such numerals, then it is also possible to develop a metalanguage with its own numerals. In that metalanguage, it will then be natural to use the verb 'refer' between expressions that mention object-language numerals and its own numerals, *e.g.*, ' "2" refers to two'. None of this shows, however, that such languages provide a better metaphysical picture of reality than rival languages which eschew such terms in favor of eliminable pseudo-terms. Moreover, it does not seem to me to be philosophically important whether empirical research into ordinary language reveals it to be more like one kind of language or more like the other.[11] The issue of which is to be preferred remains open.

It is tempting to go further and cite Occam's razor in favor of some kind of nominalist approach. An abstract science that treats its special expressions in an eliminative, nominalist, way is metaphysically simpler than one that postulates *abstracta* as genuine references of its terms, or as values of ontologically committing variables. But this is perhaps to take too narrow a view of what is involved in the intellectual virtue of 'simplicity'. It is not simply a matter of how many, or how many kinds, of things are presupposed. It may have to do with the simplicity and unity of the basic principles assumed in the theory, how well integrated such a theory is with other theories, and so on. Tracing how well the eliminative theories sketched here fare on these matters is a difficult task, and I cannot provide a full evaluation here. However, I think the advantages, even the advantages regarding simplicity, are not limited to a reduction in what is quantified over. The approach, I believe, simplifies our epistemology in so far as we do not need an account of special *de re* knowledge of abstract objects. It simplifies our semantic theories, in so far as we do not need a special theory of reference for such terms. Depending on the abstract entities eliminated, it may simplify our logic, as certain abstract objects (extensions, ordinal numbers) are prone to lead to contradictions if taken realistically. It may simplify our account of the applicability of abstract studies to the concrete world, as our account of abstract objects resolves such discourse into quantification over concrete entities (and concepts applicable to them).

But there are costs and disadvantages as well, which is why even I am reluctant to put forth these analyses with any great confidence. One of the greatest disadvantages, as I see it, with this approach is that, because abstract objects are introduced by means of quantifiers over concrete objects which is some sense 'participate' in them, we are left without a view of the 'unparticipated-in' abstract notions. For example, (NoTypes1) provides no room for word types of which there are no tokens. It is not hard to imagine, *e.g.*, rare verbs for which certain conjugations have never been used; yet such conjugations still exist as words, someone might argue. Our account of numbers does not leave room for numbers of cardinalities where no concepts apply to that number of things; this could be seen as the reason (Inf) was needed to capture the basic laws of finite numbers. I think these concerns are genuine, and here I offer only the *tu quoque* reply that a similar difficulty would arise for a Platonist who wished to introduce abstract objects by means of abstraction principles of the form (AP) as well. While HP may help in proving certain concepts of finite cardinality to exist, it does not get all the cardinalities discussed in, *e.g.*, contemporary set theory. Taking T as an axiom does not help at all with types of which there

[11] In the philosophical literature, Hofweber's work, *e.g.*, [2005], takes the empirical questions somewhat seriously, with results neither fully at odds nor fully in accord with the approach suggested here.

are no tokens. There are many historical precedents of these worries, from those surrounding Aristotle's *in re* theory of universals onward.

## APPENDICES

### Proof of Equivalence of $\cong^*$ and $\cong_W$

For $\rightarrow$, suppose that $F \cong^* G$. There is then an $R$ such that

(a) $\forall x \forall y \forall z \forall w[R(\text{type}(x), \text{type}(y)) \wedge R(\text{type}(z), \text{type}(w)) \rightarrow (\text{type}(x) = \text{type}(z) \leftrightarrow \text{type}(y) = \text{type}(w))]$,
(b) $\forall x[F(\text{type}(x)) \rightarrow \exists y(G(\text{type}(y)) \wedge R(\text{type}(x), \text{type}(y)))]$, and
(c) $\forall y[G(\text{type}(y)) \rightarrow \exists x(F(\text{type}(x)) \wedge R(\text{type}(x), \text{type}(y)))]$.

We will show that the existential posit of $F \cong_W G$ is satisfied by the relation $R'$ holding between $x$ and $y$ just in case $\exists x' \exists y'(Wxx' \wedge Wyy' \wedge Rxy)$. Firstly, suppose for arbitrary $x$, $y$, $z$, $w$ that $R'xy \wedge R'zw$. Then there is an $a$, $b$, $c$, and $d$ such that $Wxa$, $Wyb$, $Wzc$, $Wwd$, $Rab$, and $Rcd$. Thus $R(\text{type}(x), \text{type}(y))$ and $R(\text{type}(z), \text{type}(w))$. By (a) above, $\text{type}(x) = \text{type}(z) \leftrightarrow \text{type}(y) = \text{type}(w)$. Suppose that $Wxz$. By T$'$, $\text{type}(x) = \text{type}(z)$, whence $\text{type}(y) = \text{type}(w)$, and again by T$'$, $Wyw$. Hence $Wxz \rightarrow Wyw$ and by a parallel argument, $Wyw \rightarrow Wxz$. This establishes the first conjunct of what needs to be shown for $R'$. For the second, for arbitrary $x$, suppose $Fx$. Because $W$ is reflexive, it holds that $Wxx$ and thereby $\exists y(Wyx \wedge Fy)$, and thus $F(\text{type}(x))$. By (b) above, there is some $a$ such that $G(\text{type}(a)) \wedge R(\text{type}(x), \text{type}(a))$. There is then some $a'$ such that $Wa'a$ and $Ga'$. Also, there are a $b$ and $c$ such that $Wbx$, $Wca$, and $Rbc$. By the symmetry of $W$, $Wxb$, and by its symmetry and transitivity, $Wa'c$. Hence, $R'xa'$. Because $Ga'$, $\exists y(Gy \wedge R'xy)$ for arbitrary $Fx$. This establishes the second conjunct of what needs to be shown for $R'$. By similar reasoning using (c) instead of (b), we can establish the third conjunct. Thus, $F \cong_W G$.

For $\leftarrow$, suppose that $F \cong_W G$. This means that there is a relation $R$ such that

(a) $\forall x \forall y \forall z \forall w[Rxy \wedge Rzw \rightarrow (Wxz \leftrightarrow Wyw)]$,
(b) $\forall x[Fx \rightarrow \exists y(Gy \wedge Rxy)]$, and
(c) $\forall y[Gy \rightarrow \exists x(Fx \wedge Rxy)]$.

We can show that $R$ itself satisfies the needed condition to show that $F \cong^* G$. Consider arbitrary $x$, $y$, $z$, and $w$ such that $R(\text{type}(x), \text{type}(y)) \wedge R(\text{type}(z), \text{type}(w))$. There are then $a$, $b$, $c$, and $d$ such that $Wax$, $Wby$, $Rab$, $Wcz$, $Wdw$, and $Rcd$. By (a), it follows that (d) $Wac \leftrightarrow Wbd$. Suppose further that $W(\text{type}(x), \text{type}(z))$. There are then an $a'$ and $c'$ such that $Wa'x$, $Wc'z$, and $Wa'c'$. Because $W$ is symmetric and transitive, $Wac$, and thus, by (d), $Wbd$. Because $Wby$ and $Wdw$, it follows that $W(\text{type}(y), \text{type}(w))$. Discharging, $W(\text{type}(x), \text{type}(z)) \rightarrow W(\text{type}(y), \text{type}(w))$, and the converse holds by parallel reasoning. This establishes the first conjunct of what needs to be shown for $R$. Consider now some arbitrary $x$ such that $F(\text{type}(x))$. There is then some $a$ such that $Wax \wedge Fa$. By (b), above, there is a $b$ such that $Gb \wedge Rab$. Because $W$ is reflexive, $Wbb$ and thus $G(\text{type}(b))$ and $R(a, \text{type}(b))$, and so, because $Wax$, we have that $R(\text{type}(x), \text{type}(b))$ and so $\exists y(G(\text{type}(y)) \wedge R(\text{type}(x), \text{type}(y)))$. This establishes the second conjunct of what needs to be shown for $R$, and a very similar argument using (c) in place of (b) establishes the third. Hence, $F \cong^* G$.

## Proof of Peano Axioms

### *PP1*

PP1 follows directly by generalizing on the tautology

$$Q(0) \wedge \forall m \forall m'(Q(m) \wedge m\ P\ m' \rightarrow Q(m')) \rightarrow Q(0).$$

### *PP3*

Suppose for *reductio* that $\#(G)\ P\ 0$. By the definition of $P$, there is an $F$ such that $\#(F) = 0 \wedge \exists x(Fx \wedge \#(\lambda y.(Fy \wedge y \neq x)) = \#(G))$. By the second conjunct there is an $a$ such that $Fa$. But $\#(F) = \#(\lambda x.x \neq x)$, and so by HP′, $F \cong \lambda x.x \neq x$. The 1–1 correlation $R$ must map $a$ to some entity $b$ such that $b \neq b$, but this is absurd. Hence, $\neg\#(G)\ P\ 0$. Since $G$ was arbitrary, by generalizing, $\forall G\ \neg\#(G)\ P\ 0$, *i.e.*, $\forall n\ \neg n\ P\ 0$, or equivalently, $\neg\exists n\ n\ P\ 0$.

### *PP4*

For arbitrary $F$, $F'$, $G$ suppose that $\#(F)\ P\ \#(G)$ and $\#(F')\ P\ \#(G)$. By definition, there are then concepts $H$ and $H'$ and entities $a$ and $b$ such that $\#(H) = \#(G)$, $\#(H') = \#(G)$, $\#(\lambda x.(Hx \wedge x \neq a)) = \#(F)$ and $\#(\lambda x.(H'x \wedge x \neq b)) = \#(F')$. By HP′, and the symmetry and transitivity of $\cong$, $H \cong H'$. Let $C$ be the 1–1 correspondence between the $H$s and $H'$s. By HP′, $\lambda x.(H'x \wedge x \neq b) \cong F'$ and $\lambda x.(Hx \wedge x \neq a) \cong F$. Let $R$ be the 1–1 correlation between the $H$s other than $a$ and the $F$s, and let $R'$ be the 1–1 correlation between the $H'$s other than $b$ and the $F'$s. Either $Cab$ or not. If $Cab$, then let $C'$ be the relation that holds between arbitrary $x$ in $y$ just in case $Cxy$ but $x \neq a$. If not $Cab$, then let $C'$ by the relation that holds between $x$ and $y$ just in case $x \neq a$ and $y \neq b$ and either $Cxy$ or both $Cxb$ and $Cay$. In either case, clearly $C'$ is a 1–1 relation whose domain is the $H$s that are not $a$ and whose range is the $H'$s that are not $b$. Now let $S$ be the relation that holds between $x$ and $y$ just in case $Fx$ and $F'y$ and there are $z$, $z'$ such that $Hz$, $H'z'$, $Rzx$, $C'zz'$ and $R'z'y$ all hold. $S$ is clearly a 1–1 correlation between the $F$s and the $F'$s. Hence, $\#(F) = \#(F')$. Discharging our assumption $\#(F)\ P\ \#(G)$ and $\#(F')\ P\ \#(G) \rightarrow \#(F) = \#(F')$. Generalizing, and replacing variables with numeric ones, $\forall m \forall m' \forall n(m\ P\ n \wedge m'\ P\ n \rightarrow m = m')$.

### *PP2a*

Suppose for arbitrary $F$ and $G$ that $\mathrm{N}(\#(F))$ and $\#(F)\ P\ \#(G)$. Suppose for arbitrary $Q$ that $Q(0)$ and $\forall m \forall m'(Q(m) \wedge m\ P\ m' \rightarrow Q(m'))$. Instantiating $m$ and $m'$ to $\#(F)$ and $\#(G)$, respectively, we get $Q(\#(G))$. Discharging and generalizing, $\forall Q[Q(0) \wedge \forall m \forall m'(Q(m) \wedge m\ P\ m' \rightarrow Q(m')) \rightarrow Q(\#(G))]$, *i.e.*, $\mathrm{N}(\#(G))$. Again, discharging and generalizing, $\forall F \forall G(\mathrm{N}(\#(F)) \wedge \#(F)\ P\ \#(G) \rightarrow \mathrm{N}(\#(G)))$; rewriting, we get $\forall m \forall n(\mathrm{N}(m) \wedge m\ P\ n \rightarrow \mathrm{N}(n))$.

### *PP2b*

Suppose for arbitrary $F$, $G$, and $G'$ that $\#(F)\ P\ \#(G)$ and $\#(F)\ P\ \#(G')$. Then there are concepts $H$ and $H'$ such that $\#(H) = \#(G)$ and $\#(H') = \#(G')$, and there are entities $a$ and $b$ such that $Ha$ and $H'b$ and $\#(\lambda x.(Hx \wedge x \neq a)) = \#(F)$ and $\#(\lambda x.(H'x \wedge x \neq b)) = \#(F)$. By HP′ and the symmetry and transitivity of $\cong$, $\lambda x.(Hx \wedge x \neq a) \cong \lambda x.(H'x \wedge x \neq b)$. Let $R$ be the 1–1 correlation between these concepts. Let $R'$ be the relation holding between arbitrary $x$ and $y$ just in case $Rxy$ or $x = a$ and $y = b$. Clearly $C'$ is a 1–1 correspondence between $H$ and $H'$ and hence $H \cong H'$. By HP′, we also have that $H \cong G$ and $H' \cong G$. By symmetry and transitivity of $\cong$, $G \cong G'$, whence $\#(G) = \#(G')$. Discharging our supposition and generalizing, $\forall F \forall G \forall G'(\#(F)\ P\ \#(G) \wedge \#(F)\ P\ \#(G') \rightarrow \#(G) = \#(G'))$. Rewriting with number variables, $\forall m \forall n \forall n'(m\ P\ n \wedge m\ P\ n' \rightarrow n = n')$.

*PP5*

For arbitary $Q$, suppose both (a) $Q(0)$ and (b) $\forall m \forall m'(\mathrm{N}(m) \wedge Q(m) \wedge m\ P\ m' \rightarrow Q(m'))$. Next, for arbitrary $F$, suppose $\mathrm{N}(\#(F))$. That is: (c) $\forall Q[Q(0) \wedge \forall m \forall m'(Q(m) \wedge m\ P\ m' \rightarrow Q(m')) \rightarrow Q(\#(F))]$. More fully written out (c) is

$$\forall Q[Q(0) \wedge \forall G \forall G'(Q(\#(G)) \wedge \#(G)\ P\ \#(G') \rightarrow Q(\#(G'))) \rightarrow Q(\#(F))].$$

Let $Q'$ be $\lambda H.(\mathrm{N}(\#(H)) \wedge Q(\#(H)))$. By (c), we get (d):

$$Q'(0) \wedge \forall G \forall G'(Q'(\#(G)) \wedge \#(G)\ P\ \#(G') \rightarrow Q'(\#(G'))) \rightarrow Q'(\#(F)).$$

At (a) we have $Q(0)$, *viz.*, $\exists H(H \cong \lambda x.x \neq x \wedge Q(H))$. Let $H'$ be the null concept making this true. Hence, $\#(H') = 0$ by HP′, and by the reflexivity of $\cong$, $\exists H(H \equiv H' \wedge Q(H))$. Hence we have $\mathrm{N}(\#(H')) \wedge Q(\#(H'))$. Therefore $Q'(H')$, and since $H' \cong \lambda x.x \neq x$, $\exists H(H \cong \lambda x.x \neq x \wedge Q'(H))$, *i.e.*, $Q'(0)$. Next, for arbitrary $G$ and $G'$ suppose $Q'(\#(G))$ and $\#(G)\ P\ \#(G')$. $Q'(\#(G))$ means that there is a $G''$ such that $G'' \cong G$ and $Q'(G'')$. Thus, $\mathrm{N}(\#(G'')) \wedge Q(\#(G''))$. But $\#(G) = \#(G'')$ by HP′ and so $\mathrm{N}(\#(G)) \wedge Q(\#(G))$. By (b) earlier it follows that $Q(\#(G'))$. By PP2a, $\mathrm{N}(\#(G'))$, and so $Q'(G')$. By reflexivity of $\cong$, $\exists F(F \cong G' \wedge Q'(F))$, *i.e.*, $Q'(\#(G'))$. Discharging and generalizing, $\forall G \forall G'(Q'(\#(G)) \wedge \#(G)\ P\ \#(G') \rightarrow Q'(\#(G')))$, and so, by (d) $Q'(\#(F))$. Thus, there is an $F'$ such that $F' \cong F$ and $Q'(F')$. Therefore, $\mathrm{N}(\#(F')) \wedge Q(\#(F'))$. Hence, there is some $F''$ such that $F'' \cong F'$ and $Q(F'')$. By symmetry and transitivity of $\cong$, $F'' \cong F$, and so $\exists G(G \cong F \wedge Q(G))$, *i.e.*, $Q(\#(F))$. Discharging the assumption that $\mathrm{N}(\#(F))$ and generalizing, $\forall F(\mathrm{N}(\#(F)) \rightarrow Q(\#(F)))$, or more simply, $\forall n(\mathrm{N}(n) \rightarrow Q(n))$. Discharing the initial assumptions (a) and (b), and generalizing on $Q$, we get PP5.

*PP2c (Using (Inf))*

For arbitrary $F$, suppose that $\mathrm{N}(\#(F))$. By (Inf), $\exists x \neg Fx$. Call it $a$; hence $\neg Fa$. Let $F'$ be $\lambda x.(Fx \vee x = a)$. Clearly $\#(F') = \#(F')$. Also, $F'a$ and $\forall x(\lambda y.(F'y \wedge y \neq a)x \leftrightarrow Fx)$. Hence $\lambda y.(F'y \wedge y \neq a) \cong F$ and $\#(\lambda y.(F'y \wedge y \neq a)) = \#(F)$. Thus $\exists x(Fx \wedge \#(\lambda y.(F'y \wedge y \neq x)) = \#(F))$. Therefore $\#(F)\ P\ \#(F')$. Hence, $\exists n\ \#(F)\ P\ n$. Discharging and generalizing, $\forall F(\mathrm{N}(\#(F)) \rightarrow \exists n\ \#(F)\ P\ n)$, *i.e.*, $\forall m(\mathrm{N}(m) \rightarrow \exists n\ m\ P\ n)$.

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