Reals by Abstraction†

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1. General Aim and Basic Ideas

1.1. Abstraction

A Fregean *abstraction principle* is now usually taken to be a principle of the general form:

$$\forall\alpha\forall\beta\ (\S\alpha = \S\beta \leftrightarrow \alpha \approx \beta),$$

where $\approx$ is an equivalence relation on entities denoted by expressions of the type of α and β and § is an operator which forms singular terms when applied to constant expressions of the same type. The most prominent examples in Frege's own writings are the *Direction equivalence*:

the direction of line a = the direction of line b
iff lines a and b are parallel,

together with what is now often called *Hume's principle*:

the number of Fs = the number of Gs
iff the Fs and the Gs are 1-1 correlated,

and his ill-fated *Basic Law V*:

the extension of F = the extension of G
iff F and G are co-extensive.

In general, an abstraction principle seeks to give necessary and sufficient conditions for the identity of objects mentioned on its left-hand side in terms of the holding of a suitable equivalence relation between entities of some

† I am indebted to an anonymous referee for this journal, and to Roy Cook, Jim Edwards, Gary Kemp, Pierluigi Miraglia, Philip Percival, Stewart Shapiro, Neil Tennant and Crispin Wright for helpful discussion of earlier versions of this material, as well as to my audiences at presentations of parts of it in Cambridge, Columbus Ohio, Glasgow, *L'Institut d'Histoire et Philosophie des Sciences et des Techniques* in Paris and St. Andrews. Very special thanks are due to my colleague Adam Rieger. Work on this paper was carried out during my tenure of a British Academy Research Readership—I am most grateful to the Academy for its generous support.

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other sort. The Direction equivalence is a *first-order* abstraction, because its equivalence relation is a first-level relation on objects, whereas Hume's principle and Basic Law V are *second-order*, their equivalence relations being second-level relations on concepts.

1.2. Frege's logicism

Frege discusses at *Grundlagen* §§60–67 the suggestion that number might be contextually defined by means of Hume's principle, but rejects it because he can see no way to solve what is now called the *Caesar problem*. The problem is that while Hume's principle provides the means to settle, at least in principle, the truth-values of identity-statements linking terms for numbers when those terms are of the form 'the number of Fs' (or definitional abbreviations of such terms), it appears not to enable us to answer questions of numerical identity, when one of the terms is not of that form, such as whether the number of Jupiter's moons = Julius Caesar. Frege then immediately switches to his well known explicit definition of number in terms of extensions (or classes): the number of Fs = the class of concepts 1-1 correlated with F. This requires him to provide a theory of extensions or classes, which he does by means of Basic Law V. As is well known, Basic Law V is inconsistent. Frege's own attempt to arrive at a restricted axiom on classes which is both consistent and able to serve in its place as the basis for his hoped-for derivation of arithmetic from logic was unsuccessful and he eventually abandoned his belief that arithmetic could be provided with a purely logical foundation. Further, whilst we now know—or at least think we know—how to formulate a consistent theory of sets, this affords no comfort to anyone in sympathy with Frege's logicist project, for two reasons. One is that this theory—Zermelo-Fraenkel set theory, say—is not plausibly viewed as a *purely logical* theory, owing to the very substantial existence assumptions it involves. The other is that Frege's definition of number cannot be consistently embedded in the theory, because the objects with which it identifies cardinal numbers are too big to be treated as sets.

1.3. Neo-Fregean logicism

As far as elementary arithmetic goes, Frege's *only* indispensable appeal, in *Grundlagen* and in *Grundgesetze*[1] to his explicit definition of number (and thence to Basic Law V) is in proving Hume's principle from it. That is, once Hume's principle has been established as a theorem, no further appeal need be made, either to the explicit definition or to Basic Law V, in deriving as theorems what are, near enough, the Dedekind-Peano axioms for arithmetic. These include, crucially, the axiom asserting that every natural

[1] As far as *Grundlagen* goes, this is quite clear from a reading of §§68–83 and is emphasised by Crispin Wright [1983]. That the same is true of *Grundgesetze* is shown in Heck [1993].

number has another natural number as its successor, which amounts (in the presence of the others) to the assertion that there are infinitely many natural numbers. This fact is now, following a suggestion of the late George Boolos (see Boolos [1990]), referred to as *Frege's Theorem*. What Frege's Theorem asserts, in effect, is that if Hume's principle is added to a standard formulation of second-order logic as a further axiom, the resulting system suffices for the derivation of elementary arithmetic. It is known that this system is consistent—or at least, that it is so, if second-order arithmetic is.

Whether this fact supports any kind of logicism about arithmetic depends, of course, on the status of Hume's principle. Boolos, along with many others, denies—plausibly, in my view—that it can be regarded as a *truth of logic*. Further, Hume's principle cannot be taken as a *definition*, in any strict sense, because it does not permit the elimination of numerical terms in all contexts. This does not settle the issue, however, since it may be claimed that the principle is *analytic*, or a conceptual truth, in some sense broader than: either a truth of logic or reducible to one by means of definitions. That it can be so regarded is the view—now often called neo-Fregean logicism—of Crispin Wright and myself.[2]

I do not intend, here, to defend this view of arithmetic against the many objections to our claim that Hume's principle is a conceptual truth about numbers. Nor shall I offer a solution to the Julius Caesar problem[3]—though this must be (and we believe can be) done, if our view is to be viable. Nor, finally, shall I offer a general philosophical defence of the idea—which is again central to our view—that abstraction principles (provided they are consistent and perhaps meet certain other constraints) provide a legitimate means of introducing concepts of various kinds of abstract object in such a way that the existence of those objects depends only upon there being true instances of their right-hand sides.[4]

Instead, what I want to do is explain *one* way in which I think it may be possible to extend our view beyond elementary arithmetic, to encompass the theory of real numbers. I say 'one way' because there are, on the face of it, several different ways in which one might try to do this.

1.4. Reals *via* Fregean set theory

In some ways, the most obvious approach—the one which has probably received most attention in recent work[5]—is a set-theoretic one. This would involve formulating a consistent Fregean axiom for sets to replace Basic

[2] Cf. Wright [1983], [1997] and [forthcoming], Hale [1987], [1994] and [1997]. For Boolos's opposed view see his [1997].

[3] *Qv.* works cited in fn. 2.

[4] *Qv.* works cited in fn. 2.

[5] Cf. Boolos [1989] and also [1987] and [1993]; Wright [1997]; and Shapiro and Weir [1999].

Law V—an axiom which could form the basis of a theory of sets powerful enough to support one or other of the usual set-theoretic constructions (Dedekind's or Cantor's) of the reals. The most obvious way to do this is by means of a suitably restricted version of Basic Law V, and a good deal of work has been done on one particular axiom of this sort, which builds in a restriction on the 'size' of concepts which are permitted to have sets corresponding to them which obey the principle of extensionality.[6] I shall not discuss this work here, save to remark that some of it seems to me to show that the prospects for obtaining a satisfactory treatment of the reals along this line are uncertain at best. In particular, as Boolos [1989] observed, a theory based on second-order logic plus this axiom alone, without further comprehension or existence assumptions, will not enable us to prove either an axiom of infinity or a power-set axiom. So it will not yield sets large enough for the construction of the reals.

This is not conclusive evidence against a broadly set-theoretic approach, of course, since it may be possible to formulate some other more powerful but still consistent Fregean axiom for sets which will give us large enough sets. Or again, it may be possible to justify supplementing this particular restricted version of Basic Law V with other principles to obtain a strong enough theory. I take no stand on that question here.[7] Instead, I want to pursue a quite different approach, which is in some respects much more like that taken by Frege in his incomplete treatment of the reals in *Grundgesetze*, although it differs from Frege's in at least one quite fundamental way. This approach can roughly be described by saying that it tries (i) to minimise reliance on set theory and (ii) to obtain the reals very directly by means of abstraction principles, without any form of set-abstraction. In these respects, I think my approach may be seen as the most direct and natural way of extending the neo-Fregean position to the reals. Just as basing elementary arithmetic on Hume's principle minimises (and, indeed, eliminates) reliance on set theory by avoiding a definition of cardinal numbers as certain equivalence classes, introducing them instead *via* a specifically *numerical* abstraction—so my approach to the arithmetic of real numbers will minimise (and indeed eliminate) reliance on set theory by avoiding a definition of reals as sets of one kind or another, introducing them instead via abstraction principles which—even if not happily described as purely numerical—are not distinctively set-theoretical.

[6] The axiom (New V) is: $\forall F \forall G\, [{}^*F = {}^*G \leftrightarrow ((\text{Small}\,(F) \vee \text{Small}\,(G)) \rightarrow \forall x\, (Fx \leftrightarrow Gx))]$, where a concept is Small if fewer objects fall under it than fall under the universal concept $\xi = \xi$, and ${}^*F$ is what Boolos calls the 'subtension' of F (the subtensions of Small concepts being sets)—see Boolos [1987], and also below p. 116ff.

[7] For a brief discussion of this possibility, see Crispin Wright [1997], section XI.

1.5. Reals as ratios of quantities

Frege's actual (incomplete) treatment of the reals in *Grundgesetze* Pt III[8] is, of course, unsatisfactory—if only because it relies, as does his theory of cardinal numbers, on an inconsistent theory of extensions, and cannot be simply relocated within any standard (and plausibly consistent) theory of sets such as ZF or NBG because the objects with which he proposes to identify the reals are too big to be treated as sets. In any case, such a relocation would obviously betray Frege's philosophical aims, since it would leave our entitlement to the substantial existential commitments of the theory quite unaccounted for. From a philosophical standpoint, the most striking and most important features of Frege's treatment of the reals are two: (i) the real numbers are to be defined as *ratios of quantities* [§§73, 157] and (ii) in regard to the analysis of the notion of quantity, the fundamental question requiring to be answered is not: *What properties must an object have, if it is to be a quantity?* but: *What properties must a concept have, if the objects falling under it are to constitute quantities of a single kind?* [§§160–1].

Briefly and roughly, his insistence that reals be defined as ratios of quantities derives from his belief that the *application* of reals as measures of quantities is *essential* to their very nature, and so should be built into an adequate definition of them. It is this, more perhaps than any other single consideration, which underlies his dissatisfaction with the theories of Cantor and Dedekind, on which the applicability of the reals appears, in Frege's view, merely as an incidental extra.

As regards the second point, it is obvious to anyone that there are many different *kinds* of quantity (lengths, masses, volumes, angles, *etc.*) and that addition and comparison (as greater or less) make sense only as applied to quantities of the *same* kind. Since we may not simply take the notion of a *kind* of quantity for granted, as already understood and itself in no need of analysis, we cannot *explain* what a quantity is by saying that it is something which can be added to, or be greater or less than, (other) quantities of the same kind. If an explanation of quantity is not to be vitiated by circularity in this way, Frege thinks, it must take as its target the notion of a kind of quantity, and say what characteristics a collection of entities must, as a whole, possess if it is to form what he calls a quantitative domain [*ein Grössengebiet*]. When that has been done, what it is to be a quantity can be easily stated—an object is a quantity if it belongs, together with other objects, to a quantitative domain.

I believe Frege was substantially right on both points. Here I shall simply assume as much, without argument. Where I disagree with him is over the analysis of quantitative domains. For reasons which I shall not go into,

[8] For expositions see Michael Dummett [1991], Ch. 22 and Peter Simons [1987].

Frege decides that the elements of a quantitative domain should themselves be relations and—heavily influenced by a passage from Gauss [quoted in *Grundgesetze* §162]—analyses such a domain as an ordered group of permutations on an underlying set, with composition as its additive operation. Since quantities themselves are, on his approach, relations of a certain sort, real numbers, when defined as ratios of quantities, turn out to be relations of relations. One advantage of Frege's approach is that it provides very easily for negative as well as positive real numbers. I do not have space to discuss Frege's view properly here. Whilst there is justice in his criticism of earlier writers who simply help themselves to the notion of quantities being of the same kind, I think that the notions of addition and quantitative comparability are central and fundamental to the general notion of quantity in a way Frege fails to acknowledge. Accordingly, I shall propose a different account of quantitative domains—one which gives a central role to the idea that the elements of such a domain may always be added to yield further elements.

2. Quantities and Reals

2.1. Types of Quantitative Domain

I distinguish between the entities (usually concrete objects) which may stand in various quantitative relations to one another—such as being longer than, or being as long as—and quantities themselves, which I take to be abstract objects introduced by abstraction on quantitative equivalence relations—for example:

$$\text{the length of } a = \text{the length of } b \leftrightarrow a \text{ is as long as } b.$$

This way of introducing (terms for) quantities makes no *explicit* mention of addition. However, a full analysis of the notion of a quantitative relation would, I claim, show that the notion of addition is nevertheless central to that of quantity. I do not have space to go into details here, but the essential idea is this. Among quantitative relations, we may distinguish—as conceptually basic—what may be called relations of simple quantitative comparison (*e.g.*, longer than/as long as, heavier than/as heavy as, *etc.*) from relations of numerically definite or determinate comparison (*e.g.*, twice as long as, 2.4 kg heavier than, *etc.*). A *necessary* condition for ϕ to denote a kind of quantity is that it be associated with a pair of relations of simple quantitative comparison: *more* ϕ *than* and *as* ϕ *as*. In virtue of this, things which are ϕ may be partially ordered with respect to ϕ-ness. However, the existence of an associated pair of such relations—a strict partial ordering relation and a cognate equivalence relation—is insufficient for ϕ-ness to be a kind of quantity. There are enormously many adjectives in ordinary use which may be substituted without violence to sense or syntax in the schemas: *more* ϕ *than* and *as* ϕ *as*—'sweet', 'elegant', 'graceful', 'pretty', 'clumsy', 'ambitious', 'impatient', 'irrascible', 'probable', ... is clearly no more than the start of a potentially very long list. But in the case of only relatively few of them is it remotely plausible that they denote something properly describable as a quantity. It is therefore necessary to enquire what further condition needs to be satisfied, if such a pair of relations are properly to be viewed as quantitative. I contend that what makes the difference between quantitative ordering relations and others is that in the case of a quantitative ordering relation, but not otherwise, the entities which can significantly be asserted to stand in the relation can (at least in principle) be combined in such a way that compounds must come later in the relevant ordering than their components. In other words, for *more ϕ than* to be a quantitative ordering relation, there must be an operation of combination © on items lying in the field of *more ϕ than*, analogous to addition, such that for any a, b in *more ϕ than*'s field, a©b is *more ϕ than* a and a©b is *more ϕ than* b.[9]

Quantitative domains are composed of (abstract) quantities. My aim in this section is to provide an informal axiomatic characterisation of such domains, on the basis of which it will be possible to introduce real numbers by means of an appropriate abstraction principle. Instead of simply laying down a *single* set of axioms for something to be a quantitative domain, I shall distinguish several—successively richer— types of quantitative domain. This will be helpful later, when I come to consider questions about the *existence* of quantitative domains.

(1) A *minimal q-domain* is a non-empty collection Q of entities closed under an additive operation $\oplus$, which commutes, associates and satisfies the strong trichotomy law that for any a, b $\in$ Q we have exactly one of: $\exists$c (a = b $\oplus$ c), $\exists$c (b = a $\oplus$ c) or a = b. Any minimal q-domain is strictly totally ordered by $<$, defined by: a $<$ b $\leftrightarrow$ $\exists$c (a $\oplus$ c = b). Multiplication of elements of Q by positive integers is easily defined—inductively— in terms of $\oplus$.

(2) A *normal q-domain* is any minimal q-domain meeting the [Archimedean] comparability condition: $\forall$ a, b $\in$ Q $\exists m$ (ma $>$ b). Here and sub-

[9] The basic idea is of course not new. It is, in particular, central to the theory of measurement advanced by N. R. Campbell in a number of works first published in the 1920s, the most important of them being Campbell [1919]—see Part II—and Campbell [1928]. A briefer popular statement of his theory is given in Campbell [1921], ch. VI. Whilst there is much in Campbell's overall theory which I think we neither can nor need accept, I believe that Campbell was right, *pace* critics such as Brian Ellis (see Ellis [1966], ch. IV), to insist upon the importance of a physical analogue of addition, and right too (at least in essentials) in taking there to be an important distinction between fundamental and derived measurement. More recent treatments of measurement—see, for example, the comprehensive text of Krantz *et al.* [1971], [1989]—have not looked kindly on these distinctive features of Campbell's approach. I need hardly emphasise that the very rough and dogmatic statement of my view, both here and in the text, requires both considerable qualification and further explanation, as well as defence.

sequently (unless explicitly indicated), m (and later n as well) ranges over positive integers. This requires quantities to be *finite*, in the sense that no quantity is infinitely greater (or smaller) than any other—it rules out *infinitesimal* quantities. With his eye on Euclid's Def. 4 of *Elements* Bk. V, Howard Stein[10] describes it as the condition necessary and sufficient for a and b to have a ratio. It might be compared, in status, to the requirement on concepts presupposed by Hume's principle, that the concepts through which it quantifies be *sortal*—which might be described as the condition for a concept to have a (cardinal) number.

Where Q, Q* are any normal q-domains, not necessarily distinct, we introduce ratios of quantities by the abstraction principle:

$$\text{(EM)}\quad \forall \mathrm{a}, \mathrm{b} \in Q\ \forall \mathrm{c}, \mathrm{d} \in Q^*[\mathrm{a} : \mathrm{b} = \mathrm{c} : \mathrm{d} \leftrightarrow \forall m, n(m\mathrm{a} \ {}^{<}{=}_{>}\ n\mathrm{b} \leftrightarrow m\mathrm{c}\ {}^{<}{=}_{>}\ n\mathrm{d})].$$

That is, ratios a : b and c : d are the same just if equimultiples of their numerators stand in the same order relations to equimultiples of their denominators.[11] The condition for identity of ratios is framed so as to allow that one and the same ratio may be at the same time a ratio of pairs of quantities of different kinds—belonging to different domains—such as masses and lengths. The operation in terms of which comparability is ultimately defined (*i.e.*, addition of quantities) is, of course, domain specific—no sense is given to adding a length and a mass, for instance. But this does not preclude the introduction of ratios so that the same ratio may be found among, say, both masses and lengths.

(3) A normal q-domain Q is *full* if $\forall \mathrm{a}, \mathrm{b}, \mathrm{c} \in Q\ \exists \mathrm{q} \in Q(\mathrm{a} : \mathrm{b} = \mathrm{q} : \mathrm{c})$. This condition, which is a restricted form of the ancient postulate of 'fourth proportionals', ensures that, given a pair of ratios a : b and

[10] See Stein [1990]. Whilst the approach I pursue here differs quite radically from anything suggested by Stein, I have derived much benefit from this excellent paper.

[11] This is, of course, the central principle in the ancient theory of proportion presented in Euclid's *Elements* Book V (cf. Def. 5) and standardly attributed to Eudoxos.

I should perhaps emphasise that EM is not an abstraction principle of the form characterised at the outset. On the other hand, it should be clear that it is intended to work in essentially the same way as paradigm abstractions like the Direction equivalence and Hume's principle and that it is reasonable to regard it as one. We might bring EM into line with the characterisation of abstraction principles with which I began by first defining an equivalence relation on ordered pairs of quantities: $E[(\mathrm{a}, \mathrm{b}),(\mathrm{c}, \mathrm{d})] \leftrightarrow \forall m, n(m\mathrm{a}\ {}^{<}{=}_{>}\ n\mathrm{b} \leftrightarrow m\mathrm{c}\ {}^{<}{=}_{>}\ n\mathrm{d})$, and then setting: $\mathrm{Ratio}(\mathrm{a}, \mathrm{b}) = \mathrm{Ratio}(\mathrm{c}, \mathrm{d}) \leftrightarrow E[(\mathrm{a}, \mathrm{b}), (\mathrm{c}, \mathrm{d})]$. Alternatively, if it were felt desirable to avoid reliance on the notion of an ordered pair, we could introduce an extension of the notion of an equivalence relation so as to allow relations of arity greater than 2 to qualify as equivalence relations. Later we shall meet another abstraction principle which does not, as it stands, conform to the usual characterisation, but which may readily be brought into line in one or another of these ways.

c : d, there is a quantity c′ such that c′ : b = c : d, so that we may always, without loss of generality, restrict attention to ratios with common denominators. I shall refer to it as CD. It is easy to see that CD ensures that there is no smallest quantity.[12]

(4) A full q-domain may be *incomplete*, in the sense that it may include *only* quantities which are *rationally* measurable; in consequence, the set of all ratios on a full domain is not guaranteed to include ratios corresponding to *any*, much less *all, (positive) irrational* numbers.[13] If ratio-abstraction is to yield all the positive reals, we require a *complete* domain. Indulging—for convenience, but avoidably—in set-theoretic language, we say that a subset S of quantities belonging to a q-domain Q is *bounded above* by b iff for every quantity a in S, $a \leq b$. A quantity $b \in Q$ is a *least upper bound* of $S \subseteq Q$ iff b bounds S above and $\forall c(c$ bounds S above $\rightarrow b \leq c)$, and finally that a q-domain Q is *complete* iff Q is full and every bounded-above non-empty $S \subseteq Q$ has a least upper bound.

2.2. Real Numbers

We may straightforwardly define 'bounded above', 'lub', and 'order-complete' *for ratios* in a way that parallels our definitions of these notions for quantities and then prove, as an easy consequence of the completeness of the underlying domain, that where Q is any complete q-domain, the set R^Q of ratios on Q is order-complete.[14] It can be shown that if Q and Q* are any complete q-domains, they are isomorphic, so that $R^Q = R^{Q^*}$, *i.e.*, the set of ratios on Q is *identical* with the set of ratios on Q*. Thus *provided there exists at least one complete q-domain*, we can introduce the *positive real numbers*, by abstraction, as the ratios on that domain.

In standard constructions of the various number systems, *negative* numbers make their entry at an early stage. The method by which this is accomplished—introducing a new, enlarged domain including negative

[12] Although I am not identifying *quantities*, as such, with *numbers* of any kind, it should be fairly clear that a full domain, and likewise the domain of ratios on it, is dense, and that we can develop an 'arithmetic' of ratios structurally analogous to that of the positive rationals.

[13] Of course, since quantitative domains, as I have characterised them, do not include either a *zero* quantity or *negative* quantities, the ratios on such domains will not, in any case, have elements corresponding to *all* the reals.

[14] *Proof*: Let S be any bounded-above subset of R^Q. By CD, each ratio in S can be expressed with a single common denominator, so that the members of S are: $a_1 : b$, $a_2 : b, \cdots, a_i : b, \cdots$. The set of numerators of these ratios is a non-empty subset of Q, and so—by the completeness of Q—have a least upper bound a°. Since every $a_i \leq a^\circ$, $a_i : b \leq a^\circ : b$ for every ratio $a_i : b$ in S. And if some ratio $p : q$ is less than $a^\circ : b$, it follows [by CD] that $p : q = p' : b$ for some p′, with $p' < a^\circ$. But then by the completeness of Q, there is some a_k among the numerators of the ratios $a_i : b$ so that $p' < a_k$, and hence a ratio $a_k : b$ in S such that $p' : b < a_k : b$. So $a^\circ : b$ is a least upper bound of S.

numbers as certain ordered pairs (difference pairs) of numbers belonging to an underlying domain—is, however, perfectly general, in the sense that it is quite inessential to it that the numbers in the underlying domain should be *natural* numbers. Of course, we must start with the natural numbers if we want to get just the integers—but in general, all that is required for the application of the method itself is that the objects belonging to the underlying domain have the requisite arithmetic properties. There is, so far as I can see, no reason, either technical or philosophical, why this step may not just as well be taken at a (much) later stage. In particular, essentially the same construction can be used to get negative reals, starting from positive ones, as difference pairs of positive reals. Letting x, y, z,... range over, and $\oplus$ stand for addition of, positive reals, we obtain *difference pairs* of positive reals by the abstraction:

(D) $$(x,\, y) = (z,\, w) \leftrightarrow x \oplus w = y \oplus z.$$

Defining $<\ ,\ >$, addition, subtraction and multiplication and zero for d-pairs in the obvious way, it can be shown that the collection R of d-pairs forms a *field* with the operations $+$ and $\times$. Further, there is a subset P of R, namely the set of all pairs (x,y) such that $(z,z) < (x,y)$, meeting the conditions:

(i) if $(x,y),(z,w) \in P$ then $(x,y)+(z,w) \in P \wedge (x,y)\times(z,w) \in P$

and

(ii) if $(x,y) \in R$, then exactly one of $(x,y) \in P$, $(y,x) \in P$ or $(x,y) = (z,z)$ holds.

Thus R is an *ordered field*. There is an obvious isomorphism between the strictly positive subset P of R and the positive reals as previously defined. Using this, it can be shown without too much difficulty that R is complete.

3. The Existence of Quantitative Domains

Our result thus far is conditional: real numbers may be obtained by abstraction on quantities, *if there exists at least one complete q-domain*. Even if this were the best result that could be obtained, it is not completely obvious that this would signal the collapse of the neo-Fregean abstractionist approach to foundations. It might be possible to provide principled reasons for adopting different attitudes towards the question of the existence of reals and that of the natural numbers, holding that while the latter admits of resolution, *a priori*, in the affirmative, the existence of the reals is a matter on which no similar *a priori* assurance is to be expected. According to such a view, the existence of (at least) finite cardinal numbers would be a matter of *necessity*—whatever the universe might be like, its ingredient objects would be assignable to distinguishable sorts or kinds; there would be some sortal concepts or other, under which the objects fell, so that for various concepts F and cardinal numbers n, there would be facts of the form: the number of Fs$= n$. More importantly, for any such sortal concept F, there will be a sortal—F-and-not-F—logically guaranteed to have no objects falling under it, in terms of which zero may be defined, thus giving the necessary toe-hold for a Fregean proof of the existence of an infinite collection of finite cardinals. But there can be no similar *a priori* guarantee that the physical universe comprises quantities which are real-valued—it is perfectly conceivable, even if in fact false, that the physical world should be discontinuous. So a result which says, in effect, that if it *does* exhibit continuity, the real numbers are available to measure it, might not appear utterly outrageous. Defending this position would, naturally, require speaking to the contrary intuition, that while it may be in some way an empirical question whether the physical universe is continuous, and so an empirical question whether the reals have 'objective' application [in the sense that there actually are real-valued quantities—contrast the idea that using the reals simply affords a useful simplification of applied mathematics], the existence of the reals should not itself be an empirical, *a posteriori* matter.

Clearly, however, it is important to enquire whether a neo-Fregean can secure a stronger result. Evidently, the question of greatest interest is whether there can be proved to exist a *complete* q-domain. But it is worth emphasising that the question arises, not only for the case of complete q-domains, but equally for q-domains of the more modest kinds described—thus far, nothing has been done to establish the existence of a full q-domain, or even that of a normal, or even minimal, one. Even the question of the existence of a *minimal* domain is anything but trivial. A minimal domain is, by definition, non-empty. Since such a domain is closed under its addition operation and satisfies the additive trichotomy condition, it must comprise *arbitrarily large* quantities, and thus be at least countably infinite. To anyone who thinks of quantities as physical entities of some sort, the existence of such a domain must, for this reason, appear open to serious question. On my own view, quantities such as lengths, masses, angles, *etc.*, should not be thought of as physical entities; they are, rather, *abstract* objects, 'introduced' *via* abstraction principles employing appropriate equivalence relations on the concrete objects whose lengths, masses, *etc.*, they are. But this makes no essential difference, so far as the present question is concerned. At least, it will make no difference if the existence of a given length, say, is taken to be contingent upon the existence of a suitable concrete entity of which it is the length; for in that case, the ground for doubt about the existence of arbitrarily large quantities of any given kind remains. Clearly there must be an analogous doubt about the existence of arbitrarily small quantities, and hence about the existence of a *full* q-domain. However, it seems to me that these doubts may be assuaged and that we can actually prove the existence of at least one domain of each of the kinds I have distinguished, including complete domains.

The crucial point here is to notice that whilst *quantities* as such are *not* identified, in my approach, with *numbers*, nothing in the characterisation of q-domains precludes such domains being *composed of* numbers. As previously remarked, Hume's principle suffices for a derivation of the Dedekind-Peano axioms for elementary arithmetic, and hence for a proof of the existence of an infinite sequence of *natural numbers*—0,1,2,.... Omitting 0 to obtain the strictly positive naturals, N^+, and adjusting the usual recursive definitions of + and × to suit, we can easily show that N^+ constitutes a *minimal*—and indeed a *normal*—q-domain.

It is clear that N^+ is not itself a full domain, *i.e.*, it does not satisfy CD. However, the collection R^{N^+} of *ratios* on N^+ *does* constitute a full domain. To see this, note first that since N^+ is normal, there exists a ratio $a : b$ for every a and b in N^+. Let a, b, c, d, e, f be any elements of N^+. Then what we must show is that there is a ratio $g : h$ such that $[a : b] : [c : d] = [g : h] : [e : f]$. It is quite straightforward to verify that

$$[a : b] : [c : d] = ad : bc = ade : bce$$
$$= [ade : bcf] : [bce : bcf] = [ade : bcf] : [e : f]$$

so that $[ade : bcf]$ is our required ratio.[15] In the presence of CD, satisfaction by R^{N^+} of the minimality and normality conditions follows easily from their satisfaction by the underlying domain N^+. Thus R^{N^+} is a full domain. What we have, in effect, is a quite natural way of obtaining the positive rationals by abstraction on the positive natural numbers—each and every positive rational is simply a ratio of positive natural numbers. Thus 3/4 just is the ratio 3:4. Of course, it is also the ratio 6:8 and the ratio 9:12, *etc.*, but that is no problem, since these are all simply one and the same ratio in our sense (i.e., by the lights of EM).

It is clear that iteration of the abstractive procedure which yields R^{N^+} from N^+ will not yield any new kind of q-domain. The crucial point emerges above, in the observation that $[a : b] : [c : d] = ad : bc$. This holds quite generally—any ratio of ratios of positive natural numbers are simply ratios of positive natural numbers. In the same way, ratios of ratios of ratios of positive natural numbers collapse to ratios of positive natural numbers. Iteration of the abstraction to ratios of higher order thus merely gives us the positive rationals all over again. Thus the operation by which

[15] Recall that a, b, c, d, e, f are all positive integers. A ratio is unchanged by multiplying its numerator and denominator by the same positive integer. Hence $a : b = ad : bd$. Similarly, $c : d = bc : bd$. But the ratio to one another of ratios with a common denominator is simply the ratio of their numerators, so $[ad : bd] : [bc : bd] = ad : bc$, whence $[a : b] : [c : d] = ad : bc$. Further $e : f = bce : bcf$ and $ad : bc = ade : bce$. Hence, since $[ade : bcf] : [bce : bcf] = ade : bce$, we have: $[a : b] : [c : d] = ad : bc = ade : bce = [ade : bcf] : [bce : bcf] = [ade : bcf] : [e : f]$.

we obtained a full domain from an underlying normal one cannot, when re-applied to a full domain, yield a complete one. This is a special case of a quite general fact about first-order abstraction: no first-order abstraction on an infinite domain can generate a 'new' domain of greater cardinal size than that abstracted on. It follows that if a complete domain is to be obtained by abstraction, we must invoke a *second*-order abstraction. In this way—and only in this way—we may advance from a domain of objects of given cardinality to a strictly larger domain of abstracts. Given an initial domain comprising κ objects, there will be 2^κ properties of those objects. By taking these properties, rather than the objects which have them, as our underlying domain for an abstraction, we may obtain a strictly larger collection of abstracts—up to (but not more than) 2^κ of them.[16]

We take as our initial domain the (at least countably infinite) full domain R^{N^+} of ratios on N^+. Our goal is to obtain a complete domain $Q\#$ by *cut abstraction*, so-called because of its obvious correspondence to Dedekind's construction.[17] As anticipated, cut abstraction operates, not directly upon R^{N^+} itself, but upon properties of a certain kind defined over its elements, which I shall call *cut-properties*. These are defined by reference to the ordering on R^{N^+}. Informally, a cut-property is a *non-empty* property whose extension is a *proper* subset of R^{N^+} and which is *downwards closed* [*i.e.*, $\forall a \forall b\, (Fa \rightarrow (b < a \rightarrow Fb))$[18]] and has *no greatest instance* [*i.e.*, $\forall a\, (Fa \rightarrow \exists b\, (b > a \wedge Fb))$]. We now introduce objects—*cuts*—corresponding to cut-properties by the abstraction principle:

(Cut) $\#F = \#G \leftrightarrow \forall a\, (Fa \leftrightarrow Ga)$ where F, G are any cut properties on R^{N^+} and a ranges over R^{N^+}.

$Q\#$ is the collection of all cuts, $\#F$, for cut-properties F on R^{N^+}. It may be shown that $Q\#$ constitutes a *complete* domain, in the sense previously explained. Obviously the main thing here is to verify that $Q\#$ has the *least upper bound* property, i.e., where ϕ varies over properties of cuts on R^{N^+}, and *bounds above* and *lub* are defined in an obvious way, that if $\exists F\, \phi(\#F)$ and ϕ is bounded above then ϕ has a least upper bound. This can be done, mimicking the usual proof, by defining the property H by: $Ha \leftrightarrow \exists F\, (\phi(\#F) \wedge Fa)$—we can then show that H is a cut-property and that $\#H$ is a lub of ϕ. We may define $\#F + \#G$ to be $\#H$, where $Ha \leftrightarrow \exists b \exists c\, (Fb \wedge Gc \wedge a = b \oplus c)$, and $\#F \times \#G$ to be $\#P$, where $Pa \leftrightarrow \exists b \exists c\, (Fb \wedge Gc \wedge a = b \otimes c)$. With the aid of these and some

[16] If κ is infinite and CH holds, then we shall, of course, get more than κ abstracts only if we get exactly 2^κ of them; but I am not assuming CH, much less GCH.

[17] Cf. Richard Dedekind, *Stetigkeit und Irrationale Zahlen* (1872), translated by Wooster Woodruff Beman as 'Continuity and irrational numbers', in Richard Dedekind, *Essays on the Theory of Numbers*. Reprinted New York: Dover Publications (1963), pp. 1–27.

[18] Here and subsequently $a, b, \ldots$ range over elements of R^{N^+}.

supplementary definitions, it can then be proved that $Q\#$ is *full*, *i.e.*, that it is a *minimal* q-domain which also meets the *normality* and *common denominator* conditions.

4. Safe Abstractions and Safe Sets

Are the abstraction principles which I have employed all in good standing? The question is urgent, since we know that not all abstraction principles are acceptable, if only because some—Basic Law V being the obvious example—are inconsistent. And there may be other constraints, besides consistency, with which good abstractions must comply. A thorough examination of the question lies well beyond the scope of this paper, but I should like to conclude by saying a little about it. Of the abstraction principles I have used, two—ratio abstraction EM and difference abstraction—are *first-order*, while the other two—Hume's principle and Cut—are *second*-order. In the case of first-order abstraction, we abstract upon a domain of *objects* of some kind, and thereby come to recognise objects of another kind; with a second-order abstraction, by contrast, we abstract upon a domain of *concepts*, themselves defined on some underlying domain of objects, and come to recognise 'new' objects, *i.e.*, objects of a kind other than those belonging to this underlying domain. I shall call the field of an abstraction's equivalence relation the *domain for the abstraction*, and in the case where this is a domain of (first-level) concepts, I shall call the domain of objects on which these concepts are defined the *underlying domain*.

In the case of second-order abstractions, the underlying domain—if it has a determinate size at all—is much smaller than the domain for the abstraction; if the underlying domain has cardinality κ, then the domain for the abstraction (assuming it to comprise all the concepts defined on the underlying domain, and assuming concepts to be individuated extensionally) has cardinality 2^{κ}. In consequence, the abstraction may 'generate' up to 2^{κ} abstracts—and so many more abstracts than there are objects in the underlying domain. It is this feature of second-order abstractions which has led some writers to think that it is these abstractions—in contrast with first-order abstractions—which pose the greatest worry, as far as the risk of inconsistency is concerned. I think that is correct, and I shall therefore focus on the second-order abstractions. In fact, since Hume's principle is known to be consistent, I shall concentrate upon the other second-order abstraction I have used—cut abstraction.

Cut—in contrast with Hume's principle and Basic Law V—is a *restricted* abstraction principle, in the sense that the domain for the abstraction comprises only cut-properties on a certain specified underlying domain of objects. It is obvious that if the side constraints on it are ignored, Cut is just a notational variant on Basic Law V. Clearly, then, from unrestricted Cut, we could derive Russell's contradiction. If we define a Russell property R by: $Rx \leftrightarrow \exists F\ (x = \#F \wedge \neg Fx)$, then by unrestricted Cut we have: $\#R = \#R \leftrightarrow \forall x\ (Rx \leftrightarrow Rx)$, whence: $\#R = \#R$—so $\#R$ exists, and we may proceed:

1	(1)	R(#R)	assn
1	(2)	$\exists F\ (\#R = \#F \wedge \neg F(\#R))$	1, Def R
3	(3)	$\#R = \#F \wedge \neg F(\#R)$	assn
3	(4)	$\#R = \#F$	3 $\wedge$E
	(5)	$\#R = \#F \leftrightarrow \forall x\ (Rx \leftrightarrow Fx)$	(unrestricted) Cut
3	(6)	$\forall x\ (Rx \leftrightarrow Fx)$	4, 5 $\leftrightarrow$E
3	(7)	$R(\#R) \leftrightarrow F(\#R)$	6 $\forall$E
3	(8)	$\neg F(\#R)$	3 $\wedge$E
3	(9)	$\neg R(\#R)$	7, 8 $\leftrightarrow$E
1	(10)	$\neg R(\#R)$	2, 3, 9 $\exists$E
	(11)	$R(\#R) \rightarrow \neg R(\#R)$	1, 10 $\rightarrow$I
12	(12)	$\neg R(\#R)$	assn
	(13)	$\#R = \#R$	=I
12	(14)	$\#R = \#R \wedge \neg R(\#R)$	12, 13 $\wedge$I
12	(15)	$\exists F\ (\#R = \#F \wedge \neg F(\#R))$	14 $\exists$I
12	(16)	$R(\#R)$	15 Def R
	(17)	$\neg R(\#R) \rightarrow R(\#R)$	12, 16 $\rightarrow$I
	(18)	$R(\#R) \leftrightarrow \neg R(\#R)$	11, 17 $\leftrightarrow$I

With the constraints on Cut in place, however, this derivation will not go through without two further assumptions: to establish the existence of $\#R$, and to justify the (second-order) $\forall$E step involved at line (5), we must assume that R is a cut-property on R^{N^+}; and for the application of $\forall$E at line (7), we must further assume that $\#R$ is in R^{N^+}. Since the contradication at line (18) depends upon these further assumptions, we may apply *reductio* to infer that either R isn't a cut-property on R^{N^+}, or $\#R$ is not an element of R^{N^+}.

Does that settle the matter? Well, no. The particular cut-abstraction principle I've used may be viewed as a special case of a *general schema* which runs:

(#) $\#F = \#G \leftrightarrow \forall a\ (Fa \leftrightarrow Ga)$ where F, G are any cut properties on a suitable domain Q and a ranges over Q.

A suitable domain Q here will be any domain with an at least dense linear ordering, with respect to which cut-properties are definable. Two obvious questions which may be raised about this general schema are: Are all its instances safe? If not, what distinguishes those which are from those which are not? I'll venture a few somewhat tentative thoughts about these questions.

Perhaps the first thing I should say is that I am *not*, so far as I can see, committed to endorsing *all* instances of (#)—*i.e.*, to defending its universal closure with respect to Q—though I would think that, should it prove that some of its instances are either prone to Russell trouble or otherwise unsafe, it should be possible to provide some principled characterisation/explanation of the limitations here.

It is clear that so long as the underlying domain Q for an instance of (#) is *not inclusive of all objects whatever*, any derivation of Russell's contradiction can be seen, not as showing the inconsistency of that instance (#), but as a demonstration that either the Russell property R cannot be a cut-property on Q or the Russell cut $\#R$ cannot be an element of Q. If the *universe of all objects whatever* constitutes an admissible underlying domain for cut-abstraction, then the Russell cut, if there is such an object at all, must belong to that domain—so the second option lapses. But the first remains open. There will be such an object as the Russell cut only if the Russell property is a cut-property on the universe. But, at least in the absence of any compelling independent reason to think (#) defective, a derivation of the Russell contradiction would seem to give us ample reason to think that the Russell property *cannot* be a cut-property on the universe.

If what I have said is right, it is possible to block Russell trouble without challenging the assumption that the universe constitutes an admissible underlying domain for cut-abstraction. The point is, however, somewhat academic since there are other worries—having more to do with Cantor's paradox than with Russell's—which are, I think, best answered by rejecting that assumption. Briefly, cut-abstraction, for all I have said thus far, may be applied to any domain on which cut-properties are definable—that is, any domain with an at least dense linear ordering. If the chosen domain is *strictly dense* (*i.e.*, *dense*—like the rationals— but *not complete*—like the reals), then an instance of cut-abstraction will *inflate*, in the sense that there are more abstracts 'generated' than there are objects in the underlying domain (*i.e.*, the domain on which the cut-properties are defined).[19] If it is dense but complete, then there will be no inflation—the collection of abstracts will be isomorphic to the underlying object domain. If the universe of all objects whatever admits of a strictly dense linear ordering and can be taken as a domain for cut-abstraction, we shall wind up with more abstracts (and so more objects) than there are objects altogether! How should we avoid this disastrous conclusion?

The answer I shall tentatively commend makes crucial play with the contrast I drew previously between *unrestricted* abstractions, such as Hume's principle, and *restricted* ones, such as cut-abstraction. In the case of Hume's

[19] As an anonymous referee, Stewart Shapiro, and his student Roy Cook (independently) pointed out to me, cut abstraction inflates at every cardinality, in the sense that, for every cardinal κ there is a domain of size κ with a strictly dense linear order on it, so that cut abstraction applies to yield a 'new' domain of size 2^{κ}.

principle, it is essential that the first-order quantifiers on its right-hand side be allowed to range unrestrictedly over all objects whatever, including—crucially—the numbers themselves. In this sense, the first-order quantifiers in Hume's principle must be understood impredicatively. If instead those quantifiers were restricted so as to range only over objects other than numbers, we could not prove the infinity of the sequence of finite numbers—at least, not without the additional assumption that there exist infinitely many objects of some other kind.

With cut-abstraction, by contrast, it is unnecessary—in order to ensure that the abstraction delivers all the abstracts we require—to construe its first-order quantifier impredicatively in this way. Moreover, if we do allow that—in particular, if we allow an instance of the cut-schema whose first-order quantifier ranges over all objects whatever—then we will (provided the universe admits of a strictly dense ordering) run into Cantor-type trouble. But we do not *have* to allow this. As I have explained, cut-abstraction is—in contrast with Hume's principle, and Basic Law V—a restricted abstraction, in the sense that each instance of the cut-schema (#) involves a restriction to a specified underlying domain, over which its first-order quantifier ranges. All I have said thus far about what constitutes a suitable underlying domain is that it shall be some densely ordered collection of objects. But as far as I can see, nothing stands in the way of imposing a further restriction which will preclude application of cut-abstraction to the universe as a whole.

It may seem that the most obvious way to do this would be to incorporate a 'limitation of size' requirement in the conditions for a suitable domain for cut-abstraction—the idea would be to require that any suitable domain Q for cut-abstraction be smaller than the universe. This would bring cut-abstraction much closer to the modified version of Basic Law V which George Boolos dubbed New V. Following Boolos, say that a concept F is a *subconcept* of a concept G iff $\forall x\ (Fx \rightarrow Gx)$, and that F goes into G iff $F \approx H$ for some subconcept H of G. Let V be the concept $[x : x = x]$, and say that F is *small* iff V does not go into F. Define F to be *similar* to G iff (F is small $\vee$ G is small $\rightarrow \forall x\ (Fx \leftrightarrow Gx)$). Similarity is an equivalence relation. New V is then the abstraction:

New V $\qquad {}^{*}F = {}^{*}G \leftrightarrow F$ is similar to G.

If we agree—as I think we should—that numbers may only properly be assigned to genuine sortal concepts—that is, roughly, concepts F with which are associated not only criteria of application but also criteria of identity—then we should be happy with this modification (of either cut-abstraction or Basic Law V) only if we are persuaded that *self-identity* is a genuine sortal. For if a concept F can have a number only if F is sortal, then, assuming Hume's principle, F can be equinumerous with itself only if it is sortal. And if it can't be equinumerous with itself, it can scarcely be equinumerous with any other concept. Since *small* is defined so that F is small *iff self-identity* doesn't go into F, New V is a real restriction of Basic Law V only if *self-identity* is a genuine sortal.

I do not think it is. A simple argument due to Crispin Wright shows, in effect, that if self-identity were a genuine sortal, many concepts which are plainly not sortal would qualify as such. The argument turns on the point that whenever a concept G is genuinely sortal, its restriction by any other (even merely adjectival) concept F—*i.e.*, the conjunctive concept: F-and-G—will likewise be sortal. For example, since *horse* is, presumably, genuinely sortal, so is *white horse*, for all that the restricting concept *white* is not sortal. Thus if *self-identical* were a genuine sortal, so would be any restriction of it, such as *white-and-self-identical*. However, since *white-and-self-identical* is equivalent to *white*, it would follow that *white* is after all a sortal concept. Since *white* (or *white thing*) is *not* a genuine sortal, neither can *self-identical* be one. For the same reason, clearly, *no* concept which applies *universally* can be a genuine sortal concept.[20]

If this is right, some other means of formulating the needed restriction is required. There is an obvious next thought. Why should we not simply stipulate that a predicate Q determines a suitable domain for cut-abstraction *only if* Q is genuinely sortal? Since neither *self-identity*, nor any other predicate (such as '$F \vee \neg F$') which is guaranteed application to all objects whatever, is a genuine sortal, this will ensure that the universe of objects as a whole—even if it admits of a strictly dense ordering—is not an admissible domain for cut-abstraction.

A thorough defence of this proposal requires more space than I have here. To conclude, I should like to comment briefly on three points.

(i) It might be observed that a restriction of admissible domains to those specifiable by sortal concepts will not, on the face of it, exclude certain very large domains such as those comprising all ordinals, or all cardinals, or all sets (since the relevant concepts appear to qualify as genuinely sortal)—giving rise to concern that paradox may still be derivable from cut-abstraction by taking one or other of these collections as underlying domain. I think this might be met in either of two ways. *First*, any at-

[20] Cf. Wright [forthcoming]. Wright formulates the argument slightly differently, as follows:

> Call a concept that is not sortal a mere predicable. Where F is a mere predicable, the question: 'How many F's are there?', is deficient in sense and 'the number of F's' has no determinate reference. However, attaching a mere predicable to a genuine sortal, G, produces a complex, restricted sortal, $F\&G$, such that there can be, and normally will be, a determinate number of objects falling under it. Thus if F is any mere predicable, and self-identity is a genuine sortal, there will be a determinate number of objects whch are *F&self-identical*. But since *F&self-identical* is equivalent to F, it follows that there can be no such determinate number wherever there is no determinate number of F's—*i.e.*, wherever F is a mere predicable. So self-identity is not a sortal concept.

tempt to generate paradox from (#) by taking the ordinals, say, as domain will—so far as I can see—rely on the idea that the collection of all ordinals is universe-sized. That requires the assumption that the concept *ordinal number* is equinumerous with some concept under which every object—whether an ordinal number or not—falls. But if what I have already said is right, concepts can be equinumerous only if both are sortal, and there can be no universal *sortal* concept, so that this assumption can be rejected, and there will be no need to strengthen the restriction on cut-abstraction to preclude taking the ordinals, *etc.*, as domains. But *second*, even if it should prove necessary to exclude the ordinals, *etc.*, as admissible domains for cut-abstraction, there is a quite natural way to do this. Instead of requiring simply that an admissible domain be given by a sortal concept, we might require that such a domain should have a *determinate cardinal size*. Since being the extension of a sortal concept is at least a *necessary* condition for a collection to have a determinate size, this restriction would encompass the one already proposed. If this necessary condition is not sufficient—*i.e.*, if certain sortal concepts fail to have determinately-sized extensions—then those concepts will be excluded by the revised restriction. In particular, what Michael Dummett has called indefinitely extensible concepts, such as *ordinal*, *cardinal* and *set* itself, will be excluded.

(ii) It may be objected that restricting admissible domains for cut-abstraction in either of the ways suggested is *arbitrary* or *ad hoc*. And the objection might be thought to draw strength from the neo-Fregean's willingness (and, indeed, need) to employ *unrestricted* abstractions such as Hume's principle. I shall make just two quick points in reply, leaving—no doubt—much more to be said. First, as should by now be clear, it is in fact *false* that Hume's principle is a *completely* unrestricted abstraction—although its first-order quantifiers are unrestricted, its initial *second*-order quantifiers are—crucially—restricted to *sortal* concepts. Second, my proposed restriction(s) on cut-abstraction appear to be no *more* arbitrary or *ad hoc* than the restriction which New V seeks to build into Basic Law V. It is true that the manner in which the restriction is imposed on (#) differs, formally, from what happens with New V—where what is done is not to restrict the range of any quantifier, but to complicate the equivalence relation—with the effect that when F and G are not small, $^*F$ and $^*G$ exist, but are identified irrespective of whether their concepts are co-extensive. But I think this difference is superficial. Provided that the conditions for a first-level concept to be sortal can be expressed (using only logical vocabulary) in a language of second (or perhaps third) order, I can see no reason why (#) should not be recast in essentially the same mould as New V. And if they cannot be so expressed, that is bad news (if it really is bad) not only for (#) but for New V too, for reasons already mentioned. But I am not persuaded that it *would* be bad news—since I see no ground for assuming that every philosophically important concept must be capable of definitive expression in the purely logical vocabulary of a second- or third-order language.

(iii) Finally, a quick word about the state of the economy. Some recent writers[21] have claimed—plausibly, in view of the obvious risk of some form of Cantor's paradox—that acceptable abstractions should be, in some sense, *non-inflationary*. Is cut-abstraction inflationary in any objectionable sense?

Some care needs to be exercised in characterising the relevant notion of inflationariness, since a great part of the point and interest of abstractions lies in the fact that they 'generate' objects which are 'new', and so, in a certain sense, 'expand' the underlying domain. So that in one way, inflation—or at least domain-expansion—is just what the neo-Fregean wants. Of course, this way of putting the matter is potentially very misleading, since it gives the entirely false impression of ontological prestidigitation—in which abstraction creates objects out of nothing, as it were, much as a practised conjurer appears to pull pigeons out of thin air. The neo-Fregean can, and should, insist upon a more sober description of what is going on. What an abstraction does, if all goes well, is to *set up a concept*—of *direction*, or *cardinal number*, or whatever—by supplying necessary and sufficient conditions for the truth of identity-statements linking terms which purport reference to objects falling under it. It draws our attention to the possibility of redescribing—or reconceptualising—the state of affairs which consists in line a being parallel to line b, for example, in terms of the holding of the relation of identity between certain objects, *the direction of a* and *the direction of b*.[22] Accepting the proposed reconceptualisation does not—in and of itself—involve acknowledging the existence of these objects. What it involves, rather, is accepting that the question whether there are such objects reduces to the question whether suitable instances of the right-hand side of the abstraction principle are indeed true. So what an abstraction does is not to 'create' objects, but to equip us to recognise, identify and distinguish objects which we could not recognise, identify and distinguish before—*i.e.*, in advance of grasping the concept which the abstraction introduces.

If inflation of this kind is acceptable, what kind might not be? Kit Fine writes:

> Two necessary conditions for the truth of an abstraction principle hold as matter of logic .... In the first place, it follows from the truth of an abstraction principle that its underlying criterion of identity on concepts should be an equivalence relation ...
>
> Secondly, it follows from the truth of an abstraction principle that the identity criterion should not be inflationary, the number of equivalence classes must not outstrip the number of objects. There must, that is to say, be a one-one

[21] See Fine [1998].

[22] For fuller discussion of this idea, see Wright [1997], §I; Hale [1994], §2; and Hale [1997] *passim*.

correspondence between all of the equivalence classes, or their representatives, on the one hand, and some or all of the objects, on the other. It is, of course, on this score that Law V proves unacceptable; for where there are n objects, it demands that there be 2^n abstracts. (Fine [1998], p. 506.)

There is, I think, some ambiguity or vagueness in these remarks which we need to resolve if avoidable confusion is to be avoided. Let us say that an abstraction A *inflates on an underlying domain* D if A's equivalence relation partitions D into more equivalence classes than D has elements. Then one might say that an abstraction is *weakly inflationary* if there is *some* domain on which it inflates, and *strongly inflationary* if it inflates on *every domain* (or perhaps—a little less exiguously—on some domain of cardinality κ, for every cardinal κ).[23]

To require of an acceptable abstraction that it should not be (even) *weakly* inflationary would stop the neo-Fregean project dead in its tracks, before it even got moving (as it were). It will be clear that I think there is no good ground to impose such a requirement, and I shall not discuss it further. It is much more plausible to require that acceptable abstractions should not be *strongly* inflationary.[24] Some of the neo-Fregean's key abstractions, including the other crucial second-order abstraction, Hume's principle, satisfy this requirement.[25] But whilst the requirement that abstractions not be strongly inflationary is *more* plausible, I can see no compelling reason to accept it *in full generality*—that is, as applying *both* to *un*restricted abstractions and *restricted* ones. It *may* be necessary to insist that no *un*restricted abstraction can be strongly inflationary. But, as I

[23] This characterisation of weak and strong inflation applies directly only to abstractions—like Hume's principle and Basic Law V—which are not restricted abstractions in the sense previously explained, *i.e.*, are not such that their formulation already involves a specification of a particular domain as the underlying domain for the abstraction. Since any particular cut-abstraction, such as Cut, *is* restricted in this sense, there can be no question of its being strongly inflationary. We can, however, properly ask of the corresponding *general schema*—(#) in the case of Cut—whether it is strongly inflationary.

[24] More plausible, because it might seem that strong inflation is bound to give rise to a version of Cantor's paradox. It might also be thought that if an abstraction is strongly inflationary, then there could be no hope of showing that it is satisfiable, *i.e.*, has a model—for let D be any domain, of cardinality κ, say. Then any strong abstraction inflates on D, *i.e.*, its equivalence relation partitions D into more than κ equivalence classes, and so 'generates' more than κ abstracts. Thus D cannot be a model for the abstraction. But D was any domain whatever, so our abstraction can have no models. On reflection, it should be apparent that this short argument involves an unstated assumption—that the domain of any putative model for an abstraction must be the underlying domain for the abstraction. As against this, I cannot see why, in setting up a model for a *restricted* abstraction—such as cut-abstraction—we should not choose as the domain of the model some larger collection which properly includes the collection which is to play the rôle of the underlying domain for the abstraction.

[25] Hume's principle inflates, of course, on any finite domain, but can be shown—assuming Choice, but without assuming CH or GCH—that it does not inflate on any infinite domain.

have tried to make plausible, it is unnecessary to require this of *restricted* abstractions. The cut schema, in particular, is strongly inflationary in the sense that for every cardinality κ, there is an admissible domain of cardinality κ on which an instance of (#) inflates. But that, so far as I can see, does no harm, provided admissible domains are restricted to those given by genuine sortal concepts (or perhaps, those of determinate cardinal size).

5. Summary and Concluding Remarks

My aim in this paper has been to set forth one plausible way in which a neo-Fregean account of arithmetic may be extended to encompass the real numbers. I have followed Frege himself in suggesting that the reals should be introduced as ratios of quantities. This approach, as Frege perceived, demands a prior analysis of the notion of quantity. I have agreed with Frege, too, in thinking that this should be done by providing a general characterisation of what he called quantitative domains, but have offered a somewhat different account of them from that given in *Grundgesetze*. Ratios of quantities are introduced by an abstraction principle based on the ancient theory of proportion which comes down to us from Eudoxos. The positive reals are then obtainable as ratios of quantities in a complete quantitative domain, and zero and the negative reals by essentially the move by which the integers are standardly constructed as difference-pairs of natural numbers. My construction, taken by itself, establishes *only* a *conditional* result: *if* there exists a complete quantitative domain, then the reals may be introduced as ratios of quantities on it. However, as I argue in the second half of the paper, there is a route by which a neo-Fregean may establish the existence of at least one complete domain, starting with the natural numbers (as given by Hume's principle), by successively applying ratio-abstraction to obtain a full domain and a suitably adapted version of Dedekind's method of cuts to obtain from this a complete domain.

Two points deserve emphasis: first, quantities, though (on my account) abstract objects which are sharply to be distinguished from the concrete entities which stand in various quantitative relations to one another, are *not* themselves to be *identified* with numbers; and second, although I use a version of Dedekind's method in proving the existence of a complete domain, there is no question, on the present approach, of *defining* the reals as or in terms of Dedekind cuts. Here is not the place to elaborate upon the significance of these points. The first is, I believe, integral to the defence of my approach against several more or less familiar objections to older attempts to treat real numbers as directly abstracted from quantitative relations among concrete entities—but that defence is best conducted in the context of a more searching analysis of the notion of quantity than I have had space for here. Such an analysis would also do much to motivate the axiomatic characterisation of quantitative domains which I have been obliged to state somewhat dogmatically, without the philosophical defence it surely requires. The second is essential to the claim of the present approach to respect Frege's belief—I would say, insight—that a satisfying foundational account of the real numbers should introduce them in a way which expressly provides for their applications.[26]

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[26] If one disregards this constraint—as I think one should not—then it would, of course, be possible to obtain the reals by Fregean abstraction in a much simpler and more direct way than I have described. One might, for example, start with the natural numbers as given by Hume's principle, obtain rationals by some form of ratio-abstraction (such as that employed here, but there are obviously other ways in which this might be done) and then directly introduce the reals as cuts by cut-abstraction (either as explained here, or in some similar way).

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ABSTRACT. On the neo-Fregean approach to the foundations of mathematics, elementary arithmetic is analytic in the sense that the addition of a principle which may be held to be explanatory of the concept of cardinal number to a suitable second-order logical basis suffices for the derivation of its basic laws. This principle, now commonly called Hume's principle, is an example of a Fregean abstraction principle. In this paper, I assume the correctness of the neo-Fregean position on elementary arithmetic and seek to explain one way in which it may be extended to encompass the theory of real numbers, introducing the reals, by means of suitable further abstraction principles, as ratios of quantities.