IMPREDICATIVITY AND TURN OF THE CENTURY FOUNDATIONS OF MATHEMATICS: PRESUPPOSITION IN POINCARE AND RUSSELL

by

Joseph Romeo William Michael Picard

B.A. Honors, Philosophy, University of Calgary, 1986

M.Sc., Philosophy, Massachusetts Institute of Technology, 1990

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Signature of Author ______________________
Department of Linguistics and Philosophy
July 8, 1993

Certified by ______________________
Professor Richard Cartwright
Thesis Supervisor, Department of Linguistics and Philosophy

Accepted by ______________________
Professor George Boolos
Chairman, Departmental Committee on Graduate Studies

ABSTRACT

The primary purpose of this dissertation is to state a modal account of impredicativity. A (formal or explicit) definition (under a particular interpretation) is *impredicative* if the object defined in that interpretation is a value of a bound variable occurring in the definition. An object may also be called *impredicative (with respect to a given language)*, namely just in case it can be defined in that language but only by means of an impredicative definition. Poincaré charged in (1906) that impredicative definitions are viciously circular and that impredicative objects do not exist. Russell agreed with these charges and went on to formulate a logic (ramified type theory) on the basis of a principle which banned impredicativity (vicious circle principle). The main purpose in this dissertation is to show how certain modal-semantic considerations can be used to make sense of the subject of impredicativity, and give an interesting account of what it is for an object to be impredicative. A secondary purpose is to rebut in amore direct manner the charge of vicious circularity.

Chapters 1 and 3 are on Russell. In Chapter 1, I examine Russell's early idealist work (1895-1898) in the foundations of geometry. Although Russell increasingly disassociated himself from this work, as indeed from Kant and Hegel, an examination of Russell's idealist foundations can shed light on Russell's later ban on impredicativity. Russell's idealist metaphysical views (especially regarding the continuum) make extensive appeal to modal notions such as essentiality and presupposition (ontological dependency). It was largely his change in attitude toward just these modal notions that lead him to reject idealism and adopt in its place logical atomism and an analytic philosophical methodology. The modal account of impredicativity I give in Chapter 3 will rely chiefly on modal notions Russell rejected when he abandoned his idealist philosophy. Thus the purpose of the first chapter is largely historical: to sketch Russell's views regarding essentiality and ontological presupposition as they were applied in foundations of mathematics.

Chapter 2 concerns Poincaré. I present Poincaré's views in the foundations of arithmetic and geometry prior to his rejection of impredicativity in 1906. I then try to highlight certain tensions in his thought which the rejection of impredicativity created. These tensions arise from Poincaré's use of Kant's claim that mathematical knowledge is based upon synthetic *a priori* intuition. The principles Poincaré held such intuition to justify require, for their proof, the use of impredicative definitions or the postulation of impredicative objects. Poincaré took his ban on impredicativity to show that explicit proofs of these principles were not possible, and that therefore these principles presupposed a role for synthetic *a priori* intuition. I argue that this conclusion is misguided, and that Poincaré does not successfully avoid impredicativity in the foundations.

In Chapter 3 I discuss Russell's ramified type theory and argue first that Russell's motivations for introducing this theory can be expressed as certain modal prejudices Russell held. I then extend the modal notions used to express Russell's motivations to define a notion of mutual presupposition or reciprocal ontological dependency, which can be seen to constitute the impredicativity of objects in the context of ramified type theory. One outcome of my modal account of impredicativity is an explanation why Russell thought his restriction to predicative definition could be justified on strictly logical grounds. Russell's later atomistic metaphysics simply ruled out the use of the modal notions required to make sense of impredicative objects.

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I dedicate my dissertation to Patricia and Keith Harriell, who have been to me family for the last two years. It is still incredible to me that such a chance arrangement as ours should have proven so deep a source of strength and contentment for me. I will miss it, and I will miss them.

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0. Introduction

0.1 Opening Remarks

The subject of this dissertation is impredicativity. The term "impredicative" can significantly be applied to a variety of things. I will begin by saying how the term "impredicative" is to be used in this work, and then go on to say why impredicativity has been thought to be problematic.

The word "impredicative" was applied originally to definitions, which one can think of for present purposes as stated in a formal or symbolic language. A definition or, more precisely, a definition under a particular interpretation, is called *impredicative* if the object defined on that interpretation is a value of a bound variable occurring in the definition. By extension, the term "impredicative" can be applied to objects, which is to say, to the values of bound variables. It is in fact here that the subject of impredicativity becomes most interesting philosophically. An object may be said to be *impredicative (with respect to a given language)* just in case it can be defined in that language but only by means of an impredicative definition. The idea here, very roughly, is that if it is impossible to say what an object is without supposing that it exists, then the object is impredicative. This comment, and these definitions of "impredicative," are subject to qualifications, which I will only get to below. First I want to say why impredicativity has been thought to be controversial.

Objections to impredicativity are usually framed in terms of definitions, but the harder philosophical problems have to do with impredicative objects. It is said that impredicative definitions are viciously circular. From this, it has been charged, one may conclude that there are no impredicative objects (at least with respect to languages used in the foundations of mathematics, where the subject of impredicativity first arose). Henri Poincaré, who first objected to impredicative definitions in a general way, held that we humans create mathematical objects by an act of definition. He claimed that it was impossible to create an impredicative object, because such an object had already to be presupposed to exist before it could be defined at all. If an object can be defined only by assuming that it is there to be quantified over in the first place, then we can't be said to have created it ourselves. Since all mathematical objects are created by our defining them, impredicative objects cannot exist.

Poincaré's argument has had its followers. Bertrand Russell, though he disagreed with almost everything else Poincaré believed in the philosophy of mathematics, was among those who accepted the basic drift of Poincaré's idea. What Russell did *not* accept was that mathematical objects were created by the mind. Still, Russell accepted that impredicative definitions were viciously circular, and he denied that any impredicative objects existed (as far as any language was concerned which he used in the foundations of mathematics). Obviously, Russell based these conclusions on arguments very different from Poincaré's as stated above. Somehow, the combined efforts of two thinkers of otherwise very different outlooks has seemed to leave impredicativity very much in need of defense.

I believe that impredicative definitions are not in general viciously circular, and also that impredicative objects might well exist. (I do not believe that we create mathematical objects, but that is not my main concern in this thesis). I want to show here how certain semantical considerations can be used to make sense of the subject of impredicativity, and give an interesting account of what it is for an object to be impredicative. The semantical considerations involved use modal notions such as necessity, essentiality and presupposition. In this sense, my account of what it is for an object to be impredicative is an importantly *modal* account.

The literature on the objections of Russell and Poincaré to impredicativity has tended to concentrate almost exclusively on the series of articles in which the notion of impredicativity received its initial formulations.[1] I take a different tack. Anyone who has looked at this exchange is impressed by the pervasive mutual misunderstanding that occurs in it. Several of the key terms (such as "logic," "intuition" and "definition") are understood quite differently by the two writers. It seems to me that one way around this interpretive difficulty is to look farther back in the writings of both authors. In this way, some of the presuppositions with which they entered the debate may be recognized more distinctly. In addition, some of their own ideas about modality can be used to motivate my own modal account of impredicative.

0.1.1 Outline

I would like now to give some indication of the overall structure of the present work. In Chapter 1, I examine Russell's very early work in the foundations of geometry. Despite the fact that Russell increasingly disassociated himself from this work, and from the idealist philosophical framework in which it was carried out, I believe there is much to learn from Russell's early efforts about the notion of impredicativity. Russell's metaphysical views at this time make extensive appeal to modal notions such as necessity and essentiality, and his eventual rejection of his early work was very largely motivated by a change in attitude concerning just these notions. I will claim that certain of the modal

[1]This series is: Russell (1905a), Poincaré (1905), Russell (1906), (1908), Poincaré (1909), (1912). Heinzmann (1986) collects all these and other primary source materials.

notions he rejected when he abandoned his idealist philosophy are precisely those required for a correct understanding of impredicativity. One outcome of my modal account of impredicativity is an explanation why Russell thought his restriction to predicative definition could be justified on strictly logical grounds. Russell's later metaphysics simply ruled out the modal notions required to make sense of impredicative objects.

In Chapter 2, I turn to Poincaré. Poincaré developed many of his opinions in the philosophy of mathematics long before he formulated the notion of impredicativity in 1906. There are some strains in his early thought which anticipated the notion, but there are others which did not sit well with the notion once it had been introduced. My general purpose in this chapter is to highlight the tensions in his thought which the introduction of the notion of impredicativity creates. These tensions arise primarily from Poincaré's reliance in the foundations of mathematics upon the ideas of Immanuel Kant. In particular, Poincaré insisted that our mathematical knowledge was founded upon synthetic *a priori* intuition. But the principles he held such intuition to justify are such as require, for their proof, the use of impredicative definitions. When Poincaré came to reject the legitimacy of such definitions, he thought he had a new argument why the proofs of these principles should be rejected. He wished to conclude from this, not that the principles themselves should be rejected, but rather that they should be accepted on the basis of so-called pure intuition. I argue that this conclusion is misguided, and that Poincaré does not successfully avoid impredicativity in the foundations.

Chapter 3 is the heart of the present work. In it I discuss Russell's ramified type theory and argue first that Russell's motivations for introducing this theory can be expressed as certain modal prejudices Russell held. I conclude that Russell's prejudices which led him to the

conclusion that impredicative definitions were impermissible, and that impredicative objects cannot exist. I then extend the modal notions used to express Russell's motivations to a
define a notion of mutual presupposition or reciprocal ontological dependency, which can be seen to constitute the impredicativity of objects in the context of ramified type theory.

This dissertation also includes two bibliographies. One is a list of the works referred to in body of the dissertation. I explain there how references are to be made in this work. The other bibliography covers the subjects of predicativity and impredicativity. It is, I think, the most extensive on the subject.

0.2 Two Informal Examples.

In this section, I discuss the concept of impredicativity in more detail and sketch a fuller account of its nature. As is clear from the above definitions, the concept of impredicativity is formal and rather technical, but a few informal examples are fairly widely discussed. I try here to motivate some of the technicalities by showing how two of the earliest and most common informal examples are in certain respects misleading.

In the philosophical literature surrounding the formal studies of impredicativity there exist several non-technical examples of impredicativity. Although useful for introductory purposes, these examples bear undeniable limitations. A closer consideration of two of these examples will bring these limitations into clearer focus. The examples may be put in the form of definite descriptions, thus: "the tallest person in the room" and "the property of having all the properties of a great general".[2] The impredicative feature of both these examples consists in the use of the universal quantifier *all*. In the second example, one quantifies over *all* properties of a certain sort, and the impredicativity consists in the fact that the property to which one wishes in particular to refer falls within the quantifier's intended range. And when we speak of the tallest person in the room we mean a person in the room tallest among *all* those in the room, who thus, on grammatical grounds alone, falls into the intended range of the quantifier implicit in the superlative. When, therefore, we seek to define (single out or uniquely characterize) an entity such as this tall person or that martial property, we do so *impredicatively* if we employ the concept *all* and intend this to cover, among other entities, precisely that which we seek to define.[3]

The limitations are as follows. The first example is fine as an impredicative *definition*, but it does not define an impredicative *entity*. This distinction is of paramount importance to the study of impredicativity. It is easy enough, in most cases, to go up to the tallest person and point to him or her. We could also call out an appropriate proper name, if we know one; or, provided with suitable instruments, we could make various measurements, and discuss instead the person of such-and-such a height (there being, as it eventuates, just one of them in the room). These methods, on assorted assumptions, single out the tallest person just as well as the impredicative method, and there seems in

[2]Definite descriptions may be thought of as definiens of explicit definitions; thus I will sometimes speak of the descriptions as themselves definitions. The first example is slightly modified from Ramsey (1926 204), the second is found in Russell (1919 189 ff; 1959 120-6), who supplies a number of inessential variations. The problems with two examples chosen are quite distinct. Many informal examples discussed in the literature on impredicativity fall in with Ramsey's, for instance Quine (1969 243) and von Kutschera (1976 171), "Die Spezies mit der kleinsten Umfang aus der Menge der Spezies der Gattung *Rosa*". The remarks I wish to make about Ramsey's example carry over to these.

[3]The existential quantifier "some" can also be used impredicatively. We may provisionally define "some" in the usual way as "not all not" and continue to speak simply of "all".

principle always to be some way to refer to ordinary objects other than through an impredicative use of the word "all". Objects ordinarily have no shortage of characteristic features expressible without any quantification whatsoever, and the mention of any one of these as a definition suffices, as does, often enough, the mention *in context* of non-unique features. Hence if we can not *in fact* predicatively refer to such an object, it seems this will always be due to an artificial restriction in our capabilities — tied hands, inessential ignorance, or a shortness in metersticks — and will have nothing to do with the object itself. Yet it is precisely when our inability *stems from the object itself* that we say that it, and not just our means of characterizing it, is impredicative. *Impredicativity is an ontological issue.*

The second example appears, in our need, to be just such a case, for it is difficult to imagine being *given* the special property of having *all* the properties of every great general in a way which avoids quantifying over all such properties. One speaks here of a particular property (which if it exists at all is certainly an object) but one can do so, it might seem, only in terms of all the properties of a sort to which the first already belongs. There are, however, two related objections that curtail the philosophical utility of this second impredicative definition. The first is that it isn't at all obvious that *this* property *is* an object (that is, a value of a bound variable); and the second is that properties generally speaking, if indeed objects, are not *obvious objects*. Both of these objections require explanation.

When discoursing of generals and their properties, the times are few when we must irreducibly speak of a special property P of having *all* the properties of a great general. Why not instead speak severally of all those properties? Now surely, *if* P *exists*, it falls within the intended range of the quantifier "all", so that to speak of all here is to speak of

P as well. But we lack good reasons to admit P's existence in the first place. For when is it *theoretically necessary* to attribute a *single* property P rather than a variety of properties, each displayed by every great general? More precisely: need a general who has each of these properties *besides P* also have a single property consisting in having all of them (and P)? Not *obviously* so, in any case. Moreover, in order to turn these questions into affirmations, we might just have to survey all the contexts in which discourse of the properties of generals could find a place. This would not be easy — it might even be impossible — which alone shows that the *need* to quantify over P is not obvious. P is not obviously an object, *a fortiori* not obviously an *impredicative* object.

The second objection to the second example is that properties, if sometimes obviously objects, are not *obvious* objects: we don't really know what they are. To see the force of this, we admit outright, contrary to the last objection, that P is after all an object, and we inquire whether it really is impredicative. If so, it must not be possible to characterize P without admitting it as the value of a bound variable. We must try, therefore, to restrict the range of our variables so that P is excluded, and check if our words are thereby invariably prevented from denoting P. Now, if this be the test, we needn't go very far to conclude that P seems *predicative*. For we need merely relegate P to a higher logical type than everything else in the *intended* range of the variables occurring in the given description. As in the last objection, we remove P from the intended interpretation of the variables and consider only, so to speak, all the properties *besides* P of great generals. *Unlike* the last objection, however, we admit P to be an object, and indeed to be a property of all great generals: we question merely whether it is not equally well denoted by the description under the modified interpretation (with types) as it was under the unmodified interpretation (without types). Certainly nothing appears to prevent us from referring to P in the new situation (nor even to suggest, if we do not, whether we refer to something other than P). Admittedly, to note these appearances and to raise our question is not yet to argue. But this is just the point: to argue one way or another would require having a good idea of what counts as *the same property*: it would require an account of the identity conditions of properties. We should have to know more about properties than we do. Properties are for us in this respect inobvious objects, and this prevents us from honestly deciding on the predicativity of P.[4]

To sum up, if P is either not obviously an object, or not obviously distinguishable from the entity (if any) referred to by the typified description just discussed, then P is not obviously an impredicative object. The second example, quite as much as the first, fails to *present* us with an impredicative object.

The moral of all this is that the study of ontological impredicativity does best when we have clear identity conditions for the objects under consideration. Typically we have clear identity conditions only in somewhat artificial settings, such as formal theories concerning sets or mereological wholes. In a formal setting, we have the related advantage of being in a position to survey all the strings of symbols which may be used as a definition. In these convenient circumstances we can sometimes even say when it is *theoretically necessary* to attribute to an object a property whose any adequate expression employs quantification over that object, something which is, naturally, of crucial importance in regard to judging ontological impredicativity in individual cases. Finally, we may even quite literally speak of not being able to define an

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[4]The discussion here has an entirely different point than Russell's. For Russell the description as interpreted without types has no meaning or denotation at all (even in context). Thus, for him, we do not even have a *candidate* example of an impredicative entity, in the sense of "impredicative" being introduced here. Outside the context of Russell's type-theoretic assumptions, however, the suggestion of impredicativity is clear, if ultimately misleading.

object, or of being able to define it only under certain assumptions, for that will be understood relative to the interpretations of the kinds of languages in question. This relativization may appear to weaken the informal claim, but in a sense it strengthens it by lending it a clarity and exactitude not accessible to the informal approach. With these advantages in mind, I turn now to a more detailed discussion of the term "impredicative" as it is used in such contexts.

0.3 Definitions

In this Section I discuss certain of the more technical terms that occur in this dissertation. The first of these is the term "impredicative" itself. Above I defined the term "impredicative" as it applies to definitions as follows: An interpreted definition is said to be *impredicative* just in case the object defined on that interpretation is a value of a bound variable occurring in the definition. To this certain qualifications must be added. In first-order logic there is only one type of bound variable. Consequently, if one defines a symbol "S" in such a language and uses bound variables at all, then, provided that the existence of S is provable on the basis of the definition, the definition will be impredicative in the above sense. Since we can typically prove existence when we make definitions (definitions are not particularly useful unless we can prove existence) our definitions will typically be impredicative. This creates a problem: did the opponents of impredicative definitions really want to ban typical definitions?

This problem is in fact rather superficial. In the first place, the usual formal setting for a discussion of impredicativity is not first-order logic, so that more than one type of bound variable will be in use. So long as the type of bound variable used in the statement that S exists is not the same as the type of any bound variable used in the definition itself, the definition will not be impredicative in the above sense. Thus in the usual formal setting for a discussion of impredicativity, definitions will not be impredicative whenever they are useful.

But even when we assume the underlying logic is first-order, there is a conventional way to avoid the conclusion that definitions are typically impredicative. All that is needed is a judicious use of bounded quantification. For example: consider a definition of "S," stated in the language of first-order set theory, that employs a bound variable; and suppose we can conclude in this theory that S exists. By relativizing the quantifiers in the definition to a given set M — that is, by replacing every occurrence of "$\forall x(\ldots)$" in the definition by "$\forall x(x \in M \rightarrow \ldots)$" — one can imitate a logic with distinct types of variables. Although strictly, the bound variable "x" in the definition still ranges over everything in the domain of the interpretation (and thus over S), we can reasonably speak of the variable "x" *as if* it ranged only over the elements of M. After all, the definition relativized to M will define exactly the same entity as that which the original definition would define if the bound "x" in it were interpreted as ranging over just the elements of M. We can then make the convention that our original definition of "S" is *impredicative with respect to a given M* just in case $S \in M$ and the relativization of the definition to M still defines S. Similarly, the object S itself can be said to be *impredicative (with respect to the language of set theory)* just in case no relativization to any set M of a definition in the language defines S unless $S \in M$. In this way, we can avoid the difficulty that most definitions in first-order languages are impredicative.

The term "impredicative" can also be applied, not just to definitions and objects, but also to theorems of a given theory. It sometimes happens that a particular theorem of a given theory cannot be proved unless certain objects can be proved to exist. In the simplest case, the theorem itself affirms the existence of the objects satisfying the definition. In other cases, it is simply not clear how a particular theorem could be proven except by way of defining certain objects. If the objects that must be shown to exist in order for proofs of the theorem to go ahead are impredicative (with respect to the language of the theory), then we can reasonably speak of an *impredicative* theorem (again understood relative to the language). This intuitive idea can be made precise. Feferman (1964) for instance has given a precise sense to "predicatively provable theorem of analysis" and has identified subsystems of classical analysis from which exactly the predicatively provable theorems of analysis are deducible. If a theory has only predicative theorems, it may itself by called *predicative*. If it contains some impredicative theorems, it may be called an *impredicative* theories. In a similar way, a proof or an informal justification of a theorem may be called *impredicative* if the theorem is impredicative.

The fact that the term "impredicative" can sensibly be applied to theorems, as opposed to just definitions or objects, has useful consequences. One relatively common view in the philosophy of mathematics, dating back to Kant, states that certain theorems are knowable through "intuition." Now whatever quite is meant by a given author who employs the term "intuition" to make such a claim, it is clear that the impredicativity (with respect to a given language) of the theorems allegedly knowable through this "intuition" may be ascertainable in the way just now discussed. Under these conditions, it is convenient shorthand to say that the intuition itself is impredicative. A similar remark applies to such familiar (if obscure) phrases from the philosophy of mathematics as "form of experience," "form of understanding" and "category." In most cases (certainly in Russell and Poincaré) these phrases are thought to denote some fact about our minds in virtue of which we may know that specific theorems are true. So long as knowledge of *specific* mathematical theorems is believed to be based upon a "form of experience" or a "category," we can meaningfully speak of such "forms" or categories as predicative or impredicative. They are impredicative just in case the theorems which are thought to be knowable by way of them are impredicative.[5]

There is a further remark to be made concerning the use of "impredicative" as it applies to a theorem. Generality in a theorem is a natural desideratum. Occasionally, a very general version of a theorem is impredicative, while a less general version is not. Again, a convenient shorthand permits one to speak of the instances of *impredicative instances* of a theorem. These are instances of a theorem which follow from a general impredicative theorem, but which do not follow from any less general predicative version.

Before leaving this section I would like to say something about the use of the word "continuous" in this dissertation. In its most technical significance, the term applies to series.[6] A *series* is an ordered pair (K,<) such that K is a non-empty set and < is a 2-place relation satisfying the following conditions (for $a,b \in K$):

$$a \neq b \rightarrow (a<b \vee b<a)$$
$$a<b \rightarrow a \neq b$$
$$(a<b \wedge b<c) \rightarrow a<c$$

A series is *dense* if it is also true of it that, for any $a,b \in K$,

$$a<b \rightarrow \exists x \in K(a<x \wedge x<b).$$

A series is then said to be *continuous* if it is dense and satisfies the following important postulate: for any S and T,

[5]A natural requirement might be that if a given author has principled objections to impredicativity in mathematics, then no theorems he or she holds to be knowable on the basis of an intuition, category or form of experience may be impredicative. It is just such a requirement that Poincaré will be seen to violate.

[6]The following definitions are based on those given by Huntington (1905).

(*) $$(S,T\neq\varnothing \wedge S,T\subseteq K \wedge \forall x\in K(x\in S \vee x\in T) \wedge \forall x\in S\forall y\in T(x<y)) \rightarrow \exists z\in K(\forall x<z(x\in S) \wedge \forall z<y(y\in T))$$

Although the term "continuous" applies to series (K,<), it can be extended to apply to the set K alone provided that the relation < is obvious in the context (e.g.: for real numbers, "<" denotes the relation less than).[7] When in the chapter on Poincaré I speak of "continuity in the mathematical sense" it is this technical sense of continuity that I mean (the phrase is a rough translation of a phrase in Poincaré).

This notion of continuity is important in discussion of impredicativity because to show that the real numbers are continuous in the above sense requires the use of an impredicative definition. The theorem that shows this is often called the least upper bound theorem (*lub theorem*). A set of real numbers is said to be *upwardly bounded* when it has an *upper bound*, i.e., when there is some real number greater than or equal to every number in the set. Then the lub theorem says that *any upwardly bounded set of real numbers has a least upper bound.* It is fairly straightforward to verify that, if the least upper bound theorem obtains, then the set of real numbers is continuous according to the above definition. Briefly, the least upper bound of an upwardly bounded set S of real numbers will be an element z of K which makes the existentially quantified consequent of (*) true. Speaking very roughly, one might define such a real number z as follows:

$$\text{the } z\in K \text{ such that } \forall x<z(x\in S) \wedge \forall y\in T(z<y)$$

The definiens here, however, contains bound variables "x" and "y" ranging over all real numbers, so this definition is impredicative. It follows from the work of Feferman (1964 98, 126-7) that it will not always be possible to define the least upper bound of a set of real numbers without

[7]Space can be said to be continuous in a related sense, but there is some additional complexity here since space has several dimensions. Full detail is perhaps not required here but, roughly, n-dimensional space can be regarded as the n-th Cartesian product of K. Then, we may say space is *continuous* if K is continuous.

making a similar use of quantification. In other words, the least upper bound theorem is impredicative, for it affirms the existence of real numbers definable (in the language of set theory) only by means of impredicative definitions.

Chapter 1: Early Russell

1.0 Introduction

In 1898, Bertrand Russell underwent a wholesale change in outlook. This change was so broad Russell called it a "revolution" in his thought, and later stated that it was the only revolution in thought he had experienced (1959 11, 54-64). In this revolution, Russell changed from a follower of Kant and Hegel to an analytic philosopher. It is an interesting historical question just what Russell abandoned at this time and just what he came to believe in its place.[8] In the present chapter I discuss Russell's early work in the foundations of geometry in an effort to highlight certain modal notions and principles which Russell adopted as an idealist, but which he rejected when he came to endorse analytic philosophy. I do not mean to claim that these are the only notions or principles he abandoned in 1898, but they are among the most important. The notions I highlight will be fundamental in Chapter 3 to my account of impredicativity.

I begin the present chapter with a rare example of Russell's dialectical skill. My intention in discussing this dialectical argument is to outline three broad philosophical frameworks in terms of which Russell, at various times in his career, attempted to give an account of mathematical continuity. I go on in the rest of the present chapter to discuss one of these philosophical frameworks in more detail. In Sections 1.2 and 1.3, I sketch the general outlines of Russell's early work in foundations of geometry. This may be expeditiously done by documenting Russell's indebtedness to Kant. In Sections 1.4, I discuss the geometrical principle Russell called the "axiom of relativity," making particular

[8]Recent books relating to the subject include Hylton (1990) and Griffin (1991).

reference to the modal notions implicit in Russell's understanding of this "axiom." These modal notions raise a certain problem for Russell's view, which is presented in Section 1.5. Russell's changing solution to the problem is the subject of Section 1.6. Finally, in Section 1.7, I return to the dialectical argument with which I began and try to show in terms of it, and in terms of the preceding discussion, what it meant to Russell to abandon idealism. The particular modal notions and principles he abandoned in 1898 will be taken up again in Chapter 3, in order to show their importance for the subject of impredicativity.

1.1 The Philosophy of the Continuum.

> The principle of our dialectic appears to lie in making the Whole gradually more explicit. Our separate particles turn out, first to be related to other particles, and then to be necessarily related to *all* other particles, and finally to err in being separate particles at all. With this we pass to the plenum. (1897x 23)

The unlikely author of this passage is Bertrand Russell. He wrote it in 1897, when the dominant influences on his thinking were idealists such as Hegel and Bradley. It is in fact one of the few surviving passages in which some indication is given of how Russell understood and applied dialectical reasoning during his idealist period (1894-1898). More than this, however, the passage suggests three broad philosophical frameworks in terms of which Russell, at various stages of his career, conceived of continuity. The notion of continuity is important to this dissertation because the theory of continuity is impredicative. In my view, an account of impredicativity can learn much from Russell's various efforts to supply a logically adequate account of the continuum. Thus I now offer a brief exegesis of what Russell is asserting in this early passage.

Hegel's influence on the early Russell is noticeable in the latter's choice of a dialectical form of argument; but one should not assume that Russell, even at this time, is entirely faithful to Hegel. In fact, the stages of Russell's dialectic are related to one another in a way quite different from that in which the stages of a hegelian dialectic are related. It is common to think of a hegelian dialectical argument proceeding in three stages as follows. A thesis is affirmed at the initial stage, but its contradictory is affirmed at the second. In the final stage, the two earlier theses are "synthesized" — something from both is retained and something from both is rejected. The result is held to be a more satisfactory explanation than those hitherto available in the procedure of the subject at hand.

Whether a dialectical explanation of this sort is ever required, or even makes sense, is not the issue here. The point is rather that Russell departs here from this customary pattern of dialectical explanation. At the second stage, Russell supplements his initial affirmation ("particles [are] related to other particles") with a *compatible*, not a contradictory, thesis ("particles [are] necessarily related to all other particles"). Only at the third stage does a contradiction emerge, and then what is contradicted is not one of the earlier theses *per se*, but a claim implied by (Russell might have said "presupposed by") the natural interpretations of both earlier theses. It is denied, in the end, that there are after all "separate" or distinct "particles". (The significance of the term "particle" is discussed later.) Russell expresses this by saying that, at the third stage, the "particles" *err* in being separate. Strictly speaking, of course, the "particles" don't err in the sense of holding false beliefs. But nor does Russell mean that *he* made a simple mistake earlier in dialectic. Rather, as we shall see, the "error" that was committed was a necessary one, and would have to be made again.

For Russell, no doubt, the differences between the two kinds of dialectic were less important than the similarities. A further similarity lies in the conviction that the final stages of both provide the most satisfactory explanation of the subject, free from whatever errors must occur at the earlier stages. But what exactly is the subject here? What is "the Whole" Russell is trying to make gradually more explicit? The context from which the quoted passage is taken leaves no doubt about the answer: "the Whole" is the material or spatio-temporal world. It is this which, at the final stage of Russell's dialectic, is seen to be a "plenum". Now to be a "plenum", in Russell's early terminology, is to be a "material continuum", *i.e.*, to be both continuous and made of matter. Thus it was in order to account for the *continuity* of the material or spatio-temporal world that Russell, in 1897, appealed to the dialectic.

Before I proceed to examine the three stages of this dialectical account of the continuity of the material world, something should be said about Russell's use of the word "particle". The particles Russell has in mind here are mass points, which he thinks of as actual material entities. For my purposes, however, the fact that the particles are material is unimportant. What is important is what is said about the particles in the course of the dialectic. At the beginning, they are taken to be "separate" or distinct, but in the end they turn out not to be distinct. This contradiction is not only what makes Russell's explanation "dialectical" (as was mentioned above), it is also precisely similar to a contradiction occurring in Russell's earlier account of continuity in pure geometry. In (1897 189), Russell had argued that geometrical points ("particles" of a non-material kind) had to be thought of for certain purposes as distinct, but for others as identical. Thus the fact that the "particles" Russell is speaking of in (1897x) are material plays no role in his account of continuity *per se*, but is relevant only insofar as he happens to be discussing the "plenum" or "material continuum". As regards the account of continuity itself, the crucial fact is quite simply that the "particles" are *logical subjects*. A logical subject, in the terminology Russell employs at the time, is that of which something true can be said.[9] Russell believed that, in order to explain the continuity of geometrical space or the spatio-temporal world, one had in the end to treat it as a single logical subject *not composed of distinct logical subjects*. To get this explanation off the ground, however, he also thought something true had to be said about distinct logical subjects which collectively *composed* the continuum in question. The contradiction, then, as to whether or not the continuum was composed of distinct logical subjects was held to be unavoidable, and indeed to demonstrate the necessity of a dialectical account of continuity.

I come now to the individual stages of Russell's dialectical explanation of the continuity of the spatio-temporal world. A closer examination of these will lead to a sketch of three general philosophical standpoints in terms of which Russell, at various times, attempted to give an account of continuity. In the first stage of the dialectic, "separate particles" turn out to be related to other "particles". From what was said above, it follows that the crucial claim here is that distinct logical subjects are related to other logical subjects. Of course, if being distinct is one way of being related, this first stage in the dialectic is trivial. But Russell denies the hypothesis; difference, according to him, is not a relation but, as he says, "presupposed by relations" (cf. 1897 198). Evidently, Russell is thinking of relations on the model of those denoted by "x lies some distance from y" and "x lies to the left of y". If these are seen to be instantiated (as spatial experience in fact suggests) then there must be distinct logical subjects: something true must be said about more than one "particle". The claim at this first stage of the dialectic, then, or at least part of it, might be formulated as follows:

[9]See (1898x 167-8) By this definition, anything at all is a logical subject. Although Russell does want this kind of generality here, we will find him elsewhere imposing further conditions on logical subjects. His equivocation is discussed later in Chapter 1.

there is some irreflexive relation, and it is instantiated. This apparently obvious move already puts Russell into *prima facie* opposition with Bradley, who held that no relation was "ultimately real" and that there was only one logical subject.

At the second stage of the dialectic, "particles" turn out to be "necessarily related to all other particles". Two additional claims are in fact being made here. First, *all* logical subjects turn out to be related to *all* others, just as every geometrical point is some distance from every other geometrical point, and every mass point exerts some gravitational influence upon every other. The second claim made here is that all particles are "necessarily related" to all others. This second claim is itself ambiguous in several respects.[10] Let us assume first of all that some particular irreflexive two-place relation R is in question, and that R is not difference. Then the second claim might be: it is necessarily true that, for any distinct logical subjects x and y, xRy. Or it might be the stronger thesis that, of any two distinct logical subjects x and y, it is necessarily true that xRy. This stronger claim is sometimes expressed by saying that it is essential to x and y that xRy. It is not clear from the text alone which of these two claims Russell is making here, but circumstantial evidence suggests he intended the stronger second reading. Years later Russell would take the second reading, and the notion of essentiality that it may be taken to explicate, as characteristic of views he held between 1894 and 1898, and characteristic in fact of a great many philosophers, from Leibniz to Bradley. If this is so, and I shall present more evidence to this effect below, then the second stage of Russell's 1897 explanation of continuity may be said to consist in the claim that the logical subjects involved are all necessarily related (in the strong

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[10]Russell might mean either "x∀y∀R (x is necessarily related by R to y)" or "∀R∀x∀y (x is necessarily related by R to y)." At present I am more concerned with the ambiguity inside the parenthesis. I assume here that the former is intended, but the entire matter is discussed again at the end of this chapter.

sense) by the relation R to all the others.

At the third and final stage of the dialectic, the particles "err in being separate particles at all". A good deal has already been said about this stage. Perhaps all that remains is to indicate the drastic consequences. If the material world is a "plenum", as Russell held, then it consists of a single incomposite logical subject. The rift with Bradley which appeared at the outset of the dialectic is now repaired, and Russell thought of himself as accepting here something like Bradlian *monism*, which consists in the view that there is but one logical subject. The similarity with Bradley goes further, since Bradley and Russell would agree that the single logical subject is both continuous and incomposite. Except perhaps the "logical subject" terminology, this a position endorsed by Parmenides, and it is worth noting that the point of Russell's dialectic is similar to the paradox of continuity sometimes attributed to Parmenides' most renowned disciple, Zeno of Elea.[11] If something is continuous, it is incomposite. Now monism, if filled out in further detail, may not be as bizarre as it initially appears, but left as it is stated here it is certainly unsatisfactory. Quine once complained that Russell's ontology of a later date was "intolerably indiscriminate". But however zealously one sides with Quine in preferring desert landscapes in ontology, one understandably hesitates to embrace the desert consisting of a single grain of sand.

The stages of Russell's 1897 dialectic have not been drawn out in order to show precisely what Russell's early views were. This is discussed in more detail below. The point rather is that the different stages of the dialectic correspond to general philosophical standpoints Russell held at

[11]See Grünbaum (1952). Roughly, the paradox states that spatial continua (of finite length), which seem ultimately to be composed of points, cannot be, since points have no finite size and thus no number of them can combine to form a whole of finite size. This and similar paradoxes involving motion also attributed to Zeno were originally meant to demonstrate Parmenides' monism.

different stages of his career, and to the accounts of continuity he gave in terms of these standpoints. This has already been done sufficiently for the final stage. Russell understood this stage as committing him to a sort of materialistic *monism*, as he could not conceive of attributing continuity to anything except a single logical subject. I turn now to the other stages of Russell's 1897 dialectic.

In his mature period, of course, Russell dispensed altogether with the dialectic. He refused, to put it another way, to go beyond its first stage, or at least very far beyond it. At the first stage of the dialectic, distinct logical subjects are said to be related to other logical subjects: some irreflexive relation is instantiated. This, certainly, is in accord with Russell's mature metaphysics, where distinct logical subjects (he would eventually call them "logical atoms") do stand in relations, and where these relations are in certain cases irreflexive. More importantly, however, the mature Russell would maintain *part* of the second stage of the 1897 dialectic, namely the view that every logical subject is related to every other. (The relation of difference may even be excluded from consideration here.) But Russell would go no further along the dialectic: "logical atoms" were never "necessarily related" (in the strong sense mentioned above) to one another; for if one logical atom were ever to cease to stand in a relation to another, it would not thereby become something else or cease to be at all. Indeed, after 1900, Russell denied that this strong sense of "necessarily related" was even coherent. Certainly his theory of continuity made no explicit appeal to it. Instead, Russell relied on the theories of continuity developed by Cantor and Dedekind, which consist in the specification of what must be true of a relation if it is continuously to order the elements of its domain. These conditions on the ordering relation, according to Russell, do not require one to suppose there to be only one logical subject. More important, Russell denied that any single logical subject is ever itself said to be continuous. Russell's logical *atomism* is characterized by the claims that there are many logical subjects; that there are relations in which these logical subjects stand; and that none of these relations hold necessarily (in the strong sense given above). His view of continuity reflects all of these: continuity consists in distinct logical subjects related non-essentially in a certain way.

Now the second stage of Russell's 1897 dialectic similarly corresponds to a philosophical framework in terms of which Russell tried to give an account of continuity. In the second stage of the dialectic every particle is "necessarily related" to every other in the strong sense given above. The corresponding framework consists in the view that there are many logical subjects; that these stand in various relations to one another; and that all these relations are necessary in the strong sense above. Except for the brief period in 1897 when Russell wrote the passage quoted above, the early Russell worked entirely within this framework. I shall call this framework *monadism* despite the fact that Russell sometimes used this word somewhat differently. The difference turns mainly on ambiguities of the term "internal relation", and the important question of the relation between the various relevant senses of this term is addressed in more detail below. The conception of the continuum corresponding to monadism in my sense consists in the claim that the logical subjects composing the continuum are essentially related to one another: they are what they are *only in relation to each other*. As we shall see, the relativistic view of space which Russell argued for in (1897) held analogously that points are what they are only in relation to the other points. Between 1896 and 1898 Russell worked on generalizing this view of space to continua in general.

It was while working on the details of this generalization that Russell came to see serious difficulties with monadism, and eventually to abandon altogether the notion of internal relation. Although it is an exaggeration to say that with this move analytic philosophy was born, it is certainly true that Russell's shift gave the fledgling philosophy a new lease on life. Below I will give an account of the difficulties Russell thought monadism faced, and trace this momentous change in Russell's philosophical development. The role of the theory of continuity in this transition will be seen to be quite paramount.

There is, however, a further reason for interest in early Russell's monadism. In my view the corresponding view of the continuum is not quite as mistaken as Russell thought. This can be seen by a careful study of the inadequacies of Russell's final *atomistic* theory. In 1925, Russell admitted that his official "logic" (ramified type theory without the axiom of reducibility) was inadequate to the Dedekind-Cantor account of continuity he endorsed. In short, this inadequacy is due to the fact that the Cantor-Dedekind theory of continuity is impredicative and Russell's "logic" is not. In chapter 3 I argue that the principle upon which Russell based ramified type theory and which banned impredicative definition is in fact a modal principle (despite Russell's claim to have dispensed with modal notions in logic). Impredicative objects, then, (such as those Russell would have needed to accept in order to render his logic adequate to the Dedekind-Cantor theory of continuity) can be understood as violations of this modal principle underlying ramified type theory. I shall argue that this modal understanding of impredicative objects strongly suggests the modal principles characteristic of Russell's earlier monadist metaphysics. This is the central thesis of the present dissertation. But before I come to it in chapter 3, I want now to discuss Russell's monadist philosophy in more detail.

1.2 Russell's Early Foundational Program: Contrasts with Kant.

The major philosophical topics in Bertrand Russell's early professional work come from the foundations of geometry. His 1895 Trinity College dissertation dealt with our knowledge of geometry and with the metaphysics of space; and although his original fellowship-winning work is now lost, part of it probably appeared in (1896y 267-86), and a revised version was published two years later in (1897). From these two sources, as well as from others, such as numerous notes unpublished until (1990x), it is possible to get a fairly clear interpretation of Russell's ideas in the foundations of geometry at the time. I will discuss these ideas as the first part of an effort to show that certain of the metaphysical problems Russell treated in the 1890s are intimately related to the metaphysics of impredicativity. Since Russell at the time was rather Kantian, I will develop Russell's early views in part by contrasting them with Kant's.

In the foundations of geometry, as indeed in all epistemic inquiries, Russell took sensation as his starting point. Space, he says, is "given in sensation" and "immediately experienced" (1897 188; cf. 1896x 48). Thus the knowledge of space in which geometry was thought to consist starts with spatial sensations. Now sensations, in Russell's terminology, like *Empfindungen* in Kant's [A19-20/B34], are "the only mental states whose immediate causes lie in the external world" (1897 ; cf. 1895y 258). But (again as in Kant) the external cause of a sensation cannot alone account for its appearance or nature. Rather "its nature is composite, in part due to the external cause, in part to the nature of the subject affected" (1895y 258; cf. Kant [A20/B34]). The former is the "material element" of the sensation, the latter the "formal" or "*a priori*" element, which was held to be presupposed in actual experience (1897 2). Now *spatial* sensation has its own particular formal element, which Russell called the "form of externality." The "content" of this form is given by describing it, *i.e.*, by articulating it as a set of principles. With these principles Russell hoped to arrive at necessary conditions of the possibility of spatial experience in general (1897 3). Such axioms of geometry as followed from these principles were to be regarded as themselves *a priori* and necessary. The primary purpose of Russell's 1895 dissertation, then, as well as several papers on the same subject from around the same time, was to isolate and identify the necessary conditions of any possible spatial sensation, and to deduce from these the necessity of axioms of geometry.

Already it is evident that Russell's task had a strongly Kantian cast to it; and the similarities do not stop here. As a general principle, however, differences of opinion are often concealed by similarities in terminology, and this applies already to Russell's starting point. While Kant would agree that space is "given in sensation" and "immediately experienced," both these phrases have a rather different significance for Kant than for Russell, who leaned heavily on the tradition of idealist monism represented by Hegel and Bradley. These differences are deep and important, but it will not be possible to do justice to them here. I will return briefly to the notion of sensation, but only after more has been said about Russell's positive views in the foundations of geometry.

Russell's early foundational program in geometry overlapped with Kant's at more points than those just mentioned. It was certainly a major part of Kant's goal to isolate and identify the formal or *a priori* element of spatial sensation [A22/B36], and Kant also expected principles articulating this "reine Form der Sinnlichkeit" to express necessary conditions of every possible spatial experience. Furthermore, it was originally Kant's idea to derive "geometrische Grundsätze" from such principles. His and Russell's common goal was to demonstrate the necessity of geometrical truths on this basis. There is reason to think, however, that this type of argument cannot succeed. Moore (1899) severely criticized Russell (and indirectly Kant) on a crucial inference required by the purported demonstration, namely from the so-called *a priori* (that presupposed by *actual* experience) to necessary conditions of *any possible* experience. The problem of moving from the latter to necessity *tout court* is also serious. These are traditional errors which we will find Poincaré committing as well. My use of Russell's early work does not depend on his success on either of these points.

Another reason Kant's efforts to demonstrate the necessity of the axioms of geometry fell short is that he never actually provided explicit deductions of this kind; nor did he articulate especially clearly the principles of the pure form of sensibility. Russell, on the other hand, tried to improve on Kant in both these respects. He gave a list of alleged geometrical axioms and explicit arguments to the effect that some of these, at least, followed from a refined description of the form of externality. Although Russell's work also lacks formal precision, his attempt is certainly to improve upon Kant.

At several points, however, Russell ventured into open disagreement with the "creator of modern epistemology" (1897 1). Only some of these are relevant to the subject of impredicativity. Whereas Kant maintained that principles of the pure form of sensibility were synthetic, Russell, citing the authority of Bradley and Bosanquet (1897 57), tells us that "modern logic" has rejected any exclusive distinction between synthetic and analytic propositions:

> although we cannot retain the term synthetic, we can retain the term *a priori*, for those assumptions, or those postulates, from which alone the possibility of experience follows. (1897 59-60)

Russell's preference for the notion of the *a priori* over that of the synthetic corresponds to the expanded role he accorded, even at this early date, to logic. At one point Russell calls the whole project of demonstrating *à la* Kant the necessity of geometrical axioms "purely logical" (1896y 291), and elsewhere a similar point is made:

> Of course Kant is right in maintaining that *something* must be presupposed to make experience possible [... but] *logic* alone must be presupposed. (1895y 261)

The purview of logic in the early Russell is stunningly broad, just as it was later in Russell's analytic period. Thus at (1895x 259) Russell casually remarks that *all* arithmetical axioms are attributable "to purely logical motives." It would be wrong to say Russell was already a logicist in 1895, but the views he held then must have made it easier for him later to accept the idea that arithmetic was reducible to logic. Both the expanded view of logic and the reluctance to accord the Kantian notion of synthetic *a priori* much significance continue in Russell past his 1898 "revolution;" and both will cause considerable confusion in his debate over logicism with Poincaré, during which the concept of impredicativity is first introduced. Poincaré puts a predicativity constraint on logic and accepts certain impredicative principles only if they are viewed as deriving from synthetic *a priori* intuition. The implications of this are dealt with later, but it is already obvious that the extent of logic and the role of the synthetic *a priori* in mathematics are both bound up with the problems of impredicativity. For the present, however, we need say little about the synthetic *a priori*, except to point out that it is a notion Russell makes no use of, even at this early stage.

By far the most important area of open disagreement between the early Russell and Kant concerns the positive content of the *a priori* form of sensibility. Differences of this kind can be brought out in two ways: directly, via consideration of their alternative descriptions of the *a priori* form of sensibility; or indirectly, via consideration of the geometrical axioms held to be deducible from this description. I have already mentioned that Russell refined the description of the form of sensibility beyond what one finds in Kant. This refinement stems in part from differences as to the nature of sensation, and in part from differences as to the nature of immediacy. In both, but especially in the latter, Russell exhibits the influence of Hegel and Bradley. Again, however, it is best to discuss Russell's refinements later.

The indirect way of exhibiting differences as to the content of the *a priori* form of sensibility yields quicker results. Here we compare the geometrical axioms thought to be deducible from the form. Even here, however, difficulties arise, for Kant was not entirely explicit on this point. Still, in Russell's day it was widely assumed that Kant held Euclid's parallel postulate to be so deducible. If this were correct (and Kant knew nothing of non-Euclidean geometries), then Kant would have maintained that we know Euclidean geometry *a priori*. Russell, at any rate, interpreted Kant in this way, and then took issue. It is not *a priori* determinable whether actual space is Euclidean or not, Russell argued, because no information regarding the curvature of space was deducible from the form of sensibility beyond the fact that this curvature was constant. All the axioms of geometry common to both Euclidean and non-Euclidean geometry were deducible from the form of sensibility, Russell thought, but the actual value of the constant of space-curvature, and therefore the parallel postulate, could at best be known empirically. Russell's position here is perhaps the best known of his early views, but it is not itself my central concern. One consequence of his view, however, is important to the issue of impredicativity, since impredicative mathematics has often seemed incompatible with "subjectivist" accounts of mathematical existence. I turn to this consequence now.

The consequence in question is an effective denial of what Kant calls the "transcendental idealism of space." Russell's position makes it necessary that he uphold the objectivity of space. He writes:

> Kant contended that extension is subjective in a way in which the secondary qualities are not so: that is, there is no

> counterpart to it in the object. (1895y 260)

In this, he seems to be correct. Kant denied

> daß der Raum eine Form der Dinge sei, die ihnen etwa an sich selbst eigen wäre. [A30/B45]

This view, that sensed spatial properties of objects have to do exclusively with our sensation, and nothing at all to do with the objects in themselves, is what Kant calls the "transcendental idealism" of space. Admittedly, Russell does not set out to disprove this view, and seeks rather to leave aside all questions concerning the "subjectivity" of space (1897 3-4). But his final position leaves him little room to maneuver. In point of fact, Russell is careful to allow himself just enough room for the objectivity of space:

> necessity for experience [*i.e.*, aprioricity] can only arise from the nature of the mind which experiences; but it does not follow that the necessary conditions could be fulfilled, unless the objective world has certain properties.... Owing to the constitution of the *mind*, experiences will be impossible unless the *world* accepts certain adjectives. (1897 179).

Now my claim is that, if Russell is correct about exactly what is *a priori*, or necessary for experience, then the property of *being spatial* is among those the world has to accept. In particular, since Russell holds that the actual curvature of space is empirically discoverable, he must obviously hold that there is something there to discover in the first place. The curvature of space must have "a counterpart to it in the object." But if the curvature of space has a counterpart in the objects themselves, how can space itself go without? The metric of space cannot be an empirically discoverable property of space unless space itself is objectively present. Russell, then, although importantly Kantian in the other respects mentioned, denies Kant's transcendental idealism of space.

This brings us back to one of the first differences I mentioned between Kant and the early Russell, namely the notion of sensation. For Kant, sensations are intuitions; in fact, they are the only empirical intuitions humans have. Thus Kant speaks interchangeably of the form of (outer) intuition and the form of sensibility. Russell, on the other hand (most of the time at any rate), distinguishes between sensation and intuition. He takes it to be a *substantive* question whether space is given as a sensation or an intuition (1897 55, 180), and the only remarkable difference, as far as I can tell, is precisely that, in veridical sensation, a counterpart in the object is required; whereas, in veridical intuition, this possibility is excluded (1897 180, 1896y 291-2). Russell's terminology "form of externality" is designed to allow him generality across these two possibilities (even if in the end his view of the *details* of the form of externality force him to accept objective space). For Kant, on the other hand, there simply is no room in the first place for such added generality, since sensation is already a special case of intuition. To say that space is the form of sensation is, for Kant, unlike for Russell, to affirm the transcendental idealism of space. By choosing the word "externality" Russell sought to gain neutrality.

1.3 Difficulties with Difference.

Notwithstanding this, Russell's use of the term "externality" *is* suggested by one part of Kant 's "metaphysische Erörterung" of the concept of space. It is in fact the only part Russell accepted (1897 55-6, 60-1), and it is also the basis of his refined "direct" description of the formal element of spatial sensation.

> ... damit gewiße Empfindungen auf etwas außer mich bezogen werden ..., imgleichen damit ich sie als außer und nebeneinander, mithin nicht bloß verschieden sondern in verschiedenen Orten vorstellen könne, dazu muß die Vorstellung des Raumes schon zu Grund liegen. [B38, cf. A23]

Sensation presents things in different places and external to one another. This "Außereinandersein" is the motivating connotation of Russell's "form of externality" terminology. But despite Kant's emphasis on difference of *place*, Russell expects his own "metaphysical deduction" to yield insight into *substantival* difference. Externality, he says, "must mean, in this argument [*i.e.*, in the metaphysical deduction], the fact of Otherness, the fact of being different from some other thing."[12] "Real diversity" is intended here, he says, which is "an Otherness of substance, rather than of attribute" (1897 62). The principle articulating the form of externality, then, at least as Russell understands this, must inform us as to what it is for one logical subject to be distinct from another.

Interestingly, Russell hesitates to identify the principle describing his form of externality with the traditional *principium individuationis*, but he is happy to call it a "principle of differentiation" (1897 136). Unfortunately, besides an unhelpful reference to Bradley (1883 63), Russell does not explain his terms. Still, it is clear that both principles purport to explain substantival difference; or, to put it in other words, which I will henceforth take to be equivalent, they *seek to explain what makes what is, what it is*. The difference between them, I think, lies in how they seek to do this. The former, which Russell shies away from, puts emphasis on the *individuality* of the logical subject: it looks to the qualities (possibly essential) that an individual substance displays to find what makes it the particular logical subject that it is. To answer Aristotle's question, "What is it?", this approach would provide a list of 1-place properties which uniquely characterize the substance, and possibly indicate its essence. The principle of differentiation, by contrast, emphasizes the *differences* between the logical subject in question and any other: it looks instead to the relations (possibly essential) that an individual substance displays to find what makes it the particular logical subject that it is. The answer to Aristotle's question would importantly include some relations, which

[12]At this stage, Russell uses the term "thing" interchangeably with "substance" and "logical subject" (see 1897x 21, 1896x 14; but cf. 1898x 168). It is difficult, perhaps impossible, to say precisely what Russell intended, but the only entities which are not logical subjects are qualities (which, following Bradley, Russell sometimes calls "adjectives") and relations. This traditional, intuitive distinction suffices for my purposes. I use the term "(n-place) property" to cover both qualities (n=1) and relations (n>1).

may form part of the essence of the substance in question. It is this, I expect, which accounts for Russell's preference, since, as we shall see, he thinks relations are necessary to explain the difference of substance presented in sensation. His explanation of what makes what is, what it is, *contains*, and does not merely imply, information regarding what makes what is not another thing.

These principles suggest different ways, not only of answering Aristotle's question, but also of satisfying the Identity of Indiscernibles. As we shall see, Russell makes silent appeal to the Identity of Indiscernibles in his version of the metaphysical deduction. Ultimately, Russell failed in his effort to articulate a principle of differentiation, and to provide an explanation of substantial difference. Within a few years he would argue that his effort was destined to fail, and that, that ultimately his early view "collapsed into monism," and was incompatible with there being more than one thing. The arguments Russell would later are already suggested by his 1897 dialectic. But it is best to postpone a discussion of this matter until later (cf. Sec. 1.6).

To return. One major disagreement Russell had with Kant regarding the positive content of the *a priori* form of sensibility was that, whereas Kant expected it to tell us about difference of place, Russell expected it to tell us about difference in substance. This is obviously connected with the fact that Russell conceives of space realistically, as a form of sensation (in his sense), whereas Kant conceives of space idealistically, as a form of intuition. It is hard to see, however, how even Russell's stronger expectation will get him what he wants, namely a principle from which to deduce all the geometrical axioms common to both Euclidean and non-Euclidean systems. But in practice, Russell supplements the "fact of Otherness" with another principle which ensures the *homogeneity* of the form of externality. Homogeneity consists in the fact that "one position [in the form] is exactly like another" (1896x 11, n. 1), or somewhat more precisely, "positions do not differ from one another in any qualitative way" (1896y 277). This supplementation is regarded as a simple consequence of the *formal* nature of the "fact of Otherness" in question. As Russell puts it:

> when we abstract a form of externality from all material content, and study it in isolation ..., a position can have no intrinsic quality (1897 136-7).

It is difficult, of course, to attach any clear meaning to the traditional philosophical technique of abstraction, but for historical purposes this is not necessary. Suffice it to say that Russell's view is that the *a priori* element of spatial sensation provides us with an awareness of distinct but qualitatively indistinguishable things, and that this is what Russell means when he says the principle of differentiation he is seeking is concerned with "*bare* diversity" (1897 136).

The homogeneity of the form of externality is a linch pin for Russell because he took the mathematical work of Georg F. B. Riemann to show that homogeneity was an essential property of space. Thus if Russell could show that homogeneity is *a priori*, or a necessary condition of spatial experience, he would be well on his way to deriving fundamental properties of space from the form of externality. Using the term "manifold" to translate Riemann's *Mannigfaltiges* (which of course occurs in Kant as well), and distinguishing examples of *non-spatial* manifolds also found in Riemann, Russell writes:

> This absence of qualitative difference [among the elements of a manifold] is the distinguishing mark of space as opposed to other manifolds, such as the colour and tone-systems. (1896y 277; for "manifold" see 1987 14 fn 2)

Essentially, Russell takes Riemann to have shown that the distinctive property of space is constancy of curvature; and this, Russell says, implies the homogeneity of space (1896y 277). The details of both Riemann's and Russell's arguments are not important here, except perhaps to say that the constancy of curvature was considered necessary to the possibility of measurement, which in turn seemed to require "motion" of figures through space.[13] Riemann proved that, if figures were to retain a constant magnitude through "motion", the curvature of space had to be constant. Russell writes:

> since magnitudes are to be independent of place, ... space must, within the limits of observation, have a constant measure of curvature, or must, in other words, be homogenous in all its parts. (1897 22)

The homogeneity of the differences presented in sense is the primary feature of Russell's form of externality. But Riemann's work showed homogeneity to be the essential or defining feature of space. Russell sought to exploit this happy coincidence in his metaphysical deduction.

The homogeneity of space, Russell writes, is "our great resource" (1896y 279). From it he will claim to deduce all the axioms of projective geometry, which are those common to both euclidean and non-euclidean geometry. I will concentrate mostly on the first axiom, which affirms the *relativity* of space, but the "philosophically interdependent" (1897 132) axiom of the *continuity* of space became increasingly important for Russell as time went on. Throughout my concern will be with the metaphysical difficulties these two properties, relativity and continuity, raise for Russell, difficulties he attempted to face in the succeeding years. I will not, however, be concerned with the errors in Russell's sketchy "metaphysical deductions" of the axioms from the assumption that "positions do not differ from one another in any way." His deductions are so bad criticism is virtually redundant. My point, rather, will be historical. The difficulties these properties raise are in a new form raised again by the problem of impredicativity, which remains closely related to continuity. Russell considered himself to have done away with these problems once and for all when he abandoned idealism in 1898 and

[13]The "abstract motion" in question was thought to be required for the superposition of one geometrical figure upon another, a test of congruence dating from Euclid at least. Moore (1899a) condemns the idea.

accepted the metaphysical position I called *atomism* above. The paradoxes forced Russell to refine his atomism into type theory. This metaphysical refinement allowed him to exclude impredicativity on logical grounds alone, but it also prevented him from giving an account of classical continuity. Today, the acceptance of classical or impredicative continuity requires a return to a clarified version of the idealist notion of relativity which bothered early Russell.

1.4 Relativity

Russell states the first axiom of projective geometry, which affirms the relativity of space, as follows:

> We can distinguish different parts of space, but all parts are qualitatively similar, and are distinguished only by the immediate fact that they lie outside one another. (1897 132)

This statement of the so-called "axiom of relativity," Russell says, "is not intended to have any exclusive precision," and indeed three separate claims are made. The second claim made is in fact just the claim that space is homogenous, which is apparently a serious slip of precision, since this axiom was to be deduced from the homogeneity of space. This lack of precision shows up elsewhere, for in unpublished notes Russell writes: "Homogeneity is synonymous with complete relativity" (1896x 14). More often, however, Russell speaks of the two properties as equivalent, and I will follow this practice where possible. This leaves the first and the third claims in the above statement; but of these the third, since it involves no reference to persons, is evidently the more fundamental. The relativity of space, then, consists in the fact that parts of space "are distinguished only by the immediate fact that they lie outside one another."

There is a subtle but crucial difference between the homogeneity and the relativity of space. Since space is homogenous, all positions in it are qualitatively indistinguishable. But positions are external to one another, and so they stand in some relation. To affirm the relativity of space is to affirm that these relations, and not any qualities of the positions themselves, are what *make* the positions what they are. This step takes Russell toward the "principle of differentiation" he is seeking, for it begins, at least, to explain what makes what is, what it is and not another thing. Strictly, of course, Russell would have here at most a principle of differentiation for positions, but since Russell's goals were much broader, I will continue to discuss a general form of the principle concerning logical subjects, and ignore the very difficult problem of how Russell got to the general version on his slim basis.

Now to infer the relativity of space from its homogeneity one needs to make special assumptions. It is difficult to put these assumptions in a form to which one can be certain Russell would have assented; for, despite their appearing indispensable, Russell does not explicitly state them. But I suppose the following would have seemed to him acceptably precise. First, one must assume that distinct positions must be distinguishable. Putting this in the general form Russell wants (and assuming a broad scope reading of the implicit modal claim), we have:

> It is necessary that distinct logical subjects be distinguishable.

This assumption, evidently, is a form of Leibniz' Identity of Indiscernibles, which may itself be regarded as a consequence of Leibniz' Law of Sufficient Reason. (If everything true is true for some reason, then for substantival difference too there must be a reason.) But the principle I have stated is useless, unless we know the conditions under which two things are discernible or distinguishable. To specify these conditions, Russell may have assumed that positions are distinct only by virtue of the qualities the possess or relations they enter into. More generally:

> Logical subjects are distinct only by virtue of the qualities

or relations they enter into.

As was mentioned above, Russell held to the traditional metaphysical view that there are, in the world, only substances, qualities and relations. Given this, if Russell accepted the Law of Sufficient Reason, he probably also made both the above special assumptions. I just noted why the Law of Sufficient Reason might be taken to motivate the first assumption. Assertions of substantival difference, according to that law, can not be true on their own, but must be true for a reason. But in what would a reason consist if not a fact (perhaps a necessary fact), and facts would seem to be nothing other than logical subjects entering into qualities or relations? Thus the Law of Sufficient Reason provides a motivation for the second assumption as well. Now whether this was Russell's reasoning is hard to determine, but he does appeal at times to the Law of Sufficient Reason (1897 185, 1896x 39). Furthermore, Russell later took both the Law of Sufficient Reason and the Identity of Indiscernibles as views characteristic of "monistic idealism," which he says he subscribed to during this early time (1900, 1907). In any case, we must recall that Russell is seeking a *reason* for the substantial difference given in sensation. This shows he thought *something* accounted for logical subjects being what they are and not other things. Such an account is precisely what the two principles from Leibniz would seem to provide.

I mention these debts to Leibniz (which are ultimately debts to Spinoza) in order to give some indication of Russell's probable train of thought in his version of the metaphysical deduction. But another purpose is served as well. I shall need to assume a weak version of the Identity of Indiscernibles later, when I argue in the third chapter that the impredicativity of an object consists in its bearing certain relations essentially. The issue of essential relations is already relevant here too, for it is easy to see that Russell, in fact, took the relations differentiating positions to be *essential* to those positions. Russell writes:

> The whole essence of one part of space is to be external to another part... (1897x 74).

Apparently, then, if one position or part of space exists in a possible world, then so do others:[14]

> From the absence of qualitative differences among positions, it follows logically that positions exist only by virtue of other positions (1896y 277).

Or again:

> all position is relative; that is, a position exists only by virtue of relations (1896y 276).

These statements are not as precise as one might like. "*Relation*," as Russell says, "is an ambiguous and dangerous word" (1897 193). It is not clear, for instance, whether Russell means that, given one position, all others must exist, or only some others. If he means the latter, exactly how many? And are these specific other positions or may they be arbitrary? My guess is that Russell did not really address these questions until 1898. At that time he came to believe that in fact all other positions would have to exist, and this situation was unacceptable to him. His response was to adopt atomism in the sense described above. But this is a subject I discuss later. For the present it suffices to note that Russell took the differentiating relations of positions to be, in some significant sense, essential to those positions.

1.5 Essential Relativity and the Notion of Logical Subject

This fact, that Russell took the differentiating relations of positions to be essential to those positions, leads to a very general

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[14]Compare Kant: "erstlich kann man sich nur einen eigenen Raum vorstellen, und wenn man von vielen Räumen redet, so versteht man darunter nur Teile eines und desselben alleinegen Raumes.... Er ist wesentlich einig, das Mannigfaltige in ihm ... beruht lediglich auf Einschränkungen." [A25/B39]. Russell may have felt his essentiality claim was needed to obtain his goal of demonstrating the *necessity* of geometrical axioms, and also justified by the quite reasonable idea that things are not merely contingently themselves, *i.e.*, that "x is distinct from y" is always necessary.

observation about Russell's notion of a logical subject. The general observation is that Russell places conflicting demands upon this notion. It was noted in the introduction that a logical subject is that about which something true can be said. By this definition, anything at all is a logical subject. But despite this, the fact that a position can be differentiated from others only by means of relations essential to that position led Russell to the rather startling conclusion that positions are not logical subjects. "Position," he says,

> is a term in a relation, not a thing *per se*; it cannot, therefore, ... exist by itself, apart from the other terms of the relation (1897 86).

From this passage, it is clear that Russell required that a thing or substance be able to "exist by itself" or *per se*. The idea that some manner of independent existence is required by a substance has roots going back at least to Aristotle; so it is not entirely peculiar to find the idea in Russell. What is peculiar is that Russell equates substances in this sense with logical subjects in the quite general sense given above. "Matter," he says for example elsewhere, "is not a mere relation or adjective, but a thing, substance, or logical subject" (1897x 21; cf. 1896x 14). Similarly, an arbitrary spatial position is a logical subject in the general sense, since geometrical axioms will in general have something true to say about it; but since what it turns out these axioms must say, according to Russell, is that positions depend for their existence on other things, then positions are not logical subjects in a new sense. Obviously if one insists on imposing conflicting conditions upon a notion, contradictions will follow. In fact, to state such contradictions as they apply to notions in the foundations of geometry was one of Russell's main goals in (1897). The contradictions he formulated, he thought, must inevitably arise in the foundations (cf. 1897 189). But the inevitability Russell felt did not arise merely from wanton equivocation. Rather, Russell thought the equivocation itself was necessary. I am not quite able to see why Russell thought this, but that he did so can hardly be in dispute.

But my purpose in this Section is not simply to indicate a fallacy in Russell, nor to show how the fallacy may have been motivated by other beliefs he had. I am trying neither to refute Russell's early views nor to show that they were plausible on their own terms. My point here is instead to observe that Russell's equivocation, invalid or not, raises a difficulty for Russell of a quite another sort. The difficulty is to maintain his particular conception of the form of externality in the face of his claim that the differentiating relations of positions are essential to those positions. Let me elaborate a little further on this difficulty, running the notions together just as Russell would have.

A position, according to Russell, cannot be a logical subject since, being essentially related to other positions, it depends on them for its existence. Position must, therefore, be a property of some underlying substance. The obvious candidate is the point, but the position of a point is its *only* differentiating feature: it is what makes a point the particular point that it is. Now position is relative, so a given point cannot be what it is, without there being other points bearing other positions whose existence is implied by the existence of the position of the given point. The essential property of a point is therefore a relational property, so points too cannot be substances, or logical subjects. This is what Russell means by saying that, "metaphysically, space has *no* elements" (1897 68).

For Russell, the only other candidate for the logical subject bearing positions, considered as properties, is space itself. But just this causes difficulty for his foundational program, since it challenges his notion of the form of externality. It is important to see, however, that the idea that space is the logical subject, that space is itself a substance having positions as its properties, fits together very well with several aspects of Russell's views. First, the idea that only one logical subject is needed to account for geometrical knowledge must have been for Russell a pleasant reminder of Bradley's monism, which affirmed the existence of only one substance. But beyond such pleasant associations, various considerations seemed to force him to treat space as a substance. Space, recall, is "given in sensation" and "immediately experienced." But "whatever is immediately presented has a This, and may therefore be regarded, to some extent, as possessed of thinghood" (1896x 57). So space can be neither relational nor "adjectival," but a thing or logical subject. Further, geometry seemed to be *about* space. If so, then the correct logical form of geometrical axioms would make space their logical subject. Axioms really ought to be given "in the most desirable form, namely as adjectives [affirmed] of the conception of space" (1897 15). Both these considerations gave Russell cause to regard space as a substance.

But by far the most compelling reasons for doing so came from within geometry itself, from the second axiom of projective geometry. This axiom asserts that "space is continuous and infinitely divisible" (1897 132). As was common at the time, Russell tended to regard these two properties as definitionally equivalent. The substantiality of space followed easily from its divisibility, via the apparent truism that "no mere adjective or relation can be divided" (1897x 19).[15] But its divisibility also *seemed* to imply that space was "complex," the natural interpretation of which was that it was composed of distinct logical subjects acting as parts. This apparent implication, however, was incorrect, since the division could be carried on *ad infinitum*. For Russell, every substance or "thing is either

[15]It is interesting that this is a "truism" Russell would come to deny. In (1925), where are classes are dispensed with and only countable many individuals assumed to exist, the continuum is divisible but nevertheless "degraded to an adjective" (1896x 14), *i.e.*, to a propositional function.

simple, or built up of simple things" (1897x 19). Divisibility implied space was a substance, but its infinite divisibility implied it was not built up of simple substances. This was in accord with the essential relativity of position, which implied that the parts of space were essentially related to one another and thus not logical subjects. Thus space was a simple substance, and if it could be said to have parts at all, these could not be construed as independent logical subjects.

The substantial view of space was thus highly motivated by various aspects of Russell's views, but it caused serious difficulties for Russell. The difficulties threatened his refined account of the form of externality. For, if geometrical axioms are really about some one single substance, what can we possibly learn from spatial sensation about substantival difference? What is to become of Russell's original claim, which arose from his interpretation of Kant's metaphysical deduction [B38/A23], that spatial sensation presents us with the very form of substantival difference? Contra Kant, Russell hoped to "infer real diversity, *i.e.* the existence of different things, ... from difference of position in space or time" (1897 187). But difference of position, owing to the axiom of relativity (as stated above in Sec. 1.3), seemed uniquely unable to support any such inference. But then, no principle pertaining to "substantival complexity," to difference of substance, was forthcoming from an analysis of space. Russell was in hot water.

1.6 Getting to Monism

Russell's solution to this problem changed over the next few years, but initially it took the following form. He held to the belief that spatial sensation yielded knowledge of the existence of more than one substance. Indeed, Russell continued to insist, at first, that the positive content of the form of externality had to consist in principles which said what it was for one substance to differ from another. Only, the substances of whose difference we learned in spatial sensation were not themselves "parts" or "elements" of space. Space, says Russell, is "not a thing, nor built up of things," "neither simple nor built up of simple things" (1897x 20). For "real parts ... would be discrete elements" (1896x 17), and space is continuous. Yet Space is "no whole, either, of any real sort" (1896x 13). Thus space is not composed of substances, nor yet itself a substance, and it therefore had to be a property. Notice, Russell's view is not that space consists in *the fact that* certain differing substance possess some property. Rather, strange as it may be, space itself is said to be a property. In Russell's mind, this immediately raised the question whether space was a quality or a relation, for these were the only two types of properties his view admitted. But in accordance with his belief that spatial sensation taught us of the existence of many substances, Russell held (at least at first) that space was a relation entered into by the substances whose existence we determined from sensation. These substances were related by the relation which was space, but they did not literally compose space. Despite appearances, geometrical axioms were not strictly about space, but about these substances; they did not attribute properties to space, but spatial properties to these substances.

This, then, was Russell's initial solution. It was not without its own difficulties, some of which Russell himself felt. There was first of all the difficulty previously mentioned as to whether or not a property can correctly be said to be divisible (1897x 19). Accordingly, Russell came to believe that space was not divisible at all, and that its apparent divisibility was "psychological illusion" (1897 196). Geometry was "compelled" (1897 189) to treat space as a "thing *per se*", and it derived from this the useful illusion that space was divisible, but the reality of space was otherwise. I will not comment on this idea, except perhaps to say that it has an early adherent in Spinoza. A second problem for

Russell's initial solution was the difficulty as to precisely what relational property space was supposed to be. One can easily enough think of spatial relations, such as those denoted by such familiar phrase as "x is some distance from y" or "x is to the left of y." But these relations could hardly be said to themselves *be* space. Even when Russell later came to doubt that the property which was space was a relation, a problem similar to this second one arose. Spatial qualities like being extended are perhaps easy enough to imagine, but what quality was *itself* space? It is not clear Russell ever recognized this question as a problem for his solution.

Connected with this second problem is a third, this one recognized by Russell himself. If space is a property, what exhibits it? Initially, while Russell continued to hold that space was a relation, the third problem required that he say just what stood in this relation. Here Russell took the substances whose difference were known to us by spatial sensation to be the things that stood in the relation he called "space." The further question might well be asked as to just what *these* are, but little need be said for my purposes. Russell did consider these substances to be material (on the grounds that we may come to have true beliefs about them through sensation), and he called them variously "material points (1896x 14) and "particles" (1897x 23). He seems in fact to have identified them outright with mass points.

Russell's initial solution did not last long. Sometime in 1897 he came to believe that the space was a quality, and not a relation. It was at this time that he wrote the dialectical argument with which I began. The reasons for his change depend on an equivocation similar to that mentioned above between the terms "substance" and "logical subject." Mass points must on the one hand be substances, and thus exist "by themselves" or independently; but on the other hand they seem to depend on further mass points to which they are essentially related. Again, the exact details the "antinomy" Russell deduced from this need not be stated (see 1896x 18-19). The key point here is simply that, for much the same reasons as he changed his view as to whether space was substantive or not, Russell came to believe that space, though still a property, was not a relation. Of course, the new view had still to resolve the third problem as to what precisely exhibited the quality identified with space. Again, the substance in question was material; in fact, strange to say, it was matter itself: "Matter is the One Whole (sic), of which space and motion are mere adjectives" (1897x 22). Russell's new solution left little room for the form of externality he had labored so hard to put in place in (1897); for there were no longer different substances to be sensed. But eventually Russell simply could not see how the existence of distinct substance could be made compatible with the apparent fact that they had essentially to bear relations to one another. The hot water which Russell's systematic equivocation had gotten him into in (1897) drove him to monism.

1.7 From Monism to Analytic Philosophy

Russell's shifting solution to the problem described in Sec. 1.5 shows his early indecision as to whether there was in the world one or many substances. But the alternatives, as Russell saw them then, spanned only the last two stages of his 1897 dialectic. I want now to resume my discussion of Russell's 1897 dialectical argument with the goal of better understanding what it was Russell gave up when he rejected idealism in 1898. I hope to show in Chapter 3 that an idea closely related to what he gave up can be used to make sense of impredicativity.

In this Section I make use of possible-worlds semantics. Russell, of course, had no such semantics in mind for his statements. It would be wrong, therefore, to claim that the interpretation of the principles I discuss in this Section precisely capture Russell's ideas at the time. Yet this is not to say that a possible-worlds semantics can be of no use in the effort to become clear as to what Russell *did* believe as an idealist. On the contrary, I think it will become evident that Russell had interesting reasons for rejecting idealism, and that these make sense even in a more formal setting. There is, too, an additional benefit to be gained from using a contemporary semantics. I shall want to state my own modal account of impredicativity in these terms, and having that account and Russell's early views expressed in a similar way will greatly facilitate comparison.

1.7.1 Russell's Dialectic Revisited

Russell's 1897 dialectic ran as follows:

> The principle of our dialectic appears to lie in making the Whole gradually more explicit. Our separate particles turn out, first to be related to other particles, and then to be necessarily related to *all* other particles, and finally to err in being separate particles at all. With this we pass to the plenum. (1897x 23)

I said in the introduction that the first stage of Russell's dialectic may be taken to represent *atomism*, the philosophical framework which Russell ultimately adopted, and in terms of which he tried a third time to give an account of mathematical continuity. Yet the last two stages of his dialectic retained a certain significance to Russell even after he had so strictly dissociated himself in 1898 from idealism, which is to say from the philosophical frameworks associated with the last two stages of his dialectic. In particular, Russell continued to believe that, once at the second stage, the transition to the third was unavoidable. Roughly, this transition becomes Russell's later argument that monadism, the philosophical framework associated with the second stage, collapses to monism, that associated with the third (1900 58-9, 1907 39). Russell's later claim suggests that he saw some difficulty getting to the second step at all. This, I think, can be seen to be the case.

In Russell's numerous arguments against the idealists, a principle he calls the "doctrine of internal relations" plays a pivotal role. Russell states this principle in a variety of ways, not all of which are equivalent. Behind some of the statements Russell's gives of the principle is the important, if imprecise, idea that *a relation must be essential to its terms.* This idea may be formalized in the following way: for any x and y, and for any 2-place relation R,

$$xRy \rightarrow \Box xRy \qquad (1)$$

As an interpretation of the doctrine of internal relations, (1) is intended to be a very general claim about all (2-place) relations. It may be read: if any R relates any x to any y, it does so necessarily. In a possible-worlds semantics, (1) will be evaluated as true just in case the formula 'xRy' is evaluated as true in all possible worlds whenever it is evaluated as true in some possible world.[16] But in order for the use of "□" in (1) to capture a notion of essentiality, it must be stipulated that 'xRy' may be assigned the value true even in worlds where one or both of x and y do not exist. Without this stipulation, (1) would have to be modified to express the essentiality of relations to their terms.[17] The suggested stipulation, however, considerably simplifies the formalization of the above imprecise statement of the so-called "doctrine of internal relations," and may be accepted here despite the apparently odd idea that

[16]Here and elsewhere I use the phrase "'xRy' is evaluated as true in a world" as a shorthand for the phrase "for some assignment of objects to 'x' and 'y', 'xRy' is evaluated as true in a world." Without the abbreviation, I should say: (1) will be evaluated as true just in case if, for some assignment of objects to 'x' and 'y', 'xRy' is evaluated as true in a world, then on that assignment, the formula 'xRy' is evaluated as true in all possible worlds.

[17]An effective modification would be (1'): for any x and y, and for any 2-place relation R, $xRy \rightarrow \Box((Ex \vee Ey) \rightarrow xRy)$, where "Ex" is read "x exists" and is interpreted as true in a given possible world just in case the value assigned to "x" on that interpretation is an element of the domain of the possible world.

"xRy" may be true in worlds where x or y do not exist.

(1) bears an interesting relation to Russell's 1897 dialectic, but before that is discussed, it may be useful to indicate how the early Russell would have understood the words "any relation" as they occur in (1). I said that (1) was intended to be a general fact about 2-place relations. One might note, in the first place, that the qualifier "2-place" was redundant for Russell, since he thought all relations were 2-place. More important, Russell says at (1897 198) that difference or "externality" is "a necessary aspect or element in every relation." In other words, all 2-place relations are irreflexive. This view seems odd today, but it is not necessary to pay it much heed. For my purposes it does not matter whether we consider Russell as having accepted (1) for any 2-place relation whatsoever (all of them being, on his view, irreflexive) or more simply as having accepted it only for all irreflexive relations R. A very similar consideration applies to Russell's view that difference is not itself a relation, but rather presupposed by all relations. The idea may seem ludicrous today, since it is hard to image what else difference could be, if not a relation. But I will let the matter pass, with perhaps only the note that Russell would very probably have accepted (1) with "≠" substituted for "R."

Another point should be made regarding Russell's understanding of the term "relation." Russell (most of the time) viewed predication as a relation. To say that an entity exhibits a quality is to assert that a special relation obtains between that entity and that quality. Thus what we might be inclined to symbolize "Qx," Russell may have included as a special instance of "xRy." If so, (1) for Russell would have as a special case the following:

$$Qx \rightarrow \Box Qx \qquad (2)$$

If this is the case, and if, as I suggested earlier, Russell held that, among properties, there are only qualities and relations, he would have been in a position to conclude that every property exhibited by an object is necessarily exhibited by that object. This is certainly an odd view, but it is worth noting that idealists like Bradley and Hegel may well have wanted this conclusion. Later I will suggest that something quite like this conclusion was important in Russell's rejection of idealism.

(1), interpreted in the above manner, is of special interest in regard to the transition from stage one of Russell's 1897 dialectic to stage two. In the dialectic, we begin with the proposition that "separate particles [are] related to other particles." This was interpreted in Sec. 1.1 so as to imply that some irreflexive relation is instantiated. In the hypothesis of (1), we suppose similarly that, for some irreflexive relation R, and for x and y, two arbitrary possible entities, 'xRy' is true in some possible world. In Russell's dialectic, we arrive with the second stage at the proposition that particles are "necessarily related to *all* other particles." This may be divided into two parts: namely, the particles are necessarily related by some R; and they are all related to one another by R. (1) clearly motivates the first claim, since the consequent of (1) affirms that x and y are necessarily related by R. The second part of stage two can be added by adopting the principle that, for arbitrary x and y,

$$\exists R xRy \qquad (3)$$[18]

The consequent of (1), then, together with this latter principle, quite plausibly represent the second stage of Russell's dialectic. For very roughly, these two amount to the view that all things are necessarily related (by some relation or other) to all other things.

The point of the above comparison is that the "doctrine of internal

[18]Russell would no doubt have preferred us to say: for arbitrary x and arbitrary $y \neq x$, $\exists R xRy$, where R is irreflexive but not itself $\neq$; but such niceties may be ignored.

relations," interpreted as (1), plays a pivotal role in Russell's 1897 dialectic. In particular, (1) may be understood as motivating part of the transition from the first to the second stage of Russell's dialectical argument, his explanation or account of the continuity of the material world. (1) then is well situated to be one of the principles Russell rejected when he abandoned idealism in 1898. Below I argue that Russell's rejection of idealism consisted largely in his rejection of (1), but it is important to note that there are other plausible principles which may have been the doctrine of internal relations as Russell understood this in 1898. I wish to digress for a moment and state one such principle, which will prove important in Chapter 3.

1.7.1.1 Ontological Dependency and Internal Relation

Perhaps the oddest fact about the doctrine of internal relations is that Russell seems not to have stated it until well after he rejected it. It seems to have operated as an unnoticed presupposition. There is, for example, no general enunciation of it in (1897). One does find Russell mentioning, in an almost offhand manner, "that interdependence which a relation requires" (1897 198). The presupposition here would seem to be that

> *any two distinct logical subjects, provided they are related in any way or another, depend on one another for their own existence.*

Much later Russell claimed to detect a similar presupposition in the writings of Joachim, an idealist rather after the Bradlian mold, which Russell expressed: if A is independent of B, A cannot be related to B (1906b 529; cf. also 1907 37). It is conceivable that the notion of mutual ontological dependency implicit at (1897 198) above is at work again in Joachim. Indeed, the same notion might have been the motivation for Russell's view noted above that "a thing *per se* cannot ... exist by itself, apart from the other terms of the relation" (1897 86). In practice, then, the so-called doctrine of internal relations may enforce some form of mutual ontological dependency upon related things. This may formalized as follows: for any x and y, and for any 2-place relation R:

$$xRy \rightarrow \Box(Ex \leftrightarrow Ey) \qquad (4)$$

(4) represents a second plausible interpretation of the so-called doctrine of internal relations as it was applied by idealists.

(4) does not follow from (1), but I suspect that in practice Russell took the two to come to much the same thing. In any case, he apparently had rather good reasons to accept (4), for it is plausible to suppose he accepted other principles from which, together with (1), (4) does follow. One of these is sometimes called "K", and ensures the distributivity of "$\Box$" with respect to "$\rightarrow$":

$\Box(A \rightarrow B) \rightarrow (\Box A \rightarrow \Box B)$. (4) follows from K, (1) and the following principle: for arbitrary x, y and 2-place R:

$$\Box(xRy \rightarrow (Ex \wedge Ey)) \qquad (5)$$

That Russell held K seems plausible of itself. That he held (5) may perhaps inferred from the following, which he wrote before his 1898 revolution:

> whatever can be the subject in a true judgment must have what, in opposition to existence, I shall call *Being*. (1898x 168)

If "xRy" expresses a true judgement, then the logical subjects x and y have being. (5), then, interprets "x has being" by "Ex," which is said to be true in just those possible world where x exists. While not an historically accurate account of Russell's intention, (5) might still be thought to be a serviceable approximation.

It should be observed that (5) does not run counter to the stipulation made above, that "xRy" be interpretable as true in worlds without x or y. The stipulation simply requires one to express an idea explicitly, *within a theory*, rather than implicitly as a convention for stating theories. Stating (5) explicitly helps to show that Russell had good reason to believe (4), a second interpretation of the doctrine of internal relations. But in fact, it is not likely that Russell clearly distinguished (1) and (4), or (5) from the stipulation opposite to the one I made. (4) is not crucial to my account of Russell's rejection of idealism, but it is relevant to my account of impredicativity in Chapter 3. With these remarks I will end my digression.

1.7.2 Revolution and the Rejection of Idealism

I come now to the main question: what principle or principles did Russell give up during his 1898 revolution, in which he abandoned idealism and embarked upon analytic philosophy? This question is a difficult one, and I do not mean to answer it in any complete way. But something along the lines of the following argument may well have occurred to Russell.

Let us suppose there are two logical subjects, x and y. Like Russell, we might say these are known to us through sensation, or even known *a priori* on the basis of a form of externality. More formally, this may be interpreted as the claims that (i) "Ex" is true in *some* possible world (not necessarily the actual world; call it w_x); (ii) "Ey" is true in *some* possible world w_y (w_y not necessarily the actual world); and (iii) "$x \neq y$" is true in all possible worlds.

As mentioned above, Russell seemed to believe that there is a relation standing between any two logical subjects. This was formalized by (3) above. Thus for logical subjects x and y just introduced there is a relation R such that xRy. More formally, this may be interpreted as the claim that "xRy" is true in a world *w*. (Presumably *w* is the actual world, but this is not important to my purposes.) It should be noted, however, that it is not assumed that $w=w_x$ or that $w=w_y$. For all we know, it could be that $w \neq w_x \neq w_y \neq w$.

Russell further believed that any logical "subject in a true judgment must have ... *Being*." This was formalized above by (5). This may be interpreted in the present context as the claim that in every world *w* where "xRy" is true, so is "Ex ∧ Ey". Given (5), there is no possible world in which "xRy" is true but in which x and y fail to exist. There is as yet no reason to believe the converse holds; x and y may exist in worlds where "xRy" is not true.

(1) supplies this converse. Given (1), and the x, y and R as above,

$$xRy \rightarrow \Box xRy.$$

As interpreted above, this signified that, if "xRy" was true in one possible world, it was true in them all. Now "xRy" is true in *w*, so "xRy" is true in all possible worlds. By (5), therefore, "Ex ∧ Ey" is true in all possible worlds.

The problem with this is that x and y were perfectly arbitrary. It was not even supposed at the outset that they existed together in any single possible world. It follows that every possible entity, that is to say every value of any free variable such as "x" or "y", exists in every possible world. Similarly, regardless of the interpretation of "S" and "z", "xSz" will be true in all possible worlds provided only that it is true in some possible world. Parallel remarks apply to "Qx", given (2). Put very roughly, everything possible is actual. Thus in the presence of (i)-(iii), (3) and (5) then, there is no distinction to be made between possible worlds.

In this context, it is difficult to attribute much significance to "□", the necessity operator. After all, if "□xRy" is true just in case "xRy" is true in all possible worlds, and there is no distinction to be made between possible worlds, why not assert merely "xRy"? I suggest that this question, or one quite like it, is the question Russell came to ask himself in 1898, and his answer, that *merely* "xRy" should be asserted, constitutes the heart of his 1898 revolution.

The idea that what Russell rejected in 1898 was the notion of necessity fits well with the historical facts.[19] In the first place, a definitive feature of Russell's later metaphysics is the lack of any robust role for necessity. For instance, Russell later writes that Bradley's

> opinion seems to rest upon some law of sufficient reason, some desire to show that every truth is "necessary". I am inclined to think that a large part of my disagreement with Mr. Bradley turns on a disagreement as to the notion of "necessity". I do not myself admit necessity and possibility as fundamental notions; it appears to me that fundamental truths are merely true in fact, and that the search for a "sufficient reason" is mistaken. (1910a 374)

Thus Russell thought his rejection of necessity marked a major point of difference between his late metaphysics and Bradley's. But other historical details fit well with the present account of Russell's revolution. Without the notion of necessity, (1) become not false but meaningless. Under these conditions, there is *no* relation R such that, for some x and y,

$$xRy \rightarrow \Box xRy$$

This is far stronger than a simple denial of (1), and yet it seems to be what Russell held. For Russell the atomist, "fundamental truths are merely true in fact." Insofar as (1) is an interpretation of the statement, "all relations are internal", Russell, in rejecting (1), did not merely claim that some relations failed to be internal; rather he insisted that no relation was internal. Russell writes:

> Mr Bradley has argued much and hotly against the view that relations are ever purely "external". I am not certain whether I understand what he means by this expression, but I think I should be retaining his phraseology if I described my view as the view that *all* relations are external. (1898x 142)

[19]See also (1903 454) quoted later. In a paper on Meinong, Russell wrote: "it seems impossible to distinguish, among true propositions, some which are necessary from others which are mere facts" (1904 26).

I conclude, then, that Russell's 1898 revolution consisted very largely in his rejection of the notion of necessity, and his consequent strong denial of (1).

I will make one final remark concerning (4). Since the consequent of (4) contains a necessity operator, Russell after his revolution must apparently deny (4) any significance whatsoever. But just as (4) seemed to act as an unstated presupposition for Russell the Hegelian, and so Russell after his revolution seems to have generally left it unstated and not singled it out particularly well as an instance (or even as a consequence) of the doctrine of internal relations. In Chapter 3 I will return to the question of the role of (4) in Russell's atomistic metaphysics. There it will be found that the modal notion of mutual ontological dependency formulated in the consequent of (4) can be extended in a certain way to yield an modal account of impredicativity.

Chapter 2: Poincaré: Two Hard Choices

2.0 Introduction

I shall endeavor to explain Poincaré's ideas in the foundations of mathematics, continuing in a historical and narrative style. It would be sufficient to consider solely the contributions Poincaré made to the subject of predicativity, but it is advisable to examine Poincaré's more general positions as well. By doing so, one finds *prima facie* tension between what Poincaré insists upon and what he forbids — more precisely, between his opinions as to the legitimacy of "les vraies mathématiques" and as to the illegitimacy of impredicative definitions. In brief, Poincaré first developed a general epistemological account of our knowledge in arithmetic and analysis, but then later sought to restrict our knowledge to predicative mathematics. Although the latter move is in keeping with some aspects of his earlier theory, it is in serious conflict with certain other positions Poincaré did not wish to abandon. One must allow for change in his views over this period of more than twenty years, but Poincaré nevertheless has some hard choices to make.

As in my discussion of the early Russell, I will develop Poincaré's ideas in part by contrasting them with Kant's. This will add unity to my overall discussion, but it helps as well because of the continuing importance of Kant's ideas in regard to the philosophical issues of impredicativity. The two hard choices Poincaré has to make turn on his interpretation of the Kantian conception of the synthetic *a priori*, which Poincaré appeals to in the foundations of arithmetic and analysis. In each case, the only plausible formal equivalents of the conception are impredicative, and Poincaré must choose between an illegitimate Kantian intuition or a legitimate formal impredicativity. I will suggest that this conflict puts pressure on Poincaré's subjectivism, *i.e.*, his belief in the dependence of mathematical entities on activities of the human mind.

2.1 Poincaré's Program: The Primacy of Arithmetic

To account for mathematical knowledge, Poincaré endorsed, with certain important modifications, Kant's view that mathematical knowledge is derivable from synthetic *a priori* intuition. Of course, the significance of the term "synthetic *a priori*" is no clearer in Poincaré than it is in Kant, but for general historical purposes a full analysis is not necessary. The following rough and partial gloss will serve my initial purposes, and would very likely meet with the assent of both Poincaré and Kant: mathematical knowledge is *a priori*, epistemically certain knowledge of necessary truths which do not follow from the meanings of words used to express them.[20] Again, I will not undertake a complete exposition of all the elements of this initial gloss, but I will come back to some of them as my narrative demands more detail. Here I want to begin the discussion by pointing out that Poincaré (at least in the earliest phases of his career) sought to trace the synthetic *a priori* character of mathematical knowledge back to a *single* primitive *a priori* notion, the "intuition du nombre pur, celle d'où est sorti ... le véritable raisonnement mathématique." "Cette intuition du nombre pur," he held, is "la seule qui ne puisse nous tromper" (1900 122). With characteristic optimism, the early Poincaré conceived of this single indubitable intuition as underlying *every* branch of mathematics:

> Autrefois, on parlait d'un grand nombre de notions, regardées comme primitives, irréductibles et intuitives; telles étaient celles de nombre entier, de fraction, de grandeur continue, d'espace, de point, de ligne, de surface, etc. Aujourd'hui une seule subsiste, celle du nombre entier; toutes les autres n'en sont que des combinaisons, et à ce prix on a atteint la

[20]It is best, perhaps, to indicate at the outset that Poincaré does not adequately distinguish the notions of aprioricity, certainty and necessity. He takes each to be sufficient evidence for the others; e.g., if we know P with certainty, we know it *a priori* and P is necessary. The triple mistake is traditional, and I will not be calling Poincaré on his error, although I will have to interpret several important passages where it occurs.

rigueur parfaite. (1899a 129)

My first major task, then, will be to examine this fundamental intuition into natural number (see Sec. 2.1.1). But before that it is worth pausing over Poincaré's list of reduced notions. Except for fractions (the reduction of which by Poincaré's day presented no special problem) all the mentioned notions receive explicit reduction in Poincaré's hands.[21] The notion upon which Poincaré expended the greatest effort was that of space, the topic of (1895), (1898), (1903) and (1912). Its inclusion here among the non-primitive notions marks the first important modification Poincaré made to Kant: space is not a form of our sensibility. Below I concentrate only on a component part of this reduction, namely Poincaré's treatment of "le grandeur continue" or, more simply, of continuity (Sec 2.2). This notion is important of course because a correct analysis of it is required in the foundations of geometry, analysis and topology, all of which Poincaré made major contributions to and considered part of "les vraies mathématiques". Poincaré believed that the reduction of continuity to the natural numbers had been accomplished with "perfect rigor." This belief reveals his early faith in the great arithmetization of analysis that had been pursued throughout the 19th century. Poincaré never wavered from defending the certainty and legitimacy of analysis, but he was eventually to adopt a principle which would leave him little room for an explanation of our knowledge of classical continuity. This situation, we shall see, poses the second of the hard choices I want to raise for Poincaré: he believed we had knowledge of classical continuity, which is impredicative, but he rejected impredicativity (Sec. 2.2.2).

At the turn of the century, then, Poincaré was upbeat. Not only had

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[21]Points are discussed in (1895 640-1), (1897 65-7) and (1898 24 ff), lines in (1898 20), and surfaces briefly in (1899 130). Some indication of Poincaré's view of fractions is found in (1893) and (1904 260).

the long-sought arithmetization of analysis been accomplished, but it had been accomplished with perfect rigor: "On peut dire qu'aujourd'hui la rigueur absolue est atteinte" (1900 122). Poincaré was aware that if analysis was to be traced back to a primitive intuition of the natural numbers, the construction of subsets ("combinaisons", elsewhere "systèmes") of the set of natural numbers would have to be allowed for. The infinite would have to be tolerated, but for Poincaré, even as late as 1902, this was not a problem:

> Notre façon de concevoir l'infini s'est également modifée. M. G. Cantor nous a appris à distinguer des degrés dans l'infini lui-même .... La notion du continu, longtemps regardée comme primitive, a été analysée et réduite à ses élémentes" (1902a 93-4).

Poincaré is writing here after almost twenty years of familiarity with Cantor. He was one of the first mathematicians in France actually to *employ* results of Cantor, and in 1883 he aided in the first French translation of several of Cantor's important early papers.[22] Thus, despite Poincaré's later condemnation of the new "façon de concevoir l'infini", he evidently felt early on not only tolerance but considerable sympathy for the work of Cantor. He included Cantor's theory, or at least as much of it as was required for the analysis of the continuum within "les vraies mathématiques" and accorded it the same "perfect" and "absolute" rigor he found in other contemporary mathematics.

2.1.1 Pure Arithmetical Intuition and Mathematical Induction

But I now return to the single arithmetical intuition Poincaré early on identified as the sole primitive concept in mathematics and the source of "le véritable raisonnement mathématique". I mentioned that, although

[22]These translations, as it turns out, were Russell's first contact with Cantor. His notes have been preserved and were published in (1896x 463-481). For further references on Poincaré's early involvement in support of Cantor, see Heinzmann (1985 15). Veuilleman (1968 213) states that Poincaré in this early period was "encore cantorien", and Hadamard (1921 161, 171) discusses generally Poincaré's contributions to topology.

Poincaré considered himself a Kantian, he introduced certain modifications of Kant's views, one of which was his denial that space was a primitive, synthetic *a priori* form of intuition. The second modification is Poincaré's specification of the "véritable raisonnement mathématique" licensed by our *a priori* intuition into pure number. In particular, that intuition underwrites the application of mathematical induction, "le raisonnement mathématique par excellence" (1894 379; 1905 818). "Cette règle," he says, "inaccessible à la démonstration analytique et à l'expérience, est le véritable type du jugement synthétique *a priori*" (1894 381-2).[23] This idea, that mathematical induction is the principle which articulates the content of the primitive intuition at the base of mathematics, is new to Poincaré and not to be found in Kant's own work. But other familiar features of the traditional Kantian notion of synthetic *a priori* are still found in Poincaré. For example, mathematical induction for Poincaré is the principle which establishes "la possibilité même de la science mathématique", understood as yielding certainty of truths which are necessary but not analytic (1894 371; cf. Kant [B14-15]). Secondly, the principle of induction, according to Poincaré, "n'est que l'affirmation d'une propriété de l'esprit lui-même" (cf. Kant A36-7/B52-3). As we shall see, there are other points at which one may wish to challenge Poincaré's allegiance to Kant, but in these respects there is similarity.

It was just noted that, according to Poincaré, the principle of mathematical induction is an affirmation of a property of the human mind.

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[23]He goes on: "On ne saurait d'autre part songer à y voir une convention." But in his interpretation, the logicists ("les logiciens intransigeants") would soon dream of just that (cf. 1905 818). Notice the equation of conventionality and explicit definability. Convention in Poincaré is a difficult but crucial notion, and his competing demands on it are discussed more later. Here the difficulty is that, since conventions, on his view, are neither true nor false (1895 645, 1898 42), the logicist justification of induction on the basis of an explicit definition, construed as a convention, could not be a justification for believing induction is *true*. But this is obviously a misrepresentation of the logicist position.

It is not clear on the face of it what Poincaré intends by this. How can a mathematical principle *just be* the affirmation of a property of the mind? Closer examination shows that Poincaré has several theses in mind which he is not always careful to distinguish. The property of the mind which he wants to emphasize is a capacity. The assertion of the principle of mathematical induction "n'est que l'affirmation de la puissance de l'esprit qui se sait capable de concevoir la répétition indéfinie d'un même acte dès que cet acte est une fois possible" (1894 382). The idea that the mind can indefinitely repeat certain types of acts is relatively uncontroversial, although obviously a qualification such as "in principle" would have to be added and analyzed. Rather than undertake such an analysis, I will try to explain the relation, as Poincaré conceives it, between this capacity or "puissance" of the mind and mathematical induction.

One thing is clear: the capacity Poincaré has in mind is closely related to our understanding of the sequence of natural numbers or, in his terminology, to our "intuition du nombre pur". In fact, one indefinitely repeatable act attributable to the mind is the very creation of the natural numbers themselves:

> Quand je parle de tous les nombres entiers, je veux dire tous les nombres entiers qu'on a inventés et tous qu'on pourra inventer un jour" (1909 477).

It is odd, but not uncommon, to speak of creating or inventing mathematical objects. The early Russell, for example, also held that "counting creates numbers" (1897x 20). For Poincaré, invention takes place by *definition* or *construction*. Abstract mathematical objects

> "n'existeront qu'après qu'ils auront été construits, c'est-à-dire après qu'ils auront été définis" (1912 7).

Since the natural numbers are thought to be "invented" or "constructed" by repeated acts of explicit definition, Poincaré evidently conceives of the natural numbers as being introduced, not as a totality by an explicit definition, but successively by a so-called recursive definition. A "définition par récurrence", he says, "est d'une nature particulière qui la distingue déjà de la définition purement logique; [elle] contient en effet une infinité de définitions distinctes" (1894 375). The synthetic "intuition du nombre pur" itself, then, can be codified or expressed by means of a recursive definition. This interpretation accords with Poincaré's view (which is very important in Sec. 2.1.3) that mathematical induction is not *analytically deduced* from prior synthetic principles, but rather *follows synthetically* from a certain intuition (1894 371).[24] For induction is informally motivated by a recursive definition of natural numbers, but cannot be formally deduced from it. Additional confirmation of this interpretation is found in one assessment Poincaré gives of the significance of the principle of mathematical induction itself, which implies that the natural numbers are indeed defined "par récurrence": "Le principe d'induction complète", he writes, "signifie que sur tout nombre qui peut être défini par récurrence, on a le droit de raisonner par récurrence" (1906a 142, 1905 835). Thus the "puissance" of the human mind indefinitely to repeat the act of adding one to a previous result underpins our understanding of the natural numbers, and a recursive definition of these expresses or captures our synthetic "intuition du nombre pur". Poincaré stresses induction as the "le véritable type du jugement synthétique *a priori*" because induction is the primary principle which spells out the content of the intuition represented by this recursive definition, and he is correct to see this connection between them as non-deductive.

From this it is clear that Poincaré's conception of synthetic *a priori* intuition in arithmetic actually consists in three distinguishable theses. First,

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[24]Cf. Beth (1955 234) on this point. Actually, Poincaré seems to have admitted the existence of *explicit* definitions of the (set of) natural numbers, but denied that they yield induction "analytically". In some cases, the only strong principle appealed to in the relevant derivation is the axiom of choice. See (1905 303, 1906 867). In all cases, some form of impredicativity is involved.

i) the human mind has the power to repeat a type of act indefinitely.

We see or intuit that "il n'y a pour ainsi dire aucune raison intrinsèque de s'arrêter" (1893 31). Second,

ii) the natural numbers are created by the exercise of this power.

A recursive definition of the natural numbers states the instructions or rules for this "creation". Finally,

iii) we see by "une intuition directe" that our creations obey the principle of mathematical induction.

The first two theses relate directly to a modal claim Poincaré wants to make about the human mind. The third thesis is more plainly epistemic in that it offers an *a priori* justification for a specific proposition. Still, Poincaré can be found at various points identifying each of these with the allegedly unique synthetic *a priori* "intuition du nombre pur." For us, obviously, it is advisable to keep the three theses separate in our minds. Note also that it is, strictly speaking, only at this third stage, once we have intuited or seen the validity of instances of mathematical induction, that we may meaningfully speak of "tous les nombres". Before long we will see that some of these instances are in an important respect impredicative, and this will raise the first hard choice for Poincaré: he will have to chose between a legitimate impredicativity or an illegitimate "pure intuition".

A few interpretive and historical remarks are perhaps in order here. Poincaré may not have intended a hard and fast distinction between *i* and *ii*, but opted instead for a kind of structuralism. That is, he may have thought that virtually any indefinitely extendable linear sequence of mental acts was tantamount to the construction of the natural numbers. First, Poincaré was aware of instances of mathematical induction which pertain not to the natural numbers, but to indexed mathematical entities of other sorts. Second, Poincaré evinces some doubt as to the genuine definability of individual numbers (1905 823-4). Both of these suggest that he took the form or structure of indefinite repeatability to be more important than the content of any particular indefinite sequence, and this seems to be confirmed by certain statements Poincaré makes (1903 427, quoted below) which suggest even repeated human body movement suffices for our intuition of number. Finally, in the following passage, which occurs in a rather different context, Poincaré once more appears to confirm that he took a structuralist attitude:

> Les mathématiciens n'étudient pas des objets, mais des relations entre les objets; il leur est donc indifférent de remplacer ces objets par d'autres, pourvu que les relations ne changent pas. La matière ne leur importe pas, la forme seule les intéresse. (1893 28; cf. 1906a 142 and 1898 40)

Still, it is not necessary for my purposes to establish Poincaré's structuralism beyond the possibility of doubt. However *ii* is interpreted, it is bound to appear astonishing. According to Poincaré, the natural numbers are created by the human mind. In fact Poincaré believes quite generally in the dependence of mathematical entities on human mental activity, and he eventually was to call this belief "idealism" (1909 10-11). I will use the term *subjective idealism about F's* (or sometimes more simply *subjectivism about F's*) for the view that F's are the products, the potential products or even the acts themselves of the human mind. In contrast to his views about mathematical objects, Poincaré was *not* a subjective idealist about actual physical space, for example. Although he held that space was given to us in a contradictory form (cf. Russell 1896x 57) and possessed no intrinsic metrical properties or dimensionality, he nevertheless thought it existed objectively (without "self-contradiction") and exhibited other properties quite independently of our mental activity. Mathematical objects, on the other hand, were created by, and are dependent on, the activities of the human mind for their existence.

How early did Poincaré adopt subjective idealism about mathematical objects? This question is important because the restriction of mathematical knowledge to predicative mathematics is typically argued for in terms of subjective idealism about mathematical entities. The quotations included above show that Poincaré was indeed a subjective idealist about mathematical objects, but only as of 1909, three years after his adoption of a predicativity constraint. There is, as far as I know, no early statement of similar generality and explicitness, but it is nevertheless reasonable to extend Poincaré's subjective idealism back to his earliest writings in the philosophy of mathematics. Certainly in (1893) the mathematical continuum in particular (as opposed to physical space) is understood as a human creation; and in (1894), as we have already seen, synthetic *a priori* judgements are said to affirm a property of the mind to act. Further, in (1905 819) Poincaré distinguishes explicitly the "existence des objets matériels", which alone amounts to "objective existence" (1905 31; cf. the later 1905 297), from "l'existence en mathématiques". Thus in 1905, prior to the introduction of the notion of predicativity, Poincaré countenanced two sorts of existence: one pertained to material objects and was considered "objective", the other pertained to mathematical objects and was thus presumably "subjective".[25] The strain of subjectivism about mathematics in Poincaré's thought, then, seems to precede his adoption of a predicativity constraint in logic, and it is probably out of this strain that his predicativism develops.

It is, however, the thesis *iii* which marks Poincaré's most original contribution to the philosophy of arithmetic. The leading function of the "intuition du nombre pur" is that it licenses mathematical induction. The third thesis asserts that mathematical induction is justified in an *a*

[25]The term "subjective" is not used by Poincaré. To explain the non-objective sort of existence, Poincaré wrote that "en mathématiques le mot exister ne peut avoir qu'un sens, il signifie exempt de contradiction" (1905 819). This suggests the word "existence" could be strictly eliminated from mathematics by making appeal to the property of non-contradictoriness in concepts (or predicates). One must not infer from this the "conceptual" or "subjective" nature of mathematical existence, however, since Cantor thought that the non-contradictoriness of a mathematical definition was sufficient to commit one to the "objective" existence of the entity defined.

priori way for the "creations" mentioned in the first theses. This is an important and interesting modification of Kant's views on the founding intuition of mathematics. Thesis *iii* incurred the opposition of many of the logicists, and was, according to Poincaré, the "objet principal du débat" between them (1906b 867, 1905 832). Couturat (1905) for example maintained that one could formulate an explicit definition of the natural numbers from which it was possible *analytically* to derive mathematical induction. At first, Poincaré replied that the need to show the consistency of the logicist definition required an unseen appeal to induction (1905 829). But in an effort to formulate more general objections to logicism, Poincaré latched onto the paradoxes that had arisen. Rejecting the idea that the paradoxes arose from ordinary ("vraies") mathematics, he charged that the paradoxes revealed a deeper *petitio* systematically committed by the logicists. This deeper *petitio* consisted in the violation of a principle banning impredicative definitions and came to be known as the vicious circle principle. Below I discuss Poincaré's charge in more detail. Here I want to emphasize that, once the predicativity constraint had been formulated, Poincaré's primary interest in it was not really the solution to the paradoxes at all (which after all did not arise in "les vraies mathématiques") but rather the resolution of "le vraie débat" concerning induction. Poincaré claimed that there was an essential appeal to an impredicative definition in the logicist proof of induction, and that this undermined the alleged analyticity of their demonstration. But, as we shall see below (Sec. 2.1.3), Poincaré's own justification of induction on the basis of a recursive definition of the natural numbers (thesis *iii*) requires a similar appeal to impredicativity (cf. Parsons 1983). This puts Poincaré in a hard position: how can the impredicativity of the logicist proof of induction indicate a vicious circularity while the impredicativity embodied in the less explicit justification he favors be unobjectionable? An exactly parallel difficulty will face Poincaré in the case of continuity in analysis.

2.1.2 Poincaré's Kantian Credentials

An underlying difficulty here is the notion of synthetic *a priori* intuition. As I have just suggested, it will turn out that the only plausible formal equivalent of the allegedly synthetic character of induction is precisely the impredicativity essential to the justification of induction. One way to put this first hard choice Poincaré has to make is to raise the question: is the impredicativity of induction itself responsible for the synthetic character Poincaré wished to underline? But here clarity as to the notion of the synthetic *a priori* must first be maintained. According to the rough gloss of this notion given above, Poincaré's position includes the claim that induction is a necessary truth, known *a priori* and with certainty, but which does not follow from the meanings of words used to express it. Yet, traditionally, the notion of "pure" synthetic intuition has involved much more than this. Kant does not merely say that the justification of arithmetic requires appeal to an underlying synthetic *a priori* intuition; he also ties this intuition to time and the possibility of experience in general. He argues that since all experience occurs *in time*, certain formal or structural properties of time are deducible from formal or structural properties of experience. In fact, for Kant, the form of experience ("Form des inneren Sinnes") just is time; so that things in themselves, apart from experience of them, have no temporal properties, and experience as we know it would be impossible unless time had the formal or structural properties it does. Finally, Kant would appear to be committed to the claim that the formal temporal sequence with which we are acquainted *a priori* is isomorphic to the standard model for arithmetic, for he believes that our knowledge of this temporal sequence (*i.e.*, of the form of inner experience) suffices for knowledge of arithmetic. According to Kant, we can rest assured that arithmetical truths are necessary because their truth is a precondition of experience.[26]

Recently, however, Goldfarb has asserted that Poincaré does not share with Kant this more robust notion of synthetic *a priori* intuition. Poincaré's synthetic *a priori* "intuition du nombre pur," Goldfarb believes, is not tied to time or the possibility of experience:

> in Poincaré's hands the notion of intuition has little in common with the Kantian one. The surrounding Kantian structure is completely lacking; there is no mention, for instance, of sensibility or of the categories.... Intuition, in [Poincaré's] sense, ... might just as well be called "immediate conviction" (1988 63)

According to Goldfarb, Poincaré's usage of Kantian terminology, as well as his striking assertions of allegiance to Kant (cf. 1905 815-6; 1906 34), are not to be taken at their face value. I have suggested above certain modifications to the Kantian view which Poincaré made, but Goldfarb would rather emphasize the complete lack of the "surrounding Kantian structure" and argue that Poincaré has abandoned the traditionally Kantian notion of pure arithmetical intuition with its links to time and the possibility of experience. Poincaré, he thinks, ought not to be considered a Kantian at all. In favor of his interpretation Goldfarb can point to the sudden increase in Poincaré around 1905 of candidate synthetic *a priori* principles. How can all of the diverse principles Poincaré suggests indicate temporal order or the form of inner sensibility? This evidence is strong, but I believe nevertheless that Goldfarb has overstated his case. If one looks beyond Poincaré's work on impredicativity to his earlier foundational research, one finds a modified but nevertheless traditional conception of the synthetic *a priori*.

Part of the difficulty in discovering the pedigree of Poincaré's

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[26]As Moore (1899 399) argues, the inference Kant makes from necessity-for-experience to necessity *tout court* is fallacious. I will not base my criticism of Poincaré on his frequent acquiescence to the related shift from aprioricity to necessity.

understanding of the synthetic *a priori* stems from his failure to use the word "intuition" in a uniform manner. In (1900 121) Poincaré distinguishes several diverse classes of "intuition." The various classes are represented by quite different kinds of knowledge, including knowledge of traditional logical inference rules, convention, mathematical insight,[27] synthetic *a priori* judgement, empirical induction, imagination and sensory experience. (In (1912) he introduces "le veritable intuition géométrique" to this list, and this, as we shall see, may mark a major shift in Poincaré's late views.) Obviously, not all of these can play the important role that pure synthetic intuition plays for Kant. A further difficulty is that Poincaré is not consistent in his discussions of the properties of the different classes of intuition. In one work we read: "cette intuition du nombre pur [est] la seule qui ne puisse nous tromper" (1900 122); but a mere four pages later, he discusses "la Logique, qui peut seule donner la certitude" (1900 126; 1899 129). In a subsequent paper, which repeats entire passages from (1899a) and (1900), Poincaré assures us that "l'intuition ne peut nous donner la rigueur, ni même la certitude" (1904 262). Poincaré's use of the word "intuitive" therefore is not only non-uniform, it is outright contradictory, and this makes the task of extracting a traditionally Kantian notion particularly difficult.

Yet despite this jumble of inconsistent usage, Poincaré's different classifications of knowledge, as well as their important properties, are relatively straightforward. These, in fact, are drawn directly from the Kantian tradition. Empirical understanding (including sensation,

[27]Mathematical insight, the ability to discover ("inventer") new mathematical truths, was an important faculty for Poincaré. In (1900 129), he tried to reduce this class of intuition to another: "C'est l'intuition du nombre pur, celle des formes logiques pures qui ... permet ... d'inventer." The reduction (not mentioned in any other paper) is not carried out, and since "intuition" of this sort (insight) is admittedly fallible, it seems he could achieve it only by denying that synthetic *a priori* intuitions always yield certainty. Only in (1900) are conventions and (traditional) logical inference rules called "intuitive", and the same applies (as far as I know) to empirical induction.

imagination representation and non-mathematical induction) are always uncertain and never "sterile" or void of content. Inference rules of traditional formal logic, on the other hand, are known with certainty but the cost is sterility. "Tout raisonnement analytique est stérile" (1897 63). Convention is typically thought of as not productive of truth at all, but there are exceptions in Poincaré's handling of this difficult notion (see Sec. 2.2.1). Like Kant, however, Poincaré maintains that the class of certain non-sterile truths is non-empty, and these truths he "baptizes" (1894 371) "synthetic *a priori*". With the exception of convention, all of this is very much as it is in Kant, who first brings in the categories and the forms of sensibility precisely in order to account for so-called synthetic *a priori* knowledge, knowledge that is neither uncertain nor empty of content. This already leads to a minor correction to Goldfarb's rendering of Poincaré's "intuition" as "immediate conviction". Our immediate conviction must be *correct* in the first place, so that what we are convinced of must be true; and it must be non-sterile or non-analytic — it must have content not entailed by the meanings of the words used to formulate it. This correction is minor, however, since if Poincaré's conception of the synthetic *a priori* went no further than this one could hardly claim he was "vindicating Kant" (Goldfarb 1988).

Poincaré, then, recognizes mathematical induction as a truth about which we can be certain, but which is not analytic or "sterile", *i.e.*, not derivable from facts about the meanings of the terms required to state it. He sees his position as a vindication of Kant, but without what Goldfarb calls a "surrounding Kantian structure", these pledges of allegiance can mean little. In order for Poincaré to place induction in a "surrounding Kantian structure", he would have to claim that it plays epistemic roles similar to those played by the categories (forms of understanding) or the forms of sensibility in Kant. As I mentioned, one crucial role these "forms" play in Kant is to render experience possible at all. This raises the apparent problem that, even in 1894, Poincaré did not take mathematical induction to be the only principle in the class of certain non-sterile truths (1905 818; cf. 1894 374). Instead he considers it "typical" of such truths. He may mean that the others are logically equivalent (*i.e.* various other formulations of mathematical induction), but if not he will have to give some account of how non-equivalent principles can both articulate the "form" of our understanding or sensibility. At this early date the problem is perhaps not very severe, for Poincaré does not offer other examples of synthetic *a priori* principles. But when Poincaré went on the defensive against the logicists, he claimed an enormous variety of non-equivalent principles were synthetic *a priori*. These include all twenty of Russell's (1903) indemonstrable propositions; our understanding of Russell's nine (1903) primitive notions; the existence of logical sums and products (1905 829-30); the passing from "the point of view of intension" to that of extension (1905 832); the existence of an infinite class (1905 311-2); and the axiom of choice (1905 313).[28] This plethora of candidate "forms" of our experience gives powerful support to Goldfarb's claim that the term "intuitive" has no special Kantian sense in Poincaré. For how can this hodge-podge of "principles" ever be made to articulate a coherent universal form of human experience?

Goldfarb's point thus has particular appeal in respect to these later "defensive" applications of the term "intuitive". Poincaré's purpose is surely to show that the logicists have "immediate conviction" of principles not traditionally part of analytic logic. His purpose is dialectical, and this observation is reinforced by the fact that Poincaré actually doubts most of the "principles" just mentioned, and considers others outright false. But if we grant this, we save Poincaré from the

[28]The same status is also suggested, albeit with hesitation, for our understanding of one-one correspondence and even of independent variables (1905 830, 831). On the former, see Couturat (1900 26).

obligation to provide a coherent account of how these many "principles" together describe the "form" of our sensibility or understanding. Poincaré does not really believe these "principles" fall into the special class of certain truths not reducible to the meanings of the terms used to state them. If no single coherent notion of "intuition" is extractible from all of Poincaré's attributions, this at least does not affect the pedigree of his own views about nature of the "intuition" underlying induction. But Goldfarb's problem is not thereby entirely dismissed, however, since we still have no account of the "Kantian structure" surrounding mathematical induction. Without this, Poincaré's Kantian credentials with respect to what he calls "le veritable raisonnement mathématique" are still lacking.

In this narrower domain, however, Poincaré's status as a Kantian is justifiable. It was mentioned that, in Kant, the categories and the forms of our sensibility make experience possible in the first place. Now it is easy to show that Poincaré believed in the existence of categories, or forms of the understanding, playing this role.[29] Poincaré is quite explicit about this in his writing on the foundations of geometry. It is not so easy, but I think still possible, to show that theses *i* and *iii* are presupposed by, and thus in some sense are part of, at least one such category. If both of these can be accomplished, a third modification Poincaré made to Kant will be evident. The "pure intuition" underlying arithmetic is not a form of our sensibility but a form of our understanding, or at least part thereof. The "intuition du nombre pur" is categorial. This constitutes a important modification but not, as Goldfarb suggests, a total abandonment of Kant.

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[29]Poincaré seems to think of the categories as sets of propositions which must be true if experience is to be possible. It will be noted that in this respect Poincaré's categories are similar to Russell's "form of externality" as discussed in the last chapter. Russell's "form" may also be identified with a set of principles which must be true if (spatial) experience is to be possible. I will say that a proposition is *categorial* if it follows from the set of propositions which constitute the category.

It is undeniable that Poincaré made at least one Kantian distinction relevant to our discussion, namely that between the categories and the forms of sensibility, since he held that the concept of group was a category of thought:

> le concept général de groupe préexiste dans notre esprit, au moins en puissance. Il s'impose à nous, non comme forme de notre sensibilité, mais comme forme de notre entendement. (1895 645)

The idea here is that the general concept of group, and in fact the various concepts of more specific sorts of groups, are all available to us *a priori*, although which group we choose to represent a given set of experiences is not "imposed" upon us *a priori*. This idea is central to Poincaré's account of geometrical knowledge, for

> What we call geometry is nothing but the study of formal properties of a certain continuous group; so that we may say space is a group. The notion of this continuous group exists in our mind prior to all experience; but the assertion is no less true of the notion of many other continuous groups. (1898 41)

Thus the "general concept" of a group is a form of our understanding, and that form is "filled out" in various ways by *a priori* constructions of the pure understanding. We may then select, empirically or by convention, one such result as that which best corresponds to our experience of space. One step still within the *a priori* "filling out" process is the introduction of the notion of continuity, to which we will later turn (Sec. 2.2.2). As we shall see, however, Poincaré makes explicit appeal to the "intuition du nombre pur" in his account of our understanding of continuity. In fact, all three theses distinguished above regarding the interpretation in Poincaré of arithmetical synthetic *a priori* intuition are implicated in his account of continuity, although again Poincaré is not always clear which he means on any given occasion. What this implies, however, is that both the notion of continuity and the three theses regarding our "intuition du nombre pur" play specific roles in possible *a priori* specifications of a general form of our understanding. This suggests a very Kantian "surrounding context" for Poincaré's use of the term "intuition".

More can be said than this, however. It is likely that the "form of our understanding" actually *contains* "l'intuition du nombre pur", in at least two of the senses given above for the latter term. To see this, one must observe that the continuous group which is (sic) space is a group of displacements. Poincaré offers a phenomenalist understanding of displacement which he claims makes no appeal to spatial understanding. The details of this account are irrelevant here (see 1895 639-41, 1897 64-5, 1898 7-12), but the consequences of the fact that displacements form a group is highly relevant:

> This ... fact, that displacements form a group, contains in germ a host of important consequences. Space must be homogeneous; that is, all its points are capable of playing the same part. (1898 12)

Homogeneity (the property of space given by Russell as the "content" of his form of externality) is explained by Poincaré in the following terms:

> If a displacement D transports me from one point to another, or changes my orientation, I must after such displacement D be still capable of the same movements as before the displacement D, and these movements must have preserved their fundamental properties, which permitted me to classify them [phenomenally] among displacements. If this were not so, ... displacements would not form a group. (1898 12)

Groups are closed under their associated composition operation. Thus after any displacement, it must be possible to "add" any other displacement. But this presupposes thesis *i* above, namely that the human mind has the power to repeat a type of act indefinitely. Indeed, Poincaré says

> C'est de cette répétition que la raisonnement mathématique tire sa virtu; c'est donc grâce à la loi d'homogénéité qu'il a prise sur les faits géométriques. (1895 640; cf. 1898 9 ff).

The situation then is clear. Our understanding of the homogeneity of space, according to Poincaré, requires that thesis *i* be true, that we can repeat a certain type of act indefinitely. If this is our "intuition du nombre pur", then this intuition is required in the *a priori* "filling out" or specification of a general form of our understanding. Indeed, the use of principles equivalent to mathematical induction on geometric figures, conceived of as in analytic geometry, rests upon our capacity indefinitely to repeat an act-type in precisely the same way that our knowledge of the validity of induction for natural numbers rests, in Poincaré's view, on our ability to "create" the numbers, in short, to count. Whatever one thinks of the truth of these foundational accounts, one at least cannot doubt that, for Poincaré, the synthetic *a priori* intuition into pure number plays a clear role in a "surrounding Kantian context". This intuition is an integral part of a category, or form of our understanding.

One final piece of evidence for my modification of Goldfarb may be mentioned. It was said above that one basic role which the synthetic *a priori* principles play in Kant is that they make experience possible: they must be true if experience as we know it is to be possible. Now there is an obvious sense in which mathematical induction makes arithmetic possible, and Poincaré is quick to exploit this (1894 371). But he also seems to have held that, without synthetic *a priori* intuition, there would be no experience at all as we know it. Again, this can be seen by understanding the relation of our arithmetical intuition to the notion of a homogenous group. In (1899), Poincaré reviewed Russell (1897). This incident, which marks their first scientific interaction, was an impressive boost to the young Russell's career. Poincaré begins by making certain concessions:

> M. Russell commence par établir qu'aucune expérience ne serait possible sans une forme de extériorité [et] que cette forme doit être parfaitement homogène. *Sur tous ces points nous sommes d'accord* (1899 254; but cf. 1899 253; my emphasis).

As we have seen, however, Poincaré attempts to base homogeneity of space is precisely on an appeal to the "puissance" upon which mathematical induction is said to be based. Thus this "puissance" and mathematical induction itself are parts of a form of our understanding, and as such "aucune expérience ne serait possible" without them.

The question whether Poincaré was a "structuralist" in the sense described above is related to the Kantian role of the synthetic *a priori* in rendering experience possible. As was mentioned above, Poincaré's objections to logicist definitions of individual numbers suggest an indispensable role for individual numbers in normal thought (1905 823-4). Although in (1894 373) and (1897 60) Poincaré does imply that individual numbers can be defined, he considers their definition irrelevant to mathematical reasoning. This suggests that *genuine* definitions, *i.e.*, explicit definitions that are both legitimate and informative, are not in the final analysis possible. Again at (1903 427), discussing the muscular sensations whose indefinite repeatability is appealed to in the foundations of geometry, Poincaré says "c'est de leur répétition que vient le nombre". This again suggests that the nature of the entities standing in sequence (here the muscular sensations accompanying human body movement) is entirely irrelevant to mathematics; all that is relevant is the form or structure of the sequence itself. More importantly, however, Poincaré asserts that this "répétition suppose le temps." Now it is quite possible that Poincaré intended by this the Kantian view that our understanding of the ordering of the natural numbers *just is* our understanding of the structure of time. I know of no place in Poincaré where he makes his opinion unambiguous, but certainly he leaves room for this very Kantian position. In any case, he does conclude, *à la* Kant, that knowledge of space presupposes knowledge of time. His idea is that our knowledge of space arises from human body movement and this presupposes some awareness of time. It is clear, therefore, that the sequence of natural numbers remains for Poincaré intimately related to (perhaps even identical to) the structure of time, even though it does not articulate the form of our "inner sensibility" but part of a form of our understanding.

To conclude: the forms of sensibility are not the only synthetic *a priori* forms according to Kant or, yet more curtly expressed, "intuition" need not translate "Anschauung". The categories, or forms of our understanding, are also synthetic *a priori*. It is here that the "intuition du nombre pur" fits in, according to Poincaré, not in connection with our inner "sensibility." Arithmetical intuition is categorial. Contrary to Goldfarb, Poincaré does mention the categories and in fact makes epistemic use of them in his account of our knowledge of mathematics. Arithmetical knowledge is tied to the possibility of experience, and may even be tantamount to our understanding of temporal sequence. Poincaré's Kantian credentials with regard to mathematical induction are thus solid. He modifies Kant in order to vindicate him, and this modification should not be construed as an abandonment of "surrounding Kantian structure."

2.1.3 The Hard Choice in Arithmetic

We have seen that Poincaré's foundational views were Kantian, that he held that our knowledge of arithmetic is based on a categorial intuition into pure number. This intuition, in the sense of thesis *i*, renders experience possible, in the sense that it articulates an experiential capacity we know ourselves to possess, namely the capacity indefinitely to repeat certain types of acts (including physical acts of movement). But it also suffices for arithmetic in that Poincaré seeks to justify induction on the basis of thesis *i*. This justification is given as thesis *iii* above. I wish now to examine the problem that this justification is irremediably impredicative.

The impredicativity of mathematical induction has been recognized by some writers for over fifty years, but it has recently become the topic of a great deal of new research. The earliest result concerning it that I have been able to find occurs in Fitch (1938).[30] Early proof theoretic

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[30]Fitch shows that transfinite induction up to ω^{ω} is not provable in Russell and Whitehead's (1925), and this is well below Γ_0, the proof theoretic number of predicative analysis as determined by Schütte (1965) and Feferman (1964).

studies of impredicativity carried out by Feferman and Kreisel concentrated on notions of predicativity which included mathematical induction, but both authors give clear and early indication that this inclusion is motivated merely by historical considerations, and both assert that mathematical induction is itself in a specific way impredicative. Later, Royce published in (1969) a short proof based on Gödel's incompleteness theorem which showed again that induction was in a specific sense not predicatively provable.[31] Finally, some ten years ago Parsons argued that the specific sort of justification of induction favored by Poincaré (namely thesis *iii*) reproduces the impredicativity present in the formal justifications. Parsons' philosophical argumentation is amply confirmed by the emergence lately of *bounded* or *predicative* arithmetic, sub-theories of Peano Arithmetic which do not accept as valid those instances of induction signaled by Parsons as impredicative. This field is burgeoning, and bears interesting connections with theories of computational feasibility and complexity.

The work just cited suggests that, with regard to the foundations of mathematical induction, there are two broad alternatives one can adopt. Either (A) one accepts an *explicit* definition of the totality of natural numbers, and attempts *formally to deduce* induction from it; in this alternative, induction is a theorem and its epistemic justification depends only on the epistemic viability of the definitions, inference rules and axioms employed in the proof. Or, rejecting this approach and adopting the second alternative (B), one accepts a *non-explicit* definition of the totality of natural numbers (a recursive definition) and eschews a formal derivation of induction in favor of a more "intuitive" epistemic motivation for its legitimacy in arithmetic. The first alternative splits

[31]Shoenfield proves the same theorem in a different way; his (1974) paper is the standard reference to the theorem in the literature although it by no means has priority. Schoenfield also shows that the identity relation is only impredicatively definable.

into two sub-cases, which are distinguished according to whether the explicit definition of natural number used in the proof of induction is predicative or impredicative. Understanding the impredicativity implicated in the justification of induction in both these sub-cases of alternative A is instrumental to seeing the existence of the impredicativity concealed in the less formal approach, B.

The totality of natural numbers was first defined last century by Dedekind and Frege, both of whom sought to transform the burgeoning programme of arithmetization into logicism, the idea that ultimately all the principles of mathematics were not arithmetical but logical in character. Arithmetic itself, according to this view, was derivable in logic alone from appropriate explicit definitions of the fundamental concepts, such as natural number. Dedekind and Frege independently succeeded in explicitly defining the property of being a natural number, and their definitions, which are equivalent, provide examples of the first sub-case of alternative A mentioned above. In contemporary symbolism, one may express their definition as follows:

$$Nx =: \forall F(F0 \,\&\, \forall y(Fy \rightarrow F\mathcal{S}y) . \rightarrow Fx)$$

Here "0" denotes zero and "*S*" denotes the one place function "the successor of"; the exact definitions of these expressions are not important here. From this definition, mathematical induction is easy to derive. If, therefore, as Frege and Couturat maintained, this definition is permissible within the confines of a logic whose axioms are analytic, then induction is provable on analytic principles alone. This view would garner little support today, since the logic underlying the definition is second-order (and must be in order that the proof of induction, in its full generality, may go ahead); and few would claim that second-order logic is analytic. For among the values of the bound variable "F" are properties which would require, for their specification, use of the defined term "N". This shows not just the indispensable role of second-order quantification in the intended proof of induction on the basis of the Frege-Dedekind definition, but also the impredicativity inherent in this logicist justification of induction. The property N is defined in part by the use of a bound variable which includes in its range not only N itself but other properties whose specification requires reference to N. Any attempt to remove such properties from the range of the bound "F" will lead to an incomplete justification of induction.

Poincaré was the first to point out the impredicativity of this justification of induction, and he concluded that the justification was illegitimate. If so, support was removed from beneath the claims Frege and Couturat wished to make on the basis of this justification, including the claim that induction could be proved analytically. On the face of it, however, logicists need not yet abandon the hope of deriving induction from principles of logic alone, even if they agree (as Russell was to do) with Poincaré's premise that impredicativity is illegitimate. For the first alternative A includes, as a sub-case, the possibility of appeal to a *predicative* explicit definition of the natural numbers. Russell's repeated attempts to derive induction in ramified type theory (1908, 1910, 1925) are examples of this second sub-case. The failure of his attempts is shown by Fitch and Schoenfield, and seems to have been known to Wang as well. Recently A. George (1987) has given the matter a nice formulation. Correcting an attempt Quine (1969) made to state an explicit predicative definition of natural number, George offers the following definition:

$$Nx =: \forall\alpha(x\in\alpha \ \& \ \forall y(Sy\in\alpha \rightarrow y\in\alpha).\rightarrow 0\in\alpha).\ \&.\ \exists\alpha(x\in\alpha \ \& \ \forall y(Sy\in\alpha \rightarrow y\in\alpha))$$

Here "α" ranges over finite sets only (at least one of each size), so the definition is predicative. As George points out, however, the justification of induction for an arbitrary predicate "Rx" in the language on the basis of this predicative definition is still impredicative. Such a justification would proceed by considering a set $\beta=\{x\in\gamma: \neg Rx\}$, where γ satisfies the bound existential variable in the second conjunct of the above definition. The problem he points out is that β will in general be impredicative, since some instances of "Rx" will contain bound variables "α" ranging over β itself. If this is not permitted the justification of induction for such "Rx" will not go through. Thus both sub-cases of the first broad alternative A for the justification of induction make indispensable use of impredicative definitions. Logicism, if it is to be viable at all, must go impredicative.

It was precisely this fact which Poincaré wished to exploit to refute logicism: if the viability of logicism depends upon its acceptance of impredicative definitions, and these are illegitimate, then logicism itself is not viable. Moreover, his argument to the effect that logicism depends upon the admission of impredicative definitions was not limited to a consideration of the first sub-case alone. This is a point the historical literature on Poincaré misses. Poincaré knew of, or at least suspected, the existence of predicative explicit definitions of the natural numbers.[32] This is of great importance for the interpretation of Poincaré's foundational program, for Poincaré is sometimes thought to have objected to logicism merely by arguing that the Frege-Dedekind definition of natural numbers is illegitimate. If so, the existence of predicative explicit definitions effectively thwarts his attack. The fact is, however, that Poincaré *concedes it is possible explicitly to define natural number*. It is rather the justification of induction on the basis of such a definition which introduces the alleged vicious circularity. This circularity is thought to consist in the need to make use of an impredicative definition in this justification. In other words, Poincaré's objection to logicism rests not just on the impredicativity of the Frege-Dedekind definition of natural number (sub-case 1 of A), but equally on the impredicativity George's exposition brings out (sub-case 2 of A).

[32]See (1905) pages 835, 303, 308-9, 867-8; compare (1905 32) with the version published in (1908).

Poincaré concludes from this dual appearance of impredicativity that induction does not *follow analytically* from the concept of natural number. Rather, it follows *synthetically* from a prior intuition.[33] After (1906), it was on these grounds that Poincaré condemned logicism. Logicism, he thought, illegitimately extended logic into the impredicative. The result was paradox and contradiction (1905 316; 1908 154).

The illegitimacy of impredicativity, on Poincaré's view, forced a restriction of the devices of logic (including definition) to the predicative. On this conception, however, a "logical" proof of induction will not be forthcoming. Without this, Poincaré argued, we must accept the second broad alternative B sketched above for the justification of induction, namely an "intuitive" justification on the basis of a recursive definition of the natural numbers. This, as Poincaré understood it, implied a return to Kantian foundations, for the possibility of offering a recursive definition of anything, let alone the natural numbers, depended upon our ability indefinitely to repeat certain act-types (such as counting and body motion) and this ability was considered necessary to experience as we knew it. Now it turns out that the failure of a recursive definition of the natural numbers to yield induction "deductively" (or, as Poincaré would put it, "analytically") arises from a kind of impredicativity implicit in certain instances of induction. Poincaré wanted these instances as much as he wanted any others, and if he wants them, I am prepared to grant them to him. But the cost is that his justification of induction (the "intuition du nombre pur" in the sense of his thesis *iii*) is irremediably impredicative. To see this, I will now review the justification of induction on the basis of a recursive definition.

[33]Similarly, Poincaré considers several proofs of Bernstein's theorem, all of which are impredicative. He concludes not that the theorem is false or unsupported, but that it is legitimate *and* based on synthetic *a priori* intuition.

It is a commonplace that induction is not strictly derivable from a recursive definition of the natural numbers. In such a definition, one supposes initially that 1) zero is a natural number and that 2) the successor of any natural number is itself a natural number. The justification of induction stems from the so-called "extremal" clause in this definition, which states that something is a natural number only in virtue of these two "initial" assumptions. To see how this justification is supposed to work, suppose now that we have a predicate "F" in the language of arithmetic for which we know that 3) F0 and that 4) if Fn, then FSn, where n is recognized to be a natural number. The principle of mathematical induction concludes that 5) "Fx" is true of *all* natural numbers x. License for this conclusion is clearly the function of the extremal clause, for falsity of the instance of induction in question would consist in there being a natural number m for which "Fm" fails, whereas the extremal clause seems to prevent there being any such m. By (3), such an m could not be 0, and so by the extremal clause, m is a natural number in virtue of clause (2) of the recursive definition. Thus m is the successor of some natural number n. It is clear that if m were the least natural number for which "Fm" fails, "Fn" would be true; but then from (4) it would follow that FSn, which is to say Fm, contrary to hypothesis. Thus the "intuitive" justification of induction (5) on the basis of a recursive definition will go ahead so long as we are permitted to assume m is the least natural number for which "Fm" fails. The extremal clause is again relied upon here, for the assumption that m is least is permissible only on the condition that there is no infinitely descending sequence of natural numbers, and this condition is intuitively fulfilled if the extremal clause is true. Like the bound second-order variable in the Frege-Dedekind definition, the extremal clause has the function of keeping to a minimum the entities satisfying the definition. Given this (ordinal) minimality, falsification of induction on "F" would require there to be a natural number m which is neither 0 nor the successor of any natural number. m would thus have to be a natural number, but not in virtue of the initial suppositions (1) and (2), contrary to the extremal clause (3). This motivation does not amount to a deductive proof, but the truth of induction is "immediately evident" if the initial assumptions and the extremal clause are.

This justification of induction is evidently the one Poincaré accepted since, as I have argued above, he took our "intuition du nombre pur" to be captured or expressed by a recursive definition and insisted that induction followed synthetically from this intuition. But instances of induction like those which give rise to the charge of impredicativity in alternative A give rise again to a charge of impredicativity here. In discussing the Frege-Dedekind definition, Poincaré (1905 309-10) singles out for special scrutiny instances of induction on predicates "Fx" which contain as part the predicate "Nx" to be defined. These predicates, in his view, by restricting bound variables to the natural numbers, presume that the notion of natural number is already on hand. In this he sees a circularity, but he is quite clear that the instances of induction in question are perfectly legitimate: only their formal derivation is not. Their legitimacy, he thinks, is made manifest by the "intuitive" justification just given, even though the quantification involved in these predicates is precisely the same. One is therefore forced to ask why the same use of quantification is legitimate in an informal setting, but illegitimate in a formal setting. Why does the use of bound variables restricted to natural numbers presume the notion of natural number is already on hand in the explicit derivation of induction but fail to do so in the non-explicit derivation? Or why does presumption constitute a vicious circularity in the one case but not in the other?

Poincaré remains strangely silent on this question. Unfortunately, the difficulty also threaten the coherence of his Kantian program in the foundations. Experience as we know it is possible, according to Poincaré, only because we have the capacity indefinitely to repeat certain act-types. It is therefore natural to ask what the precise content is of the synthetic *a priori* "intuition du nombre pur" which has this function of rendering experience possible? The Kantian programme in the foundation of mathematics must surely not arrive at the point where it says: something we know makes experience possible, but this cannot be stated. One might just as well say: we can explain the possibility of experience, but this explanation cannot be given. It may well be that a certain type of inadequacy will affect our expression of the specific content of underlying intuition which renders experience possible, so that a complete statement of this content is impossible; but surely the attempt itself to express this content in an admittedly incomplete way cannot be entirely futile. Accepting this, however, one sees that the only content of Poincaré's "intuition du nombre pur" which could play the role of rendering experience possible is precisely the impredicative content.

This charge, that the seat of the explanatory power of Poincaré's basic intuition is the impredicative content of that intuition, renders Poincaré's Kantian foundation incoherent. Yet it appears to follow from positions he allows are true. He admits that induction has impredicative instances (that is to say, instances where the predicate in question contains the predicate Nx). It is on the basis of this alone that he rejects the logicist justification of induction which proceeds from an explicit predicative definition of natural number. He admits as well that induction restricted to its predicative instances is an inadequate foundation for arithmetic. A model not satisfying induction, in its full generality, is not, on his view, a model of arithmetic. The predicative instances of induction, however, are evidently justifiable on logical grounds alone, for these do not raise any of the difficulties Poincaré tried to put in the way of the explicit proofs of induction. Obviously the possibility of experience cannot depend only on that part of the content of the underlying intuition which is merely logical, for the Kantian doctrine is precisely that the synthetic *a priori* content does this. Poincaré is left with no choice: the synthetic *a priori* content of the "intuition du nombre pur," which alone renders experience possible, is precisely the impredicative content of the justification of induction he favors. On his assumptions, the impredicative content of thesis *iii* can alone fulfill the Kantian task of rendering experience possible.

The consequence is that, even for Poincaré, impredicativity can't be all bad. This, however, runs contrary to his assertion that impredicativity is indicative of a *petitio* or vicious circularity. I conclude that there is a fundamental incoherence in the conjunction of two of Poincaré's principles: the principle which pronounced impredicativity illegitimate and the neo-Kantian view that arithmetic is based on a specific intuition into numbers satisfying mathematical induction. Ultimately, Poincaré must make a choice between these two principles. Interestingly, it was Poincaré's subjectivism regarding mathematical entities which motivated him to accept both these principles. The recursive definition of natural numbers he favored was understood as a set of instructions or rules for the "creation" of natural numbers, and the vicious circle principle is defended by citing limits on our power to "create" objects. It seems to me that the fundamental incoherence of Poincaré's two principles suggests a problem with his subjectivism about mathematical objects. But this suggestion will have to be developed elsewhere. I turn now to the hard choice Poincaré must face in the foundation of analysis.

2.2 The Changing Place of Continuity

I turn now to Poincaré's views about the continuum. As is to be expected, the notion of continuity, in Poincaré's view, is fundamental to geometry; for, as was seen above, geometry for him "is nothing but the study of formal properties of a certain continuous group" (1898 41). Now, since the notion of this and other continuous groups is part of the "forme de notre entendement," so too, apparently, is the notion of continuity itself. In this section I will try to show that this is indeed the view Poincaré held. At issue is the classical notion of mathematical continuity as defined in Sec. 0.3, which I will call "continuity in the mathematical sense," or simply "mathematical continuity." It is useful to have these terms available at the outset for the purposes of context-setting; but it will not be until Sec. 2.2.2 that I come discuss Poincaré's own account of our knowledge of continuity mathematical sense, which consists in a dialectical construction of a mathematical continuum. In the next section, I will look at some preliminary questions regarding the philosophical context in which Poincaré's construction is to take place.

2.2.1 Convention and the Continuity of Actual Space

Space is a group, Poincaré says, and geometry is the study of this group. The idea here, quite simply, is that one can point to certain group-theoretical facts to distinguish different metrical geometries, such as the various non-euclidean geometries. Poincaré believed that a complicated series of empirical inferences and conventions would lead us to the conclusion that actual space, understood as a continuous group of displacements, is euclidean. We could have adopted other conventions, but the ones we chose are, under our experiential conditions, the most convenient. That we could have gotten by, under our experiential conditions, with "non-euclidean" conventions shows, according to Poincaré, that there is no intrinsic metric of actual space: precisely measurable distances do not objectively exist. Other properties of space objectively exist, but precise metrical properties do not. The conclusion that actual space is euclidean, therefore, is to be taken with an important qualification: we do not conclude that euclidean geometry "est la géométrie la plus vraie, mais [qu'elle] est la plus *commode*" (1895 645). I will not go into Poincaré's arguments for this view, but he held a precisely similar position with respect to the number of dimensions of actual space: this was decided by a series of empirical inferences and so-called conventions, and we could have made the convention, even under identical experiential conditions, that space has more or fewer dimensions than three (the number Poincaré believe to be "la plus commode"). There is no fact to the matter regarding the dimensionality of actual space. In general, Poincaré holds that there is no fact to the matter underlying any convention, for conventions are not strictly true or false, but merely convenient.

The issue of convention is notoriously difficult in Poincaré. In a late paper, Poincaré expanded on its significance:

> ce mot de commode n'est peut-être pas ici assez fort; un être qui aurait attribué à l'espace deux ou quatre dimensions se serait trouvé dans un monde comme la nôtre, en état d'infériorité dans la lutte pour la vie (1912 498).

The idea that conventions are naturally selected for, that what is convenient is that which promotes differential reproductive success, raises problems that are difficult to answer. On the one hand, it begins to make sense of Poincaré's strange claim that some conventions (including those pertaining to the metric of space and to its dimensionality) are *non-arbitrary* (cf. 1912 503). On the other hand, there is no reason to think that conventions in the sense of evolutionarily selected-for beliefs fail to have a truth-value. Perhaps Poincaré has an argument here, but he does not state it. The problem gets worse in (1912) where, as we shall see, Poincaré introduces a new synthetic *a priori* intuition into mathematical continuity, and seems to assimilate it to evolved conventions as well, even though such intuitions clearly have a truth value according to him. My own opinion is that Poincaré's extant writings are confused on this issue, and the best we can do is recall at all times that Poincaré makes competing demands on his notion of non-arbitrary convention. He assimilates convention to evolved belief, and never faces up to the question whether they therefore do, after all, have truth-values.

In spite of the difficulties affecting the notion of convention, one must ask the question: is actual space, on Poincaré's view, continuous in the mathematical sense by a convention? This question will arise again later, but if we take it for the moment to be the question whether or not there is a fact to the matter regarding the continuity of actual space, then I think Poincaré's answer will be that there is such a fact to the matter. The conventionality of the metric of space and of the number of its dimensions imply, for Poincaré, that actual space has no intrinsic metric and no intrinsic number of dimensions. But the situation with continuity is otherwise. Theories of actual space are possible according to which space is not continuous in the mathematical sense[34] (1895, 1898). These, however, have been falsified by experience. (See (1898 15-6) for the "experiment" which shows it to be true.) Had experience been otherwise, we might have been led to favor such a theory of space, but in point of fact this has not happened. The claim that actual space is continuous in the mathematical sense is an empirical claim: we know it to be true, but we know it without certainty. I bring this up now because we shall see later that other aspects of Poincaré's views suggest that he did in fact consider space to be mathematically continuous *by convention.* My point here is that, if so, the competing demands he made on his notion of convention come into conflict here, since there can be little doubt that

[34]On the phrase "continuous in the mathematical sense" see Sec. 0.3. The phrase is meant to capture Poincaré's feeling that classical continuity alone is somehow *true* continuity.

he thought there was a fact to the matter regarding the continuity of actual space.

So actual space is continuous in the mathematical sense, but this is typically thought of by Poincaré as a contingent fact (1898 15-6), which we infer for empirical reasons. Geometry supposes the notion of continuity to be on hand, and the natural question is: where do we obtain this notion? Again, there is some indication that this notion is part of the resources of the pure understanding, in that Poincaré does say the notion of a continuous group is prior to all experience (1898 41), and that the mathematical continuum, he says, "a été créée de toutes pièces par l'esprit" (1893 30). What is required, therefore, is a brief survey of Poincaré's account of the "construction" or "creation" of the mathematical continuum. But there are still several unanswered questions concerning the overall place of mathematical continuity in Poincaré's foundational thinking. In particular, even if we obtain the notion of mathematical continuity from synthetic *a priori* intuition, the question remains as to the *content* of this intuition. Is it a special intuition into mathematical continuity itself, or does the arithmetical "intuition du nombre pur" already suffice? The answer to this question depends in turn on the success of the arithmetization of analysis. We have seen that, early on, Poincaré accepted the success of this arithmetization, but in a far later paper he seems to backpedal somewhat. It will be useful to map out the alternatives in Poincaré's mind before turning to the details of his "construction" of the mathematical continuum, the interpretation of which depends upon the supposed content of the synthetic *a priori* intuitions at work.

2.2.1.1 Arithmetization: Early Optimism

It is important to see that, for Poincaré, precisely the same notion of mathematical continuity underlies three separate mathematical sciences: geometry, analysis, and Analysis Situs, or topology. "L'espace géométrique" is continuous in the mathematical sense (1898 14-6), as is the so-called "espace amorphe" studied by topology. Similarly, as I indicate later, it is indubitable that on Poincaré's view the real numbers form a mathematical continuum. Clearly, then, the notion of continuity in the mathematical sense is part of a common foundation for all three sciences. But analysis and Analysis Situs seem to be more directly *about* mathematical continuity than geometry is (cf. 1887 79; 1903 28); for Poincaré sometimes suggests that geometry presupposes this notion only in so far as it is concerned with actual space, which we know only empirically to be continuous in the mathematical sense. The question of which of these other two sciences is itself properly the study of mathematical continuity (as well as the subordinate question whether "points" or "numbers" are the elements of the mathematical continuum) need not be answered, since Poincaré seems not to care (cf. 1893 28; 1898 40). For example, he says at one place that the theorems of topology are among the most beautiful known to the "analyst pur" (cf. 1912 484), and in (1893 26, 31) he indifferently attributes the notion of mathematical continuity to the "analyst pur" and the "géomètre pur." Similarly, the domain of "l'intuition géométrique" which Poincaré invokes late in life is not geometry but Analysis Situs. My own feeling is that, while he accepted the arithmetization of analysis without reserve, Poincaré tended to look on analysis as the true home of the notion of mathematical continuity; but later, after he began to feel reservations, he preferred topology.[35]

[35]Poincaré was one of the founders of modern topology, but he did not conceive of the discipline in anything like the general way we do today. While he considered topology to be concerned with certain properties of spaces, construed as point-sets, the spaces in question were restricted to those whose points were continuously ordered. Since nothing else was assumed about these spaces (e.g., no metric was defined for them), Poincaré understood topology to be in some sense about the notion of continuity in its simplest and most unencumbered form. This is implicit in some of the quotations given later, such as (1912 185, 187).

Early in his career, Poincaré thought that analysis had been perfectly arithmetized:

> dans l'Analyse d'auhourd'hui, quand on veu se donner la peine d'être rigoreux, il n'y a plus que des yllogismes ou des appels à cette intuition du nombre pur (1900 122).

Thus our pure or *a priori* intuition into number justified not only mathematical induction but analysis, including the important least upper bound theorem, which shows, roughly, that the real numbers are continuous in the mathematical sense (see Sec. 0.3). The "syllogisms" of mere logic were needed for this justification, but they added nothing since "c'est surtout en Logique que rien ne se tire de rien" (1887 79). Not logic, according to this view, but the categorial intuition into pure natural number accounts for our understanding of continuity in analysis. Thus analysis does not stand alone: it is built from foreign resources with imported labor. The resources are our capacity indefinitely to repeat act-types such as counting (theses *i* and *ii*); the imported labor is the synthetic justification of induction (given in thesis *iii*):

> L'idée vague de continuité, que nous devions à l'intuition, s'est résolue en un système compliqué d'inégalités portant sur des nombres entiers.... Les Mathématiques, comme on l'a dit, se sont arithmetisées" (1900 120).

Notice that Poincaré here says we obtain a "vague idea" of continuity from intuition, and then "resolve" this into "un système compliqué d'inégalités" concerning natural numbers (*i.e.*, Dedekind cuts). The "intuition" intended here is obviously not synthetic *a priori*, since it is called vague. Probably the word is being used in the sense of sensation, or Kantian "empirical intuition" since, as we shall see, sensation is the starting point of Poincaré's dialectical construction of mathematical continuity. More important for present purposes is the implication that specification of the "système compliqué" involves no non-arithmetical notions. We know today, however, that this claim is false. The use of sets of natural numbers to represent real numbers, and the proof of the mathematical continuity of the reals in their natural ordering, employs means that go beyond what is fairly called arithmetical. The great arithmetization attempted in the 19th century has failed. In (1900), however, Poincaré was still riding high.

The philosophical consequences of the perceived success of the arithmetization of analysis were enormous. According to Poincaré, arithmetization showed that the "conception ordinaire" of continuity employed by "metaphysciens" was "toute autre chose" than that constructed by the "analyst pur". Speaking of the continuity conceived as in analysis, Poincaré says:

> Le continu ainsi conçu n'est qu'une collection d'individus rangés dans un certain ordre, en nombre infini, il est vrai, mais extérieurs les un aux autres. Ce n'est pas là la conception ordinaire, où l'on suppose entre les éléments du continu une sorte de lien intime qui en fait un tout, où le point ne préexiste pas à la ligne, mais la ligne au point. De la célèbre formule, le continu est l'unité dans la multiplicité, la multiplicité seule subsiste, l'unité a disparu (1893 26-27).

It is useful to compare the metaphysical views maligned here to those the young Russell would soon hold and publish. Russell found in the foundations of geometry an antinomy regarding the notion of a point, and generalized this to the Hegelian "contradiction of relativity" which he thought inflicted any continuum. These "contradictions" were understood in such a way as to imply something quite like "the celebrated formula" Poincaré indicates. "Projective Geometry," Russell says, "is founded on the possibility of experiencing diversity in relation, or multiplicity in unity" (1897 146; cf. 136, 181 ff.). Russell concluded that continua were in one sense not "wholes," for they were not composed of simple parts, but their elements nevertheless possessed "une sorte de lien intime qui en fait un tout." The "lien intime" among elements of continua consisted in the reciprocal ontological dependency which obtained among them (cf. principle (4) of Sec. 1.7.1.1). This dependency, essentiality, threatened according to Russell the very notion of the substantive point, and for this reason he saw the notion of point as self-contradictory. He concluded that "straight lines and planes are the true spatial units" (1897 193).

Poincaré says "le point ne préexiste pas à la ligne, mais la ligne au point." The views Poincaré criticizes here significantly overlap with those Russell later held, and the overlap is not accidental. The "celebrated formula" is a veiled reference to Hegel, and precisely Hegel's views were adopted by Russell.

Evidently, then, Poincaré took the supposed success of the arithmetization of analysis to refute not only Kant's view of space, but also Hegel's. Kant was wrong to think that the form of spatial sensation gave rise to a primitive intuition into continuity (Ausdehnung [**A**20-1/**B**35]), and Hegel was wrong to take Zeno's paradoxes and Kant's antinomies of reason as "contradictions" actually obtaining in nature. Such "contradictions," judging at least from Russell, consisted in the reciprocal ontological dependency among the elements of continua. Poincaré, on the other hand, seems to believe that the elements of a continuum exist independently of one another. It is evidently this belief he intends to express when he says the elements of a continuum are "extérieurs les un aux autres" and that they possess no "lien intime" which makes them into a "whole". He sums up his view by denying the "celebrated formula", which says that the continuum is "unité dans la multiplicité." According to the early Poincaré, "l'unité a disparu" from this formula. Although the elements are arranged in a "certain ordre" they do not form any special "whole" or possess any special "unity" in virtue of this. As we shall see, however, Poincaré later shifts his position and argues for the importance of some kind of "unity" in our understanding of the "certain ordre" in question. This late unity is thought to imply that the elements of the continuum do after all form a "whole" in some robust sense, thought he continues to deny the elements of the continuum depend upon one another for their existence. Acceptance of this sense of unity (whatever it happens to consist in) will require that Poincaré withdraw his categorical denial of the "celebrated formula." At this early stage, however, Poincaré still thinks of the Hegelian view of the continuum as way off base.

2.2.1.2 Arithmetization: Late Doubts

There is considerable evidence that Poincaré backed away from some of his early hopes for arithmetization after having raised the problem of impredicativity in 1906. Initially he raised this problem to obstruct the logicist proof of mathematical induction. Of course, if induction was purely logical and the arithmetization was a success, continuity would also fall to the logicists. Some time after 1906, however, Poincaré began to sense problems with the arithmetization he had so firmly believed in, and he began to speak about "l'intuition géométrique". Now Poincaré may in fact be returning here to a view he held very early on, for in (1887 90), he wrote: "On peut montre que l'Analyses repose sur un certain nombre de jugements synthetique *a priori*". The statement, however, is inconclusive, since a "certain number" might well be one, which in the context would imply that mathematical induction alone was required for analysis and continuity. More likely, however, Poincaré had not quite decided yet that the arithmetization of analysis had been a success. It would take him only a few years to do so, but by (1912 486) he was again having doubts: "Je ne veux pas dire que cette «arithmétisation» des mathématiques soit une mauvais chose, je dis qu'elle n'est pas tout."

The problem with arithmetization, Poincaré came to feel, is that it leads us to accept, as entirely satisfactory, what he calls the "définition analytique" of a continuum of *n* dimensions. This definition runs as follows: a continuum of *n* dimensions is

> un ensemble de *n* quantités susceptibles de varier *independamment* l'une de l'autre et de prendre toutes les valeurs réelles satisfaisant à certaines inégalités (1912 486).

It goes without saying, perhaps, that this definition is imprecise and formally inadequate. But it is important to see that Poincaré's objections were not based on formal considerations. Rather, he thought of the definition as philosophically misleading:

> Cette définition, irréprochable au point du vue mathématique, ne saurait pourtant nous satisfaire entièrement. Dans un continu les diverses coordonnées ne sont pas pout ainsi dire juxtaposées les unes aux autres, elles sont liées entre elles de façon à former les divers aspects d'un tout (1912 486-7).

This "analytic" definition fails to incorporate certain intuitively evident features of the mathematical continuum. It leaves unstated the fact that the "coordonnées" are "liées entre elles" and form "aspects of a whole" which is the continuum. Although it is by no means clear precisely what Poincaré has in mind here, the similar language ("lien" and "tout") in the early and late passages is arguably significant. It suggests that Poincaré's late hesitation regarding arithmetization led him to reconsider, and partially at least to withdraw, his early denial of the "lien intime" among the elements of a continuum which "les métaphysiciens" had insisted upon.

This suggestion can be confirmed if we restrict attention to the special case of the 1-dimensional continuum. According to the "analytic" definition, a 1-dimensional continuum is the singleton of a single "variable quantity" taking all real numbers as values. Poincaré expresses the philosophical inadequacy of this definition by saying that the "coordonnées" involved are not merely juxtaposed but "liées entre elles." But what are the "coordonnées" involved here? Properly speaking, coordinates are *n*-tuples of numbers which indicate the position of a point in space. The coordinates here are therefore singletons of real numbers. But Poincaré is no doubt following the familiar convention of identifying coordinates (in general, *n*-tuples) with the positions they indicate. Assuming this, Poincaré's difficulty might better be expressed as follows: the positions of points on our 1-dimensional continuum are not merely juxtaposed (as the "analytic" definition suggests) but "liées entre elles." Evidently, some philosophically essential fact about the ordering of points along a line is left out of the "analytic" definition. Indeed, all the definition says is that a 1-dimensional continuum is the singleton of a "variable quantity" which takes all real numbers as its values. The ordering of the points composing the continuum then reduces to the natural ordering of the real numbers by magnitude. These too are not merely juxtaposed but "liées entre elles." The philosophical inadequacy of the "analytic" definition therefore comes to this: nothing is said in this definition about the underlying natural ordering according to magnitude of the real numbers.

The difficulty can be put in another way. The traditional notion of a "variable quantity", even on its own terms, has little content unless one can specify precisely the values it can take. In this case, those values are all the real numbers. But what does it mean to speak of *all* real numbers? Where do we derive this notion? A similar question was raised in arithmetic. What does it mean to speak of all natural numbers? In Poincaré's understanding, it is only after we have accepted his thesis *iii*, after we have intuited or seen the validity of instances of mathematical induction, that we may meaningfully speak of "all natural numbers." Now mathematical induction can be viewed as the characteristic ordering theorem for the natural numbers. Poincaré's late answer to the question whence we derive the notion of all real numbers follows his views in arithmetic quite closely. Like the natural numbers, the real numbers are given to us originally in a natural ordering, namely the ordering according to magnitude. It is only after we have seen or intuited their order-type that we may meaningfully speak of "all real numbers." Now the characteristic ordering theorem for the real numbers is the least upper bound theorem, which in its geometric form is sometimes called the Vollstandigkeitsaxiom. It is undeniable that Poincaré accepted this theorem as true. All that changes late in his life is the content of the synthetic *a priori* intuition which underlies our understanding of mathematical continuity. Rather than construct this continuum with purely arithmetical resources, Poincaré began to look on mathematical continuity as the object of a special primitive intuition. As we shall see, both interpretations are compatible with his early dialectical construction of a mathematical continuum. For the moment, however, I wish to return to (1912) and interpret more fully the philosophical implications of the inadequacy of the "analytic" definition of mathematical continua of n dimensions.

The failure of the "analytic" definition to provide any information concerning the order-type of the real numbers is expressed by Poincaré in terms he also used in (1893). The definition is thought to leave unspecified how the real numbers, or more generally, how the elements of a mathematical continuum, are "liées entre elles" so as to form "divers aspects d'un tout." The failure to do so "fait bon marché de l'origine intuitive de la notion de continu, et de toutes les richesses que recèle cette notion." Notice here, once more, that the notion of continuity is said to be intuitive, but this time it is evident from the context that Poincaré is referring to his newly introduced "intuition géométrique." The precise sense in which this intuition is synthetic *a priori* is best left to the side for a moment. For the present, simply note what it is that the "richesses" of this intuition are thought to consist in. Unlike the "analytic" definition, it reveals to us the *wholeness* of the continuum which arises from its elements being "liées entre elles" in a special way. Geometric intuition shows us that the mathematical continuum is, in some rich sense, a whole. This is evidently a withdrawal of Poincaré's early denial that some "lien intime" among the elements of the continuum made the latter into a *whole* ("tout") in some rich metaphysical sense. On the basis of this denial, Poincaré rejected what he called "la conception ordinaire" of the continuum along with the celebrated formula of "les métaphysiciens." The unity, he said, had disappeared from the hackneyed phrase that the continuum was "unité dans la multiplicite." By (1912), all this had changed. Now the "aspect of the whole" is said to be missing left out of a *mathematically* adequate definition, and it is restored by appeal to a non-arithmetical intuition. The existence of a specifically geometric intuition must be accepted after all, on Poincaré's final view, for otherwise we cannot account for the interrelation among the elements which consists in their being continuously ordered. The unity of the mathematical continuum has reappeared, and with it some interpretation at least of the celebrated formula must be countenanced.

To be satisfied with the "analytic" definition, Poincaré says, is to make the mistake of replacing "l'objet à définir et la notion intuitive de cet objet par une construction faite avec des matériaux plus simples". Now Poincaré is quick to point out that *he does not mean* that the correct construction of the mathematical continuum requires "materials" as complex as the continuum itself. He admits, on the contrary, that the "materials" needed are after all simpler. Unfortunately, he does not offer a criterion of simplicity. One necessary condition of simplicity which Russell accepted, however, applies perfectly. For Russell, simple substances could not be essentially related to one another. Similarly, Poincaré, despite his weakening faith in arithmetization, continues to consider the elements of the continuum as existing independently of one another. As he puts it, the elements are "exterior to" and "absolument distinct" from one another (1912 489; 1893 27). In fact, this view is, so to speak, the null hypothesis, and so in an additional sense the "simplest" assumption to make. Thus the problem with the "analytic" definition is not that the "materials" used to construct the continuum are in no sense simpler than the continuum itself. Rather, the problem with this definition is that the "construction" itself out of these materials is not specified. The mistake it makes is to leave unstated the manner in which the elements form a whole: the "certain ordre" in which the elements occur in a continuum is not articulated by the purported definition.

In sum, then, quite late in his career Poincaré began to believe that the arithmetization of analysis could deceive us into thinking that we understood the order-type of the real numbers when we did not. Understanding this order-type was tantamount to understanding the "unity" of the continuum which Poincaré earlier either ignored or failed to notice. Evidently, then, the unity of the continuum can be expressed as some fact about the natural ordering of the real numbers. This fact is essential to a philosophically adequate definition of the continuum and so is presumably constitutive of the very notion of the real numbers themselves. Arithmetization is not a bad thing, but it is not the whole story either, for it passes over this constitutive fact in silence.

If arithmetization is not the whole story, some new intuition besides the one into pure number is needed to explain our understanding of the order-type of the real numbers. "Nous ... tirons la notion du continu à *n* dimensions, non de la définition analytique précitée, mais de je ne sais quelle source plus profonde" (1912 187). This more profound source Poincaré calls the "intuition géométrique," and "le véritable domaine de l'intuition géométrique" is Analysis Situs. Analysis Situs, or topology, makes no use at all of the notion of quantity, and is thus the "purely qualitative geometry," contrary to the early Russell, who had reserved this honor for projective geometry. Poincaré writes:

> L'espace, considéré indépendamment de nos instruments de mesure, n'a donc ni propriété métrique, ni propriété projective; il n'a que des propriétés toplopgiques (c'est-à-dire de celles qu'étudie l'Analysis Situs). Il est *amorphe* (1912 485).

True "intuition géométrique" allows us to understand the amorphous continuum of topology. This continuum is amorphous with respect to metrical and projective properties, but it is nevertheless "qu'une collection d'individus rangés dans *un certain ordre*, en nombre infini" (1893 26). Our intuition allows us to recognize under what conditions two spaces are topologically isomorphic, namely precisely when there exists a *continuous* 1-1 correspondence between the elements of the two spaces. The requirement that the function be continuous is essential, and is part of the reason Poincaré says the object of this new intuition is continuity itself. It was this new intuition which was to take up the slack caused by the hesitation Poincaré had begun to feel regarding the arithmetization of analysis, and restore to the content of our notion of continuity the sense of unity. It is this intuition which is invoked to express what the "analytic" definition failed to express, namely the natural ordering of the real numbers. Our question is how well it can succeed.

2.2.1.3 The Kantian Credentials of Geometric Intuition

The continuity which is the object of true geometric intuition acts as a "fond commun" (1903 281) for the various geometries (projective and metric, euclidean and non-euclidean) which can be constructed upon it. But it is not yet clear that this intuition is "pure," *i.e.*, that it is a synthetic *a priori* intuition. The issue does not concern merely the rough and partial gloss given above, but also the more traditional and robust Kantian sense. The key issue (since we are no longer concerned with time) is whether experience as we know it presupposes, or would be impossible without, the topological notion of continuity. "A propos des théoremes de l'Analysis Situs", Poincaré asks just the right questions:

> Peuvents-ils être obtenus par un raisonnement déductif? Sont-ce des conventions déguisées? Sont-ce des vérités expérimentales? Sont-ils les caractères d'une forme imposée soit à notre sensibilité, soit à notre entendement? (1903 285)

Poincaré raises these questions in (1903) but does not answer them there. This is due to the fact that he is there concerned mainly with the number of dimensions of space. The possible answers to the present question are restricted, however, by Poincaré's views about certainty and aprioricity.

The truths of topology are known with certainty, so they must be known *a priori*. They cannot be "des verités expérimentales." Yet they cannot be obtained by deductive reasoning alone, since then they would be empty of content. Moreover, since they are indeed true, they cannot be the result of mere convention in the way the number of dimensions is. This leaves only the Kantian option. Poincaré writes:

> Je conclurai que nous avons tous en nous l'intuition du continu d'un nombre quelconque de dimensions, parce que nous avons la faculté de construire un continu physique et mathématique; que cette faculté préexiste en nous à toute expérience parce que sans elle, l'expérience proprement dite serait impossible et se réduirait à des sensations brutes, impropres à toutes organization, que cette intuition n'est que la conscience que nous avons de cette faculté (1912 504).

Geometric intuition is indeed pure. Without it, experience "proprement dite" would be impossible. It is categorial, and provides a foundation not for topology alone, but for analysis and (in part) for geometry as well. These sciences too are embedded by Poincaré in a "surrounding Kantian context." The inadequacies of the arithmetization of mathematics show, according to Poincaré, that a new categorial synthetic *a priori* intuition must be called upon in the foundation of those fields which depend on the notion of continuity in the mathematical sense. And just as arithmetical intuition (formulatable as the principle of mathematical induction) rests upon our capacity to "create" the natural numbers, so too here geometric intuition rests upon our ability to "create" a mathematical continuum.

I mentioned that the theorems of topology resting on this intuition were considered by Poincaré to be true. Thus they could not be the result of mere convention in the way that the metric of space was. But strangely enough, Poincaré seems to allow in (1912) that these theorems, despite being based on a categorial intuition which renders experience possible, were at the same time due to non-arbitrary convention. Apparently the form of our experience has been selected for. To see this, one must realize first that in his review (1902) of the first edition (1899) of Hilbert's *Grundlagen der Geometrie*, Poincaré singles out the so-called "Axiome der

Anordnung" as the specifically topological axioms. He criticizes Hilbert's presentation of them, which does not allow one easily to see that they are independent of the axioms in his other groups. But Hilbert left out of the first edition what would in later editions be called the "Vollstandigkeitsaxiom." This axiom ensures that the spaces in question are mathematically continuous, and Poincaré fills the gap left by Hilbert by formulating for him one version of this second-order axiom. Now Poincaré would have wanted to include a version of the Vollstandigkeitsaxiom among the axioms of topology, for precisely this axiom represents the content of the geometric intuition he later takes to supply the subject matter of topology, as we have seen. Although in Hilbert's treatment the Vollstandigkeitsaxiom is not among the "Axiome der Anordnung," it does impose an ordering condition on the elements that constitute the spaces under investigation. With this background, we can now turn to Poincaré's late assimilation of geometric intuition to convention.

In (1912) Poincaré returns to Hilbert's "Axiome der Anordnung" in the context of a discussion of his Kantian foundations for topology. Speaking of these axioms, he says:

> les axioms ne sont pas, en realité, pour nous simples définitions, des conventions arbitraires, mais bien des conventions justifiées. Pour les axiomes des autres groupes, je tiens qu'elles sont ... les plus commodes; pour les axiomes de l'ordre il me semble qu'il y a quelque chose de plus, que ce sont de véritables propositions intuitives, se rattachant à l'Analysis situs (1912 503).

The argumentation here is complicated and condensed, but notice first that Poincaré asserts that the axioms of order are *both* "conventions justifiées" and "véritables propositions intuitives." Of course, given his inconsistent use of the term "intuition," this in itself need not be surprising. In fact, however, Poincaré goes on to trace our knowledge of the axioms of order back to geometric intuition, which we have just seen to be synthetic *a priori*: "ces véritées, telles que les axiomes de l'ordre, nous sont révélés par l'intuition" into mathematical continuity (1912 503). This statement comes a mere four pages after the passage quoted above in which Poincaré says that justified conventions are naturally selected for. The competing demands on the notion of convention are becoming intolerable, for "conventional" axioms are conceived of as true, synthetic *a priori* beliefs laid down in us by evolution. Perhaps the notion cannot be saved, and that, as far as I am concerned, is just as well. But we should not let this cloud our perception of the general outlines of Poincaré's view. Evidently the axioms of order and geometric intuition into mathematically continuous order are pulled very close together, and appear to share fundamental epistemic properties. Geometric intuition provides us with an original model of the axioms of order. In this model, the elements also satisfy a version of the Vollstandigkeitsaxiom; *i.e.*, they are continuously ordered in the mathematical sense. We ourselves construct this model, but the possibility of this construction is built into our understanding: it is a form of our understanding. Our understanding arose in our evolutionary history and is a product of natural selection. If this is the correct interpretation, then not just the axioms of order but our understanding of mathematical continuity as well is at once synthetic *a priori* and "conventional" in the sense that it arose via natural selection.

Again, one might well object to the use of "convention" in this sense, especially if conventions are thought to lack truth-values. I am by no means interested in preserving the word in this context, except for purposes of historical accuracy. Poincaré seems to have thought it was important. It is perhaps worth noting that the apparent blending of evolutionary convention (or belief) and Kantian forms of experience, while perhaps confusing, fits in well with other views Poincaré held (cf. also Russell 1897 187). According to Poincaré, "pour un être complètement immobile, il n'y aurait ni espace, ni géométrie" (1903 294; cf. 1898 7).

The idea is that we learn about space by exercising our capacity for human body movement, for it is only through movement that we obtain the notion of displacement, and geometry is the study of a continuous group of displacements. We have already met with this idea above, but in the present context its significance lies in the fact that the capacity for human body movement is in all major details hereditarily determined. It is reasonable to conjecture that Poincaré knew this, since ablation experiments on the cerebellum performed already for decades had shown it played an important role in coordination and integration of movements. But it is beyond the scope of my study to argue conclusively that Darwin and Kant are joined in quite this way by Poincaré.

2.2.2 Poincaré's Construction of Mathematical Continuum.

I want now to discuss in more detail "la faculté de construire un continu ... mathématique." I will try to make my discussion sensitive to the change in Poincaré's views over time. The purpose of my discussion will be twofold. Through it, the precise point will become clear at which appeal to "geometric intuition" would be needed in the construction of the mathematical continuum. Secondly, by comparing this construction with a parallel formal development of the foundations of real number theory, it is possible to determine a formal equivalent of this geometric intuition. Evaluation of this formal equivalent will then show the impredicativity implicit in the original intuition into topological continuity. We shall find, I think, that Poincaré must again choose between an illegitimate intuition or a legitimate impredicativity.

Our understanding of mathematical continuity occurs in three stages, but it begins, as it did for Russell, in sensation. It begins with "les données brutes de l'expérience, qui sont nos sensations" (1893 29). The leading characteristic of sensation is what Poincaré calls its "imperfection". The imperfection of sensation prevents us from distinguishing in sensation what we can easily infer are distinct:

> Il arrive que nous sommes capables de distinguer deux impressions l'une de l'autre, tandis que nous ne saurions distinguer chacune d'elles d'une même troisième. (1903 286)

For example, the sensation produced by objects of 10 and 12 grams respectively may be easily enough distinguishable, even though neither is distinguishable from the sensation produced by an object of 11 grams. This situation is characteristic of what Poincaré calls "the physical continuum," and he expresses the "formule du continue physique" as follows:

$$A=B \quad \& \quad B=C \quad \& \quad A<C$$

This formula Poincaré considers "repugnant to reason" (1898 14): "Il y a là, avec le principe de contradiction, un désaccord intolérable" (1893 29). We escape this contradiction by an "artifice" (1898 14), the *a priori* construction of the mathematical continuum. But before continuing with the next step of this construction, a few interpretive remarks are perhaps in order.

Poincaré is quite clear that to arrive at the recognition of the physical continuum we must make many conventions and abstractions (1912 490-1). Phenomenally, it is no easy matter to isolate the sensation produced by an object of 12 grams to the point where we can compare it directly with another sensation. The music playing in the background must be ignored. The possibility of this wholesale abstraction is not one I wish to question; for my purposes it does not matter if Poincaré's phenomenalist attitude is viable. Similarly, I will ignore the related difficulty regarding the "contradiction" given in sensation. The problem here is that, unless we specify the conventions and abstractions in certain ways, the formula of the physical continuum will not actually entail a contradiction. For the identity sign "=" is obviously being used conventionally: literal identity is not intended, only identity in some respect, or similarity. Similarity is not transitive, but the "contradiction" Poincaré wishes to deduce from the formula depends upon the transitivity of the relation denoted by "=". Comparable problems infect the interpretation of "<", but I am not interested in raising this sort of problem for Poincaré. I grant he has shown that sensations are imperfect in that the "données brutes" of sensation are "contradictory."

Having assumed this, one cannot fail to notice the historical precedent in Hegel. Poincaré's account of the psychological genesis of the notion of continuity is *dialectical*. Indeed, after "escaping" the contradiction given in sensation, Poincaré will introduce the mathematical continuum of the first order, which will also be found, on certain assumptions, to be contradictory. The repair leads to the notion of classical continuity, or continuity in the mathematical sense. But Poincaré's preference for a dialectical construction of mathematical continuity should not be taken to indicate any great allegiance on his part to Hegel. After all, Poincaré maintained throughout his career, in (1893) as well as in (1912), that the elements of the continuum were modally independent of one another. Moreover, he also held (at least at the time of writing (1893)) that the "celebrated formula" was false: the mathematical continuum is in no sense unity in multiplicity, for there is no unity. It is interesting to note in this regard Russell's reactions to Poincaré's vacillations. In (1897x 78-9), Russell lauds Poincaré's dialectical account of the notion of continuity as showing conclusively that this notion is "conceptual", and not sensational. He does not at this time mention the disappearance of the unity from the celebrated formula, with which he can hardly have agreed. In (1903 347), however, Russell passes in silence over Poincaré's dialectic but quotes him with approval on the falsity of the celebrated formula. By the time Poincaré considers reintroducing a sense of unity into his new "géométrique intuition," Russell has himself reintroduced impredicativity in the form of the Axiom of Reducibility. In my opinion, both of these devices are misleading in that they merely disguise the impredicativity necessary in any account of mathematical continuity. Moreover, if, as I argue in detail later, the impredicativity of the mathematical continuum consists in the mutual ontological dependency obtaining among some of its elements, then a formal account will be forthcoming of "celebrated" unity which both early Poincaré and late Russell wished to eliminate.

Hegel's view is that the continuum of mathematics is "self-contradictory." According to Russell (1897), this "self-contradictory" character stems from the combination of the homogeneity and the relativity of continua. Homogeneity requires that a continuum have distinct elements, but the relativity requires that these not be ultimately distinct, since they stand essentially in specific relations to one another. I have indicated above how the alleged "contradiction" here, which Russell sought to identify with the Kantian form of sensation, is based on a confusion of two separate notions of logical subject. But it is interesting that, for Poincaré too, the contradictory character of sensed continua stems precisely from its elements being distinct but not distinguishable. One can see Poincaré's position as a response to the view that continua such as space are given in sensation. Hegel begins here and pronounces continua self-contradictory. Poincaré claims sensed continua are not continua in the mathematical sense, which are free from contradiction. Clearly Poincaré is trying to account for, but also limit the significance of, the "Ineinandergeflossensein" which Hegelians considered the essence of continuity. Thus he denies that elements of the sensed continuum are "exterior" to one another, but insists (even very late in life) on their being exterior in mathematical continuity.[36]

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[36]Cf. Russell (1897 189; 1896x 17) and references there. McLarty (1988) gives a nice explanation of the topological facts which may have motivated the notion of elements not being external to one another in the mathematical continuum.

According to Poincaré, "our representations are simply the reproductions of our sensations" (1898 5). Thus represented continua suffer the same self-contradiction as sensed continua. By consequence, knowledge of continuity in the mathematical sense is not representational. Since geometrical space is continuous in the mathematical sense, "we cannot image geometrical space.... We cannot represent to ourselves objects in geometrical space, but can merely reason on them as if they existed in that space" (1898 5; cf. 1895 635, 1903 424). This view is connected with the idea that it is from human body movement that we learn about space. We do not literally represent objects in space; we "localize" them only in the sense that we know what movements are necessary to obtain them. This, however, brings out an interesting difficulty for Poincaré's view: what is non-representational knowledge? How can we know about something without representing it? If, as he later thought, our knowledge of the mathematical continuum is based directly on a synthetic *a priori* intuition into continuity, then this intuition itself is not representational. From a Kantian point of view, this is confusion, since intuitions, even pure intuitions, are representations ("Vorstellungen"). But I leave this matter to the side.

To escape the "contradiction" of the sensational or representational continuum, one makes use of the arithmetical intuition represented by thesis *i* above. The "contradiction," according to Poincaré, forces us first to recognize that B is distinct from both A and C. But this raises the question: how many distinct elements (or possible elements) exist between A and C? Poincaré contends that the contradiction will not be alleviated by postulating only a finite number of such elements, since we can always imagine between them others, D, E, and F, which reproduce the "contradiction" inherent in the physical continuum, *i.e.*, such that D=E, E=F but D<F. In short, once we begin to intercalate terms between A and C, "nous sentons que cette opération peut être poursuivie au delà de toute

limite et qu'il n'y a pour ainsi dire aucune raison intrinsèque de s'arrêter" (1893 31). We "sense" this (sic) via the capacity postulated by thesis *i* above:

> Tout se passe comme pour la suite des nombres entiers. Nous avons la faculté de concevoir qu'une unité peut être ajoutée à une collection d'unités; c'est grâce à l'expérience que nous avons l'occasion d'exercer cette faculté et que nous en prenons conscience (1893 31).

The result is the "continu mathématique du premier ordre" (a *dense* series, in the sense of Section 0.3), knowledge of which is therefore synthetic *a priori*, since the same fundamental "puissance" is employed in creating it as is employed in creating the natural numbers. Again, serious questions could be raised about this account, but raising these is not part of my purpose here. Suffice it to say that the use of mathematical induction to conclude something about all the terms interpolated in this second step would no doubt be legitimate by Poincaré's lights, so that this stage of the construction of mathematical continuum is thoroughly arithmetical, and involves thesis *iii* discussed above as well.

The next and final step engenders the "continu de deuxième ordre, qui est le continu mathématique proprement dit" (1893 32). Poincaré's introduces this final step as follows. It is clear that if two continuous lines of merely first order were to cross, they need not overlap, or have a part in common. By an "effort de plus" the "géomètre pur" is able to derive a contradiction from this. This contradiction will not be alleviated unless one admits the existence of elements in the continuum corresponding to all real numbers. The "effort de plus" involves, as Poincaré conceives it, taking the limit of the common area of two crossing *represented* lines, e.g., two crossing one-dimensional physical continua:

> La partie commune nous apparaîtra comme un point qui subsistera toujours quand nous voudrons imaginer nos bandes de plus en plus minces, de sorte que nous admettrons comme une vérité intuitive que si une droite est partagée en deux demi-droites, la frontière commune de deux droites est un point (1905a 55, cf. 1893 32)

This passage, which I will interpret more precisely in a moment, raises numerous difficulties. The most obvious is perhaps the question whether the "vérité intuitive" to which the pure geometer appeals is a new non-arithmetical synthetic *a priori* intuition or somehow a further application of the "intuition du nombre pur." From what I have said above, it should be clear that Poincaré vacillated on this question over the years, so let me put this first question aside for the moment as well. A second difficulty parallels the problem I raised a moment ago for the dialectical transition from the physical to the first-order mathematical continuum. Various assumptions have to be made to obtain the "contradiction" inherent in the inferior continuum. Last time we saw that Poincaré assumed the relation denoted by "=" was transitive, and this seemed dubious. Here, however, the problem is yet more serious. The assumption needed to show that the first order continuum is self-contradictory seems to be precisely that crossing 1-dimensional continua always overlap. Since crossing first order continua do not always overlap, they cannot be continua "proprement dit." But the assumption states (admittedly in an imprecise form) the very essence of the continuum of the second order, so Poincaré seems to be reasoning in a circle. First order continua, he seems to be saying, are inadequate precisely because there are *not* second order continua. It is hard to see how his dialectic is to get off the ground.

The nature of this difficulty can be brought out by a consideration of the example Poincaré uses to illustrate the final step in his construction of the mathematical continuum. This will serve as well to clarify the passage quoted and render clearer Poincaré's argument. But, unless I am mistaken, the argument does have a circular character. What will remain apparent, however, is that even at this early stage Poincaré believed we had synthetic *a priori* intuition into classical mathematical continuity. His later vacillation concerns only whether this intuition was entirely arithmetical or only partly so. The example he uses bear this out. But, I shall argue, his intent with this example entails, as a special case, the postulation of elements of the continuum corresponding to impredicative real numbers. This is fine and well before (1906) when Poincaré first banned impredicativity. After that time, his continued belief in synthetic *a priori* knowledge of classical continuity defies explanation. Poincaré cannot have things both ways: either his synthetic *a priori* intuition is impredicative, or it is not; but then, it does not yield knowledge of mathematical continuity.

The pure geometer is able, by a special effort, to conceive a limit to the diminishing common area of two crossing 1-dimensional physical continua. This limit is the point. Since the two overlapping physical continua are arbitrary, the special effort of the geometer ensures such an intersection-point will always exist. The "vérité" that "deux lignes qui se traversent ont un point commun ... paraît intuitive" (1893 32). But if "lines" are first-order continua, this "truth" is false. Thus the contradiction:

> La contradiction serait manifeste dès qu'on affirmerait par example l'existence des droites et des cercles. Il est clair, en effet, que si les points dont les coordonnées sont commensurables étaient seuls regardés comme réels, le cercle inscrit dans un carré et la diagonale de ce carré ne se couperaient pas, puisque les coordonnées du point d'intersection sont incommensurables. Cela ne serait pas encore assez, car on n'aurait ainsi que certains nombres incommensurables et non pas tout ces nombres. (1905a 54)

I wish to draw attention to Poincaré's leading conclusion here, which is that fewer than all the incommensurable numbers *"ne serait pas encore assez."* Any two curves which cross one another must have a point in common. The example Poincaré gives is, as he indicates, not sufficient to demonstrate his contention, but what this contention is is clear enough. When we inscribe a circle in a square, and then draw a diagonal of the square, the diagonal crosses the circle twice. In order to speak of the "points" at these crossings, it is necessary that we do not conceive of the perimeter of the circle as merely first order continua. For such continua have gaps, and the crossings may fall on the gaps and not on points.

If these considerations are meant as an argument for the leading conclusion I just mentioned, the appropriate response to them is: so what? Certainly it "appears intuitive" that the diagonal of the square and the perimeter of the circle intersect, or cross at a point and not at a gap. Certainly they might cross at a gap if these lines were merely first order continua. There is nothing incompatible about believing both these at once. One may further admit that if "lines" (such as the perimeter of the circle and the diagonal of the square) always cross at a point, then "lines" are not so-called first order continua. But we have as yet no reason to jettison the idea that continua "proprement dite" are merely first order: we might just as soon jettison the "apparent intuition" that there are "lines" in the relevant sense, *i.e.*, lines which always cross at a point. Thus there is no compelling motivation to join in the "effort de plus" of the pure geometer and pass beyond the first order continuum to the "continu mathématique proprement dit." Poincaré's argument for the necessity for this move is circular.

2.2.3 The Hard Choice in Analysis

Criticism of Poincaré's dialectical account of the creation of the mathematical continuum, however, is not my real concern. Poincaré wants classical continuity, and I am prepared to let him have it. Let us just be clear *what* he has. He has a continuum composed of elements corresponding to *all* real numbers. He has this, not just as an accident of his early exposition of the foundations of analysis, but even in his final writings, in which (as we saw above) he hesitantly accepts as "mathematically adequate" the "définition analytique" of dimension, which commits him to "variable quantities" taking *all* real numbers as their values. Again, in an almost casual way, Poincaré (1902) pointed out that Hilbert (1899) had not succeed in studying "notre espace" because he neglected to include what came to be called the "Vollstandigkeitsaxiom," a version of which Poincaré actually gives and which is equivalent to classical continuity as it has come down to us from Dedekind (1872) and Cantor (1895). For Poincaré, classical continuity is not just acceptable, but a necessary idea for the foundations of geometry, analysis and topology. If, early on, he had a mistaken faith in arithmetization, and held that the elements of this continuum could be "created" simply by virtue of the arithmetical capacity given as thesis *i*, later he had the mistaken impression that such elements could ultimately be defined predicatively. As if sensing his mistake, he introduces in (1912) "l'intuition géométrique" to justify our knowledge of "ce continu ... primitivement amorphe" (1903 281) and backpedals from his earlier metaphysical objections to the Hegelian "lien intime" among its elements. "Unity" and "wholeness" of the continuum return with this new intuition, but the "lien" never gets quite so intimate as the early Russell evidently thought it was; for while the elements of the mathematical continuum are "liées entre elles," they remain "exterior l'un à les autres", or "externally related" as Russell would have put it. For Poincaré, the elements which compose the mathematical continuum are modally independent of each other.

Now, in my view, it is this last point which makes Poincaré's views metaphysically inadequate. His cherished belief in a continuum in the mathematical sense commits him to the existence of all real numbers. His early faith in arithmetization was actually incompatible with this commitment, but later his 1906 ban on impredicativity was incompatible with it as well. If there are no impredicative real numbers, there is no continuum of real numbers. This can be seen by the following illustration, which improves upon Poincaré's admittedly inadequate example of the circle inscribed within a square, but does not admit any postulate Poincaré would have wanted to deny. Let f be a continuous function such that $f(0)<0$ and f(1)>0. Recall that the notion of continuous function is supposed to be justified by Poincaré's "intuition géométrique" (1912 485-6). The x-axis and the curve described by f are crossing 1-dimensional continua. Clearly Poincaré is committed to the claim that these continua cross at a point p with coordinates (r,0). Now in general, r will be not only non-arithmetical, but impredicative. Any definition of certain such r's in the language of analysis will contain bound variables ranging over r. To put this in a more general way, any definition of certain r in the language of analysis will contain bound variables whose range is a set s containing r as an element. I think it could be argued that this statement requires a modal interpretation: in every possible world in which such an impredicative r exists, there also exist the other elements of s. r is essentially related to these, and cannot exist independently. Belief in the mathematical continuum thus requires, contra Poincaré, that we relinquish the claim that its elements are modally independent.

The argument for this will have to be made elsewhere.[37] My concern at present is still the philosophical context of Poincaré's attempted construction of the mathematical continuum. Poincaré never rejected arithmetization, he only limited its significance. In certain circumstances, according to him, arithmetization could lead us into a misunderstanding of the phrase "all real numbers." Mathematically adequate definitions using this phrase were in such circumstances apt to be philosophically inadequate. Philosophical adequacy could be attained only by introducing a categorial intuition into mathematical continuity, just as the justification of mathematical induction, on his view, could not proceed through any explicit definition of natural number, but required

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[37]Chapter 3 is intended to go part of the way toward this goal by presenting a theory of impredicativity involving similar claims of mutual ontological dependency. There, however, the claim will not concern the real numbers specifically, but only impredicativity as it is understood in Russell's ramified type theory. I hope in the future to extend this account to standard set-theoretic constructions of the real numbers, and thus explicitly to justify the suggestion made above.

appeal to a categorial intuition. Actually, the parallel between the two branches of mathematics is deeper than this. In arithmetic, the "informal" justification of induction succeeds only by hiding impredicative principles in "categorial intuition". In analysis, the impredicativity of the continuum needs similar hiding. Poincaré's "intuition géométrique" can account for the "construction of the mathematical continuum" only if it is considered equivalent to formally impredicative principles. The mathematic continuum is impredicative, and if Poincaré wants to say impredicative definition is viciously circular, he will have to hide the impredicativity of his own creation. Kantian synthetic *a priori* intuition is a suitable device for such concealment; it appeared at least to have worked in arithmetic to much the same effect. This deeper parallel leads to a second very hard choice Poincaré has to make, a choice which has exactly the same form as the choice we saw he had to make in the foundations of arithmetic: why is the impredicativity of mathematical continuity illegitimate if formalized, but legitimate if kept at the level of "intuition"? It is as if saying what you mean made what you say meaningless, but passing over what you mean in silence expresses your intentions perfectly.

The choices Poincaré has to make face us as well. Contemporary formal developments of impredicativity support a distinction like Poincaré's between "mathematical adequacy" and "philosophical adequacy" in definitions. Take analysis first. The *set* (or *concept*: it does not matter which we chose) of real numbers can be defined in mathematically precise fashion; we owe such definitions to Dedekind and Cantor. Such definitions say, in a mathematically irreproachable way, what it is to be a real number. The standards for "irreproachability" are high, and would have satisfied Poincaré, for there are *predicative* definitions of the set (concept) of real numbers. In the same way, there exist a predicative "mathematically adequate" explicit definition of the set (or concept) of natural numbers. By themselves, however, the predicative definitions of these two sets (concepts) are "philosophically inadequate" and should not satisfy us entirely, and the reason is similar to Poincaré's reason for philosophical suspicion of the "definition analytique" of the 1-dimensional continuum. This failed, he thought, to license talk of all real numbers, and such talk could be justified only by consideration of the natural ordering in which the real numbers are presented to us. The philosophical inadequacy of contemporary predicative definitions of the real numbers ought really to raise in us the same suspicions. We do not have license to talk about "all real numbers" if part of our understanding of this conception is that the real numbers are continuously ordered in the mathematical sense. The predicative definition of the real numbers does not support proof of the classical continuity of real numbers, and so it "gives" us the real numbers only in an abstract sense, *i.e.*, without justification of their natural ordering properties. In contemporary formal understanding of arithmetic, the same dichotomy applies: the predicative definition of the natural numbers does not support proof of mathematical induction, and so it "gives" us the natural numbers only in an abstract sense, *i,e.*, without justification of their natural ordering properties. Induction and the least upper bound principle are not only the fundamental theorems of the ordered structure of the numbers systems they "define," but they can even be put in quite similar forms,[38] so that the distinction between the two number systems lies in relatively weak differentiating axioms.

It seems to me, therefore, that Poincaré's attempt to shunt the formal power of explicit theories of arithmetic and analysis off into intuition does not work. He can only say his intuition suffices if he swears himself to silence on the issue of how it suffices. This may have been part of what Goldfarb intended by finding little of Kant in Poincaré's notion of intuition. For the latter's insistence that

[38]Compare e.g. the treatment in Huntington (1905).

"geometric intuition" justifies the least upper bound theorem (like his insistence that thesis *ii* justifies mathematical induction) appears to reduce to an ineffable "immediate conviction." Still, it would be wrong to separate this from the "surrounding Kantian context" evident from e.g. (1899 254) and (1912 504). It appears strange to us today to claim that, if we had no *a priori* capacity to define second-order mathematical continua, ordinary experience would be impossible; but such, I believe, was Poincaré's view.

Chapter 3: Modality and Impredicativity

3.0 Introduction

In this chapter I give a modal account of impredicativity. The principal modal notion in this account will be what I call *reciprocal ontological dependency*. The basic idea behind this notion appeared in Section 1.7.1.1. as the consequent of principle (4), which was one formalization of the so-called doctrine of internal relations. Roughly, x and y may be said to be reciprocally ontologically dependent if they are distinct and the following holds:

$$\Box(Ex \leftrightarrow Ey)$$

This notion is one which Russell apparently rejected during his 1898 revolution. Yet in (1903) Russell defined, and made systematic use of, a notion of *strict* (or non-reciprocal) ontological dependency. I begin this chapter by stating a certain difficulty Russell's definition poses in the context of his post-revolution rejection of modality. I then go on in Section 3.3 to say how a solution to this difficulty can be extended in a specific way to define a notion of reciprocal ontological dependency which is key to the modal account of impredicativity I support.

In the remainder of this introduction, I want to accomplish two things. First, I want to describe the modal semantics I will use throughout this chapter. Second, I want very briefly to sketch some technical logical results regarding Russell's ramified type theory. Ramified type theory is Russell's official logic. In this logic, Russell sought to define basic mathematical notions such as natural number and continuity, and to deduce key mathematical theorems. The technical results I sketch concern how effective Russell's logic can be in attaining these goals. This sketch will also facilitate the presentation of my modal account of impredicativity in Section 3.3.

3.0.1 Possible Worlds Semantics

In what follows I take as an underlying logic a free modal logic with two logical predicates "=" and "E" for identity and existence. Sentences are defined in the usual way and are assigned truth-values relative to a possible world. Bound first-order variables occurring in such sentences are understood to range over the objects in the possible world relative to which the sentence is assigned a truth-value. Free first-order variables, by contrast, are assigned values from the union of the domains of *all* possible world. Thus first-order free variables range over what may be called the *possible objects* in the model. As in chapter 1, I sometimes say "'xRy' is evaluated as true in world *w*" to abbreviate "for some assignment of objects to 'x' and 'y', 'xRy' is evaluated as true in world *w*."

"Ex" is assigned the value true in a world just in case the possible object assigned to "x" is in the domain of that world. Again as in Chapter 1, I adopt the convention that, with the exception of "E", predicates may be true of objects in worlds where those objects do not exist. This convention is often made only with respect to the predicate "=", since it simplifies the theory of identity:

$$x=y \rightarrow \Box(x=y) \qquad (R=)$$
$$x\neq y \rightarrow \Box(x\neq y) \qquad (R\neq)$$

But here the convention is extended to other predicates as well. The point of this extension is similarly to simplify expression of the claim that other predicates hold essentially of objects of which they hold at all. Parsons (1983a 298 ff.), for example, has proposed that modal principles similar to those just stated for identity should be adopted for set membership:

$$x\in y \rightarrow \Box(x\in y) \qquad (R\in)$$
$$x\notin y \rightarrow \Box(x\notin y) \qquad (R\notin)$$

There are obvious ways of stating the above principles under the opposite convention. Given the convention, however, the existential consequences of truths must be made explicit. Thus Parsons also argues for a principle equivalent to following:

$$\Box(x\in y \wedge Ey \rightarrow Ex) \qquad (E\in)$$

In Parsons' view, the existence of a set in a given possible world requires that each of the elements of that set exist in the same possible world. By contrast, the existence of all the elements of set in a possible world is not sufficient for the existence in that possible world of that set. Parsons' goal is to supply an analysis of the intuitive idea that sets are *constituted* by their elements. Similar remarks would seem to apply for example to extensional mereological wholes.

3.0.2 Meta-mathematical Results concerning Ramified Type Theory

I come now to certain meta-mathematical results pertaining to the logical strength of Russell's ramified type theory. I will describe these results only in the briefest outline, and only in so far as they are relevant to my account of impredicativity. The results are important because they show that Russell's type theory was not sufficient to define key mathematical ideas nor to prove certain key mathematical theorems. The theorems which cannot be proven in Russell's ramified type theory may be said to be impredicative, since type theory is based upon a principle expressly formulated to prohibit the use of impredicative definitions and exclude the postulation of impredicative objects.

Russell originally formulated ramified type theory as a means of resolving the paradoxes that emerged around the turn of the century, but also as a logical foundation of mathematics. Ramified type theory, then, was the official logic used in Russell's foundation of mathematics. But Russell's "logic" comes with a specific interpretation, which is to say with a specific ontology consisting in an infinite hierarchy of propositional functions classified into types. It was crucial to Russell's foundational goals that key theorems of mathematics be seen to be true on this interpretation of type theory, as well as derivable within his logic. In particular, Russell wanted to prove in his logic both the principle of mathematical induction and the least upper bound theorem. These two theorems are plausibly thought to be indispensable to any theory which sets out to provide a definition or analysis of the concepts natural number and real number or continuity respectively. The question is whether these theorems are, as Russell hoped, derivable in ramified type theory.

Now Russell's original formulation of ramified type theory included a principle he called the *axiom of reducibility*.[39] With this axiom in place, there is no difficulty deriving the two theorems just mentioned. But the axiom of reducibility did not prove popular; and in (1925) Russell decided that it had to be rejected. Although Russell thought he could still prove mathematical induction in ramified type theory without the axiom of reducibility, he saw no way of deriving the least upper bound theorem. Russell's hope of having derived mathematical induction without the aid of the axiom of reducibility was dashed in (1944) when Gödel pointed out an error in Russell's proof.

In the years after 1925, more general methods were worked out for determining whether a given theory could or could not deductively yield a given theorem. Developments in proof theory and other areas of logic have made it possible to measure and compare the proof-theoretic strength of theories, and under some conditions to say when certain theorems are beyond the means of a particular theory. Various attempts were made to reformulate Russell's type theory in accordance with the increased standards of rigor, and to determine whether Russell's error could be

[39]In point of fact there is more than one axiom of reducibility. Their precise content need not be explained here; see (1907 241-244) and (1910 55-60, 161-167).

repaired, and also whether the least upper bound theorem is derivable in a more rigorously formulated ramified type theory. From these developments it has turned out that Russell's ramified type theory is insufficient to prove either of these theorems, which may therefore be considered impredicative in a particular sense. More important perhaps, one must conclude that ramified type theory by itself is inadequate as a foundation for classical mathematics.

My concern in what follows will be to express in modal-semantic terms one principle underlying Russell's type theory. This principle, called the vicious circle principle, states what it is to be predicative on Russell's theory. Violations of the modal interpretation of this principle will suggest a general account of impredicativity. It is hoped that being clear on the original sense of "impredicative" in Russell will be of assistance in future attempts to extend the present modal account to set-theoretic or other foundations of mathematics.

3.1 Necessity and Logical Priority

I want now to return to Russell's rejection of modality. I argued in Chapter 1 that the principal notion Russell rejected when he abandoned idealism was the notion of necessity. Although there is considerable evidence for this, there are also passages in which Russell seems to qualify his rejection somewhat, and others where Russell seems positively to require some notion of necessity. I begin by restating some of the relevant principles from Chapter 1. I then discuss in terms of these principles the qualifications Russell sometimes seems to make regarding his rejection of necessity. But the main goal of this section is to discuss Russell's definition of a form of strict ontological dependency and to show why it is be interpreted modally.

The principles I discussed in Chapter 1 and which are also relevant in this chapter are the following. For any x and y, and for any 2-place relation R,

$$xRy \rightarrow \Box xRy \qquad (1)$$
$$xRy \rightarrow \Box(Ex \leftrightarrow Ey) \qquad (4)$$
$$\Box(xRy \rightarrow (Ex \wedge Ey)) \qquad (5)$$

In addition, I discussed the non-modal principle that, for arbitrary x and y,

$$\exists RxRy \qquad (3)$$

Now Russell's rejection of necessity led him to deny (1) in a strong way. (1) was not false, according to him, but meaningless. Perhaps Russell should have denied the others in the same strong way, to the extent, at least, that they make use of the notion of necessity. But Russell did not uniformly do so. I want now briefly to indicate what became of these principles in Russell's analytic period. I will leave (3) out of account here, since the necessity operator is not needed to state it; but it may be said that Russell evidently would have believed (3) even if the relation ≠ were excluded from the range of the bound "R".

At times, Russell's rejection of necessity seems total, as for example in (1900a) when he wrote that "the subject of modality ought to be banished from logic" (cf. 1904 26). At other times, however, it seems that Russell retained some rather thin conception of necessity definable in terms of material or formal implication:

> Everything in a sense is a mere fact.... What is true, is true; what is false, is false; and concerning fundamentals, there is nothing more to be said. The only logical meaning of necessity seems to be derived from implication. (1903 454; cf. 1910a 374)

Russell is never very clear on what this derived notion would be, and the details are not important here. The point here is just that Russell's rejection of necessity sometimes seems less than total. Still, Russell made no systematic use of his derived notion, nor did he try to use it to interpret (1) in a new way. Similarly, Russell's qualified rejection of necessity was in no way used to make (4) seem plausible or even meaningful. Despite his occasional qualifications, Russell rejected (1) and (4), as well as the modal notions of essentiality and reciprocal ontological dependence[40] associated with them.

Principle (5), however, did seem to live on in some form in Russell, despite the occurrence in it of the notion of necessity. Even after 1898 Russell appears to affirm (5), since he wrote passages quite similar to (1898x 167-8), which was interpreted in chapter 1 as an affirmation of (5):

> whatever may be an object of thought, or may occur in any true or false proposition, or can be counted as *one*, I call a *term*..... every term has being, *i.e.*, *is* in some sense. (1903 43)

This again would suggest that Russell's rejection of necessity was not total, but it is not my concern to spell out an interpretation of (5) Russell would have accepted. Suffice it to say that Russell's use of "may be" and "may occur" must apparently have some modal interpretation.

Yet despite his rejection of modality, Russell defines at (1903 137-8) a form of strict ontological dependency which he calls *logical priority* and which would seem to be a modal notion. Russell's definition is not perfectly explicit, but is based on his claim that the "logical priority of A to B requires" that "B *is* implies A *is*, but A *is* does not imply B *is*." It is difficult to formulate this definition without the aid of modal notions Russell rejected, but an obvious first try would be the following:

$$x <_{lp} y =: (Ey \rightarrow Ex) \wedge -(Ex \rightarrow Ey) \qquad (6)$$

Here "$x <_{lp} y$" can be read "x is logically prior to y" or, alternatively, "y strictly presupposes x". This is intended to be *strict* logical priority

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[40]In (1904 26), Russell goes so far as to conclude that "the subsistence or being of a whole cannot presuppose that of its parts in any sense in which that of the parts does not presuppose the whole." Although, as we shall see, Russell elsewhere defines a notion of presupposition for which this statement is not true, (1904 26) can be taken to confirm that Russell had little use for the notion of reciprocal ontological dependency.

in the sense that the relation $<_{lp}$ is asymmetric. Now Russell's original definition as quoted was intended by him to capture the intuitive idea that e.g. a part of a whole is logically prior to that whole because, whereas the part could exist without the whole, the whole could not exist without the part. But (6) fails to capture this idea, at least if "$\rightarrow$" represents material or truth functional implication. In fact, under these conditions, "$x <_{lp} y$" is true iff y does not exist and x does. There are further difficulties, since it is not clear what significant role the existence predicate "E" can play in Russell's extensional, non-free logic. With "E" as Russell's "being," if "all terms have being," clearly "$\neg Ex$" is always false.

Clearly the idea Russell is trying to express at (1903 137-8) would be better captured by interpreting "$\rightarrow$" in (6) as some form of counterfactual implication or as C.I. Lewis's strict implication. But Russell had no real way to understand "$\rightarrow$" except as material implication. This is presumably why Russell elsewhere calls logical priority a "very obscure notion." I suggest the following alternative formulation of Russell's notion of strict presupposition or logical priority:

$$x <_{lp} y =: \Box(Ey \rightarrow Ex) \quad \wedge \quad -\Box(Ex \rightarrow Ey) \qquad (7)$$

According to (7), "$x <_{lp} y$" is defined to mean that it is necessary that, if y exists, x does, but it is not necessary that, if x exists, y does. The existence of x presupposes that of y, but the reverse is not true. Thus despite Russell's sweeping rejection of necessity, his own notion of logical priority would appear to require it.

One might, therefore, pose the following dilemma for Russell. He must either reject his notion of logical priority or embrace (7), and with it some modal-semantic notion of necessity. In what follows, I will use the possible-worlds semantics described in Sec. 3.0.2 to interpret (7). On this semantics, "$x <_{lp} y$" will be interpreted as true just in case x exists in any possible world where y does, but it is not the case that y exists in every possible world where x does. By posing the above dilemma for Russell, I do not mean that, short of rejecting his notion of logical priority, Russell must accept a possible worlds semantics; for Russell need only interpret the necessity operator in (7), and this can presumably be done in some other way. On the other hand, nothing turns in the sequel on which semantics Russell would have to accept if he were to accept (7); and for present purposes a possible worlds semantics is as good as any. Thus (7), as I henceforth interpret it, is not intended in any historically accurate way to capture Russell's own intention with his definition of logical priority; but it will, I hope, help to promote clarity in subsequent discussions.

3.1.1 Serious Difficulty for Russell

I said that Russell must either reject his own notion of logical priority, or embrace (7), and with it some modal-semantic notion of necessity. *But Russell cannot reject the notion of logical priority.* He uses it not only in (1903) but in a far more important way in (1910). Thus for example Russell's characterization of ramified type theory uses terms that are meaningless unless something like his notion of strict logical priority is employed. My purpose now is to use the above possible-worlds semantics to interpret some of the claims Russell makes in (1903) regarding propositions, and in (1907) and (1910) regarding propositional functions in his ramified type theory.

In (1903), speaking of the mathematical theory of the real numbers, Russell writes:

> For the comprehension of analysis, it is necessary to investigate the notion of whole and part, a notion which has been wrapped in obscurity ... by the writers who may be roughly called Hegelian (1903 137)

Russell is not announcing a mereological foundation of mathematics, but rather using the term "whole" in the quite general way that was common at the time. He goes on to distinguish three quite different so-called "part-whole" relations: set-membership, set-inclusion (subset of) and proposition-constituenthood. (Like Russell, I will ignore the second, since it is definable in terms of the first.) What is of interest here is that, according to Russell, each of these relations formally implies logical priority; that is, if "xRy" abbreviates one of "$x \in y$", "$x \subseteq y$", or "x is a constituent of a proposition y", then the following holds for all values of x and y:

$$xRy \rightarrow x <_{lp} y \qquad (8)$$

Again, it is hard to make sense of this implication in Russell, but, suitably interpreted, (8) seems quite plausible. Parsons for example has argued for a modal principle equivalent to the following to account for the sense one has that elements *constitute* the sets of which they are members.

$$\Box(x \in y \wedge Ey \rightarrow Ex) \qquad (E\in)$$

Notice that (E∈) follows from (8) together with the definition of logical priority given in (7).

The relation expressed by "x is a constituent of the proposition p" is, according to Russell, indefinable. But Russell nevertheless feels confident that the relation formally implies logical priority:

> Again, if we take a proposition asserting a relation of two entities A and B, this proposition implies the being of A and the being of B, and the being of the relation, none of which implies the proposition [even conjointly] (1903 137-8)

Observe that Russell says that the *proposition* implies the existence of its constituents, rather than that the *existence* of the proposition does. Nevertheless his idea is evidently that the proposition is a kind of whole which strictly presupposes its "parts" or constituents, much as a set strictly presupposes its elements. Notice too that Russell quite clearly states that the relation (a propositional function) is a constituent of the proposition. When the propositional function is itself complex and has constituents, these are in turn presupposed by the proposition. This changes in (1910), when Russell denies that propositions are after all wholes, and claims that "the values of a [propositional] function are presupposed by the function, not vice versa" (1910 39, 44). Still, what applied earlier to the proposition applies later to the propositional function. Russell will continue to insist that a propositional function strictly presupposes its constituents, which are thus logically prior to it.

3.2 The Vicious Circle Principle and Russellian Bound Variables

Indeed, understood correctly, this latter claim is the very basis of Russell's ramified type theory: it is tantamount to Russell's vicious circle principle. Analysis of Russell's explication of ramified type theory, and of his formulations of the vicious circle principle, will show this to be the case. The key idea will be that bound variables, on Russell's view, are not letters but constituents of propositional functions, and as such are presupposed by such functions. What this amounts to practically is that the values of a bound variable are strictly logically prior to propositional functions which contain them.

We have already seen Russell claim that "the values of a function are presupposed by the function, not vice versa" (1910 39; cf. 54). "This," according to Russell, "is a particular case, but perhaps the most fundamental case, of the vicious circle principle." I do not want to examine this particular case in detail, except to indicate the prominent role in it of the notion of presupposition. Frankly, it is difficult to make any sense of this notion except in terms of (7), and we have already seen how ill-suited this definition is in the context of Russell's metaphysics. Yet here it is again occurring in the "most fundamental case" of the vicious circle principle, which is itself the foundation of

Russell's mature logic, ramified type theory. Unless some modal notions are employed, it seems to me, one cannot strictly make sense of Russell's logic. Furthermore, once this point is accepted, it becomes possible to understand violations of the vicious circle principle in modal terms.

Russell at one point gives the following convoluted formulation of the vicious circle principle (VCP):[41]

> given any set of objects such that, if we suppose the set to have a total, it will contain members which presuppose this total, then such a set cannot have a total. By saying that a set has "no total," we mean, primarily, that no significant statement can be made about "all its members." (1910 37)

This statement obviously calls for analysis, but it may be considerably simplified before such analysis sets in. It is worth noting up front, however, that the notion of presupposition occurs again in this more general formulation of the VCP. I take it that Russell means (at least in part) the following:

> no significant statement can be made about any plurality of objects if any of the objects in the plurality presuppose that plurality

It must be noted at once that a "significant statement about a plurality" is an interpreted sentence which contains a bound variable understood as ranging over the objects in the plurality. Russell's principle represents an attempt to restrict what may count as the range of a bound variable, or a type. It must also be said under what conditions an object presupposes a plurality. Here it is vital to bear in mind that Russell is thinking of propositional functions with *constituent* bound variables that range over the plurality. "Whatever contains a bound variable," so another statement of the VCP goes, "must not be a value of that variable." Thus an object presupposes a plurality if it contains a bound variable which ranges over

[41]It is important to note that "set" here does not mean what it might in discussions of contemporary set theory. What Russell means by it might be better expressed by us as "plurality". I use this term to avoid confusion with the contemporary notion of a set. More is said about pluralities below. It might also be noted that Russell uses the word "about" here in an unusual way. Usually he prefers to say a propositional function is about the value of the free (not bound) variable(s) occurring in it.

that plurality.

The VCP as stated must be understood in the context of Russell's logic. For Russell, the range of bound variables is fixed and not subject to re-interpretation. In order to make the same significant statement about two distinct pluralities, one doesn't reinterpret a special linguistic entity, but, as Russell would say, one substitutes one entity (a bound variable) in the proposition for another. Consider, by contrast, a predicate "x is at least as tall as every one in Room 101". In one sense, the property expressed by this predicate is the same regardless of who happen to be in Room 101. If only very short people are in Room 101, one need not be especially tall to have this property. If quite tall people are there, one must be taller to have the very same property. But regardless how tall one must be to have the property, the property does not itself consist in having just this height. Rather, the property expressed by the predicate can be regarded as one and the same, no matter who's in Room 101 and no matter how tall one must be to be taller than anyone in it.

Russell, however, does not in general regard predicates in this way. Russell's view is more nearly comparable to the idea that the property expressed by "x is at least as tall as everyone in Room 101" changes as people enter and leave the room. This is due to the fact that, according to Russell, a bound variable is a constituent of a propositional function, and the ranges of bound variables are fixed by the natures of the variables. To change the range of the bound variable in a propositional function, one must change the bound variable; and by doing this, one changes the propositional function to a different, if systematically related, one. Thus the fact that bound variables are not letters (as they are for us) and are subject to fixed interpretations (instead of to interpretations that vary however we stipulate), leads Russell to regard the interpretation of a bound variable as crucial to the identity of propositional functions.

3.3 Pluralities, Existence, and Ontological Dependency

The vicious circle principle, as Russell understood it, stated a condition that pluralities must meet if significant statements were to be made about them. Since a significant statement about a plurality is an interpreted sentence containing a bound variable which has this plurality for its range, Russell's condition amounts to a condition that a plurality must meet if it is to constitute the range of a bound variable, or a *type*.

In this section I want to formalize the condition on pluralities that, according to Russell, makes a plurality suitable for being a type. Before this can be done, I need to say more about pluralities, including what it is for a plurality to exist, and what it is for an object to presuppose a plurality. I use the capital letters "X", "Y", "T", etc., to symbolizes pluralities and "EX" to symbolize "X exists". Let me begin with some remarks about the interpretation of these symbols.

The capital letters, "X", "Y", "T", etc., are to be understood as free plural variables.[42] Substitutends of plural variables are plural terms, for example plural descriptions. Thus a substitution instance of "EX" might be "the Hindu gods exist". But it is sometimes useful to speak of pluralities in the singular, as in "*a* plurality exists" or "*the* plurality of Hindu gods exists." I will use such singular notation, but I am not to be understood as making an ontological commitment to anything other than the things in the plurality. In particular, no single entity is to be identified with the plurality. There may well be a *set* of Hindu gods; there might even be a *mereological sum* of Hindu gods; but

[42]I will not make use of bound plural variables.

ontological commitment to any such *single* entity constituted in some way by the Hindu gods will have to be made separately, and will not be understood as following from "EX". To say that a plurality X exists, then, is only to say that the things in the plurality exist.

The things that can be in pluralities are the values of the first order variables. In the modal-semantics I am working with, these may be thought of as *possible objects*, which is to say elements of the union of the domains of all possible worlds. In principle, a given plurality could have objects in it which do not exist together in any possible world, but if "EX" is true in a world, then everything in the plurality exists in that world. This claim is reminiscent of Parson's principle ($E\in$) quoted above, and may be formulated in a similar way. If an object y is in a plurality X, I will write

$$y \propto X$$

Then the following principle may be accepted concerning pluralities: for any y and for any plurality X,

$$\Box(y \propto X \wedge EX \rightarrow Ey) \qquad (9)$$

(9) serves to formulate the idea that a plurality, like a set, is constituted by the things that are in it. Unlike sets, however, which need not exist in possible worlds where all their elements do, a plurality is nothing other than the things in it, and so it does exist in every possible world where all those things do.[43]

I said above that the VCP can be understood as Russell's attempt to articulate a necessary condition for a plurality to act as the range of a bound variable, or a type. Roughly, types are pluralities none of whose

[43]Note that it is essential that (9) be formulated with free first-order variable "y", since binding "y" in (9) would make the consequent true in every world. For similar reasons, the claim that pluralities exist in every world where *all* of the things in the plurality exist is **not** to be formulated by "$\Box\forall y(y \propto X \wedge Ey \rightarrow EX)$".

members presuppose the plurality. I want now to extend Russell's notion of logical priority to apply to pluralities.

$$Y <_{lp} x =: \Box(Ex \rightarrow EY) \wedge -\Box(EY \rightarrow Ex) \qquad (10)$$

"$Y <_{lp} x$" may be read "Y is logically prior to x" or "x strictly presupposes Y". It seems plausible that some objects strictly presuppose some pluralities; for example, a set x whose members were exactly the objects in a plurality Y may be said to strictly presuppose Y. By Parsons' principle ($E\in$), every element of the set x exists in every world where x does, and this is all that is required for Y to exist in a world. Thus it is necessary that "EY" be true in all possible worlds where "Ex" is true; hence the first conjunct holds. But the existence of all the elements of a set does not require the existence of the set; thus the second conjunct holds as well.

Russell's VCP was understood above as stating that a plurality is a type only if no object in the plurality presupposed the plurality. One might therefore formulate the idea behind Russell's VCP as follows:

$$x \infty Y \rightarrow x <_{lp} Y \qquad (11)$$

Unpacking the definition, one arrives at

$$x \infty Y \rightarrow \Box(EY \rightarrow Ex) \quad \wedge \quad -\Box(Ex \rightarrow EY)$$

The first conjunct is redundant given (9), so (11) may be simplified to:

$$x \infty Y \rightarrow -\Box(Ex \rightarrow EY)$$

Only pluralities which satisfy (11), and thus strictly presuppose everything in them, may be said to constitute a type. Note that this condition on a plurality is not sufficient for it to be a type, since it obtains for pluralities which are included in pluralities satisfying the condition.

Now let "$\phi\hat{x}$" denote a propositional function of some type. Suppose that it has as a constituent a bound variable of type n, and let Y be the range of this bound variable; that is, Y is the plurality consisting of the objects of type n. Then it seems clear that Russell believed

$$Y <_{1p} \phi\hat{x} \tag{12}$$

This ensures that $\phi\hat{x}$, which contains a bound variable, is not a value of that variable; for if it were, then by (11) it would follow that $\phi\hat{x} <_{1p} Y$, whereas $<_{1p}$ is asymmetric.

Accepting (11) as a formulation of the idea behind the VCP, one sees why Russell might have accepted the VCP as a principle of logic. If it were ever the case that

$$x \infty Y \wedge -(x <_{1p} Y) \tag{13}$$

then from the latter conjunct one could derive

$$-\Box(EY \rightarrow Ex) \vee \Box(Ex \rightarrow EY) \tag{14}$$

The first disjunct of (14), however, is not compatible with (9) and the first conjunct of (13). So the second disjunct of (14) must be true. But this too would have seemed strongly counter-intuitive to Russell. Why should the existence of one thing imply the existence of many things, none of which are all parts of the former in any recognized sense? In fact, the second disjunct of (14) must have appeared contrary to fundamental metaphysical principles. The status of the VCP as a logical principle seems secure.

There is of course one metaphysical framework according to which it is not so absurd that the existence of one thing should require the existence of others, namely the holism of "the writers who may be roughly called Hegelian." In the same year that Russell completed his first version of ramified type theory, he renewed his an attack on the idealists. In the course of this attack, he characterizes the view he rejects by saying that it admits the existence of *organic unities* or *significant wholes*:

> In a "significant whole," each part ... involves the whole and every other part.... The whole is constitutive of the nature of each part

Clearly on this view, which further maintains that "there can be one *and only one* ... significant whole" (1907a 34; quoted from Joachim 1906 78), it is true for any x and Y that

$$\Box(Ex \rightarrow EY) \qquad (15)$$

This, however, leads to much the same position as that associated with Russell's idealism (principles (1)-(5)) as this was discussed in Chapter 1. By (15), every possible object exists in every world where any possible object exists. Thus when Poincaré suggested the VCP in 1906, Russell must have felt that it had to be correct, since a denial of it seemed lead to back to a notion prominent among the idealists.

According to (15), it is necessary that, if one thing exists, everything does. To deny (11), however, all one needs is that there is some plurality Y and some $x \infty Y$ such that "$\Box(Ex \rightarrow EY)$" is true. To deny the VCP as based upon (11), one only need allow that there be some significant statement about Y; which is to say that Y be the range of some bound variable. Notice that, if "$\Box(Ex \rightarrow EY)$" is true, so is "$\Box(Ex \leftrightarrow EY)$", so that the denial of (11) amounts to the claim that there is a Y and an $x \infty Y$ such which are *reciprocally ontologically dependent* as this was defined at the beginning of this chapter. According to the VCP as based on (11), no range of a bound variable can be a plurality which is reciprocally ontologically dependent on something in itself. On the modal semantics I am using, this amounts to a denial that there are possible worlds where such pluralities exist. The domains of such worlds violate the VCP. Either there are no such worlds or the VCP is false.

On one sense of "impredicative" in Russell, an object x which reciprocally presupposes a plurality Y is impredicative. Given the above modal considerations, such an x will exist in a possible world just in case Y does. This is the primary tenet of my modal account of impredicative.

It seems to me that we have no good reason to deny there are possible worlds whose domain are pluralities some of whose members require the existence of all such members. There seems to be no obstacle at all to making significant statements about such domains. The VCP therefore appears false, and the notion of reciprocal ontological dependency perfectly coherent. Indeed, given the formalizations above, there appears to be no principled reason why Russell could accept his notion of strict ontological dependency as meaningful, but deny meaning to the notion of reciprocal ontological dependency.

Moreover, since ramified type theory is not itself inadequate for defining key mathematical notions or deriving key mathematical theorems, it is hoped that the above modal account of the VCP and impredicativity will be of some use explaining what adequate foundations of mathematics are committed to. What is needed here, for example, is a development of modal set-theoretical foundations of mathematics, along the lines of Parsons (1983a) perhaps, but extended to included the notion of reciprocal ontological dependency. It could then be investigated whether, for example, real numbers impredicative with respect to the language of set theory are subject to any special modal conditions. But this investigation is beyond the scope of the present work, and will have to be left for another time.

Bibliography of Im/predicativity

This bibliography of im/predicativity makes no pretension to completeness, only to better its rivals, to which it nevertheless owes much. (A list of the works cited in this dissertation follows the present bibliography.) The Bibliography in Heinzmann 1986 served as a belated base, but our criteria clearly diverge. The *Philosopher's Index* has also been used, more by author's name than by subject. The lists in Vol. 6 (eds. Kista, v. Dalen, A. Troelstra) of the *Omega-Bibliography of Mathematical Logic* (ed. G.H. Müller) had to be sorted, and my success must have only been partial; thus there are undoubtedly articles on im/predicativity mentioned there not also mentioned here. There has been some difficulty formulating an unambiguous criterion, since many subjects overlap with im/predicativity, e.g. Kleene's early work on function quantifiers, Feferman's general work on progressions of theories, ordinals in proof theory. I have sought, for example, to include some but not all of the material on the determination of proof-theoretic ordinals of impredicative subsystems of analysis; (see instead the *Omega-Bibliography*). Finally, most of the entries in this bibliography are only partially concerned with im/predicativity, and it has not always been possible to give precise page specifications. It has also not been possible to verify every source.

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