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To cite this version:

Sébastien Gandon. Which arithmeticisation for which logicism? Russell onrelations and quantities in The Principles ofMathematics. History and Philosophy of Logic, 2008, 29 (1), pp.1-30. halshs-00299868

HAL Id: halshs-00299868

https://shs.hal.science/halshs-00299868v1

Submitted on 17 Jul 2008

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Which arithmeticisation for which logicism? Russell on relations and quantities in *The Principles of Mathematics*

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This article aims first at showing that Russell's general doctrine according to which all mathematics is deducible 'by logical principles from logical principles' does not require a preliminary reduction of all mathematics to arithmetic. In the *Principles*, mechanics (part VII), geometry (part VI), analysis (part IV-V), magnitude theory (part III) are to be all directly derived from the theory of relations, without being first reduced to arithmetic (part II). The epistemological importance of this point cannot be overestimated: Russell's logicism does not only contain the claim that mathematics is no more than logic, it also contains the claim that the differences between the various mathematical sciences can be logically justified – and thus, that, contrary to the arithmeticisation stance, analysis, geometry, mechanics are not merely outgrowths of arithmetic.

The second aim of this article is to set out the neglected Russellian theory of quantity. The topic is obviously linked with the first, since the mere existence of a doctrine of magnitude, in a work dated from 1903, is a sign of a distrust *vis-à-vis* the arithmeticisation program. After having showed that, despite the works of Cantor, Dedekind and Weierstrass, many mathematicians at the end of the XIXth Century elaborated various axiomatic theories of the magnitude, I will try to define the peculiarity of the Russellian approach. I will lay stress on the continuity of the logicist's thought on this point: Whitehead, in the *Principia*, deepens and generalizes the first Russellian 1903 theory.

1 Introduction

Is Russell's logicism to be taken as an extension of the arithmeticisation of mathematics? Worded as it is, the question calls for only one answer: an unambiguous yes. The German mathematician Christian Felix Klein, who is usually credited for having introduced the phrase 'arithmeticisation of mathematics' in a lecture delivered at the Royal Academy of Sciences of Göttingen in November 1895 (an English translation of which was published as early as 1896), regarded Weierstrass as the chief exponent of this mathematical tendency. He explained that the 'Weierstrassian method' consisted mainly of always demanding a logical justification where space intuition was formerly used in the proof:

> Gauss, taking for granted the continuity of space, unhesitatingly used space intuition as a basis for his proofs; but closer investigation showed not only that many special points still needed proof, but also that space intuition had led to the hasty assumption of the generality of certain theorems which are by no means general. Hence arose the demand for exclusively arithmetical methods of proof.[1]

[1] [Klein, 1896] p. 966.

Such a claim finds an echo in many passages of the *Principles*, where Russell explains at length the reason why his philosophy of mathematics is opposed to Kant's. For example, let us quote the very beginning of [Russell, 1903]:

> Not only the Aristotelian syllogistic theory, but also the modern doctrines of Symbolic Logic were [...] inadequate to mathematical reasoning [...]. In this fact lay the strength of the Kantian view, which asserted that mathematical reasoning is not strictly formal, but always uses intuitions, *i.e.* the à priori knowledge of space and time. Thanks to the progress of Symbolic Logic, especially as treated by Professor Peano, this part of Kantian philosophy is now capable of a final and irrevocable refutation.[2]

A parallel can thus be drawn between the two mathematicians, Weierstrass and Gauss, and the two philosophers, Russell and Kant. Gauss thought, as Kant did, that space intuition was essential to mathematics — Weierstrass showed, as Russell did, that space intuition must be rejected and replaced by logical argument. This distrust of any use of spatial intuition in mathematics, combined with the stress put on the logical structure of the proof, is enough to number Russell among the proponents of the arithmeticisation program. Moreover, in the very same article, Klein refers to Peano as a follower of the Weierstrassian method.[3] Given the close relationship between Russell and Peano, it is thus very difficult to exclude Russell's logicism from this movement.

But which program are we talking about exactly? Even at the time, many different methods could be characterised by a common stress put on logical proof and by a distrust of spatial intuition. Just before referring to the works of Peano, Klein, for instance, alludes to Kronecker's approach: 'we can introduce further refinements [to the Weierstrassian method] if still stricter limitations are placed on the association of the quantities [and] this is exemplified in Kronecker's refusal to employ irrational numbers'. Here, Klein counts Kronecker as an arithmetiser, among Cantor, Peano and Weierstrass! Given the great diversity of the mathematical and epistemological approaches developed by these three men, a program which gathers them all can only be very weak and very vague.

In fact, it seems that we can distinguish three different components of the arithmeticisation program, as it is described by Klein:

1) 'arithmeticisation of mathematics' can designate the purely negative effort to expel intuition from mathematics. In this very broad sense, the Lagrangian definition of differential coefficients in terms of the coefficients of the Taylor series associated with a function, for example, can be seen as belonging to this program, even if the approach of Cauchy was initially opposed to it.[4] This view, which undoubtely is an important constituent in the arithmeticisation movement, can be labelled the anti-Kantian standpoint, because Kant regarded intuition as an essential component of any mathematical thought.

[2] [Russell, 1903] p. 4; see [Russell, 1903] pp. 456-461 as well.

[3] Klein explains that in the 'efforts made to introduce symbols for the different logical processes, in order to get rid of the association of ideas', a 'special mention must be made of an Italian mathematician, Peano, of Turin'; see [Klein, 1896] p. 967.

[4] As I. Grattan-Guinness has shown [Grattan-Guinness, 2000], Lagrange's influence has been considerable, especially in England, throughout the XIXth Century — it has shaped the revival of the logical thought in the works of Peacock, De Morgan and the others, who make the most of the formal and algebraic aspect of Lagrangian thought.

2) More positively, 'arithmeticisation of mathematics' can refer to the concern about the logical structure of mathematical proof. All the 'arithmetisers' lay stress on the necessity to have gapless arguments. Of course, this preoccupation may be linked with the first one. But it does not have to be. Pasch, for example, who is credited as having been the first to completely axiomatize a geometry, was actually an empiricist, and regarded the points of his elementary geometry as some very small physical entities. Here, a stress put on rigour is backed up with a regulated use of intuition.[5] This component of the arithmeticisation movement, maybe its most well-known part, can be named the standpoint of the rigorization.

3) But there is another meaning of the phrase 'arithmeticisation of mathematics', which occurs in several different places in the Klein's article — for example, when he mentions Kronecker. The movement can be seen as a genetic approach, which aims at reducing mathematics to the theory of whole numbers, *i.e.* to arithmetic. Here, the demand is not only to come back to 'arithmetical methods of proof', but to come back to the theory of whole numbers, which is a much stronger requirement. For instance, Hilbert's axiomatization of geometry satisfies the first two conditions, but not the last one: a numerical model of the structure can of course be found, but Euclidean geometry is not about numbers and is not defined as an extension of arithmetic in [Hilbert, 1899]. I will call this last claim the genetic view.

We said above that Russell's logicism should be taken as an extension of the arithmeticisation of mathematics. But the brief analysis of Klein's seminal article showed us that the latter phrase has several different meanings, so that another question arises: to which arithmeticisation must Russell's logicism be related? In the *Principles* as in the *Principia*, Russell undoubtedly shares the first two claims: no spatial or temporal intuition must intervene in mathematics and every proof can and must be made logically rigorous. But did he share the third claim? Did Russell maintain in 1903 that all mathematics is founded on arithmetic?

We could be tempted to give, once more, a positive answer. Weierstrass, Cantor and Dedekind are famous for having constructed, each in their own way, the real field and real analysis from elementary arithmetic. Russell, after Frege, would have added his contribution to the construction, in defining whole numbers as sets of equinumerous sets and in founding therefore arithmetic on set theory. According to Russell, we would have not only this schema:

Real analysis and real numbers
Arithmetic and whole numbers

but also this stronger one:

Real analysis and real numbers
Arithmetic and whole numbers
Logic and set theory

The 'arithmetisers' succeeded in going from the second to the third level; Russell (after Frege) would have claimed to have pushed the reduction one step further and to have based the second level on the first one. Such an account is made by Russell himself at the beginning of his *Introduction to Mathematical Philosophy* (p. 5):

[5] And the converse is also true: the endeavour to get rid of intuitions can coexist with the use of loose arguments. Cauchy, for instance, objected to Lagrange for having used series without first investigating their convergence.

> Having reduced all traditional pure mathematics to the theory of the natural numbers, the next step in logical analysis was to reduce this theory itself to the smallest set of premisses and undefined terms from which it could be derived.

In spite of this passage, I would like here to qualify this standard presentation. In the sequel, I will set out some elements which tend to show that the genetic point of view (*i.e.* the claim that all mathematics can be reduced to arithmetic) is not shared by Russell in the *Principles*. In order to show that there are reasons to doubt the standard view, in order as well to offset the effect of the last quotation, let me first quote an unpublished letter from Whitehead[6] dated 14 September 1909:

> Dear Bertie,
>
> The importance of quantity grows upon further considerations — The modern arithmeticisation of mathematics is an entire mistake — of course a useful mistake, as turning attention upon the right points. It amounts to confining the proofs to the particular arithmetic cases whose deduction from logical premisses forms the existence theorem. But this limitation of proof leaves the whole theory of applied mathematics (measurement etc) unproved. Whereas with a true theory of quantity, analysis starts from the general idea, and the arithmetic entities fall into their place as providing the existence theorems. To consider them as the sole entities involves in fact complicated ideas by involving all sorts of irrelevancies — In short the old fashioned algebras which talked of 'quantities' were right, if they had only known what 'quantities' were — which they did not.
>
> The connection between analysis of metrical geometry is immediate — it is in fact the same thing.
>
> You see in short that I have recovered the simple faith of my angel infancy — I only hope that it is not a sign of decay of intellect or of approaching death — You will have to devote some attention to my MS — since their results will come as a shock to the current orthodoxy. In fact mathematicians will feel much like Scotch Presbyterians who might find that a theological professor in one of their colleges had dedicated his work to the Pope.
>
> Yours affectionately, ANW

Whitehead underlines here that 'the arithmeticisation of analysis is an entire mistake', and that 'the old fashioned algebra' of quantities is a better way of looking at the real numbers. How is one supposed to reconcile these statements with the conception according to which Russell and Whitehead's logicism was a development of the genetic position? If the standard view is correct, how are we supposed to deal with the content of this letter?

In the face of what appears as a plain contradiction, it could be tempting to minimise the importance of the document. Is this a letter from Whitehead, and not from Russell? Isn't it a private letter, and not an extract from the *Principles*, or from the *Principia*? However, as we will see, the challenge cannot be escaped that way: there is plenty of evidence that Russell, as early as 1900, agreed with the ideas exposed by Whitehead — what is more, there are plenty of

[6]The surviving part of the Russell-Whitehead correspondences is available for consultation in the Russell Archives at McMaster University, Hamilton.

published passages (not very much studied, to tell the truth) that either foreshadow or take up these ideas again.

I will here seek to take this Whitehead letter seriously, and try to link it with some more well-known features of the logicist project. More precisely, I will give three arguments which tend to show that the standard reading of Russell's logicism must be qualified. The first one aims to prove that the ideas developed by Whitehead in 1909 were not as shocking as he feigns to believe. Contrary to a widespread opinion, many mathematicians at the time were not at all convinced by the general relevance of the genetic point of view, and tried to algebraize and formalize the old theory of magnitude. Russell and Whitehead's position will be better understood, I think, once placed in this more refined context. The second argument (sections 3 and 4) will concern Russell's 1903 logicism as a whole. I would like to point out that the denial of the arithmeticisation program, understood as a genetic approach, is compatible with the logicist stance, according to which all mathematics are reducible to logic. Indeed, each mathematical discipline can be reduced independently to logic, without being first reduced to arithmetic. I will hold that this is exactly what happens in the *Principles*, where each traditional mathematical branch corresponds to a specific type of relation. Finally, in the last two sections of this paper, I will focus on the theories of magnitude elaborated by Russell and Whitehead. I will essentially speak about two texts. In section 5, I will examine the English version of *Sur la Logique des Relations*, from 1900 (See [Russell, 1900]), where a theory of groups and of magnitude is worked out. In section 6, I will briefly introduce the main ideas contained in part VI of the *Principia*, where Whitehead deepens the first Russellian theory. This detour by [Russell and Whitehead, 1913] justifies itself by the fact that it is fundamentally the same single theory that is expounded in 1913 and 1900, and also by the fact that part VI of the *Principia* shows that Whitehead's words in the letters above, far from being just idle talk, are shared by Russell, who co-authored the work.

2 The axiomatics of magnitude

At the turn of the last century, many important mathematicians (Stolz, Helmholtz, Burali-Forti, Hölder, Hilbert, Huntington, to mention a few), elaborated various axiomatic systems for what they called a theory of magnitude.[7] These various 'theories of magnitude' formed a little 'genre' in the mathematical literature of the time. Huntington, for instance, before offering his own construction in 1902, refered to the previous attempts and thus presented his own work as a part of a larger movement. All these axiomatics attributed to a certain set of entities (at least) a structure of a totally ordered semi-group. Sometimes an Archimedean condition was postulated; sometimes not (as in Stolz's contribution). For our present purpose, the fundamental point is that, in these theories, the elements belonging to the underlying set were not supposed to be numbers; all kinds of entities (segments, areas, volumes, weights, sensorial intensities...) could then be considered as magnitudes. The mere fact that these various scientists tried to formalise and precisely define the old notion of quantity shows that, at the time, magnitude was not universally regarded as a mere outgrowth of arithmetical concepts.

In order to give a more precise idea of what was a theory of magnitude, I list the axioms (translated in today's usual notation) given in a Burali-Forti's article from 1899. This article is important from our perspective, because it has been attentively read by both Russell and

[7] See [Stolz, 1885], [Helmholtz, 1887], [Burali-Forti, 1899], [Hölder, 1901], [Hilbert, 1899], [Huntington, 1902].

Whitehead.[8] This is the way Burali-Forti defines a set G of homogeneous magnitudes $<G, +>$:

1- $a, b \in G, a + b = b + a$

2- $a, b \in G, a + (b + c) = (a + b) + c = a + b + c$

3- $a, b, c \in G, (a + c = b + c) \Rightarrow a = b$

4'- $a \in G, \exists x \in G, a + x = a$

4"- $a \in G, \exists x \in G, a + x > a$

5 - $b \in G, a \in G\backslash\{0\}, (a + b) \in G\backslash\{0\}$ [9]

Definition of order: if $a, b \in G$ then $a > b$ iff $\exists x \in G\backslash\{0\}, a = b + x$

6- $a, b \in G, a = b \vee a < b \vee a > b$

7- $a \in G, \exists x \in G\backslash\{0\}, x < a$

8- If $U \subset G, U \neq \varnothing, \exists x \in G, \forall y \in U, y < x$, then:

$\exists z \in G, \forall y \in G, (y < z \Rightarrow \exists w \in U, y < w)$.

Observe that, here, no Archimedean postulate is formulated – but Burali-Forti says that the Archimedean condition can be derived from the eight axioms.

All these algebraic formalizations of the old Eudoxian theory raise the same question: why are these authors going back to a notion that the new arithmetical genetical theories have made redundant? It seems we can distinguish two underlying lines of thought. The first one, represented by Burali-Forti (who, once again, inspired Russell and Whitehead), is a strong philosophical program, in the sense that it explicitly aims to oppose the arithmeticisation view, according to which continuous magnitudes can be reduced to the whole numbers. Burali-Forti thus explained:

> Chapter I of this book contains the properties of the magnitudes which do not depend on the idea of number (integer or fraction or irrational). [...] Chapter II contains the basis of the theory of the whole numbers. The idea of a whole number is logically derived from the usual and concrete idea of magnitude. Once the sum of two integers defined, we found that the wholes numbers constitute a homogenous class of magnitudes; and once checked which are the primitive propositions about magnitudes which are also true for the integers, we already get an important group of propositions about the wholes numbers, by replacing the word magnitude by the word integer in the propositions of Chap. I.
>
> An analogous procedure is followed in chapters III and IV, devoted to the rationals and the irrationals.
>
> A first conclusion of the method we just exposed is the swiftness with which we can teach, at school, the formal properties of the algebraic operations, by including the elements magnitudes and numbers which are usually examined separately. *But*

[8] Whitehead alludes to this work in a second letter dated 28/1/1913: 'As to the preface -– The work on 'grandeurs' started with a study of Burali-Forti's articles in the *Rivista* and was directed initially to arrive at the same results. Of course his work is really based on Euclid Bk V — whom I ought also to have studied, but did not. Thus our antecedents are Euclid and Burali-Forti ; but it should be mentioned that (1) by the introduction of 'relations' and (2) by the keeping of the group idea in the background, and (3) by the separate treatment of ratio, and (4) by avoiding number and (5) by the introduction of cyclic groups, the subject has been entirely modified. I think these points should be mentioned somewhere, not to claim novelty, but to show people what to look for.'.

[9] Axiom 5 implies that the semi-group $<G, +>$ is strictly positively ordered.

> *another much more important conclusion is reached, which consists in making it possible to obtain the general idea of number in a concrete shape by deriving it from the concrete usual idea of magnitude*, which is essential for the metrical part of geometry as well. (We italicize).[10]

The Italian mathematician defined, in the second part of his work, a set of operations on magnitudes which satisfied the famous Peano's axioms. Burali-Forti took this to mean that the notion of whole numbers, and thus elementary arithmetic, should be derived from the notion of magnitude, and not the other way round, as the 'arithmetisers' suggested.[11]

However, many mathematicians who elaborated such a theory did not want to directly conflict with the genetic approach taken by Cantor, Dedekind and Weierstrass. As Hilbert said in 1900, the aim was only to supplement (not to refute) the genetic method (which derived $\mathbb{R}$ from $\mathbb{N}$), by an axiomatic one:

> If we cast an eye over the numerous works that exist in the literature on the principles of *arithmetic* and on the axioms of *geometry*, and if we compare them with one another, then, in addition to many analogies and relationships between these two subjects, we nevertheless notice a difference in the *method* of investigation.
>
> Let us first recall the manner of introducing the concept of number. Starting from the concept of the number 1, one usually imagines the further rational positive integers 2, 3, 4, ... as arising through the process of counting, and one develops their laws of calculation; then by requiring that subtraction be universally applicable, one attains the negative numbers; next one defines fractions, say as a pair of numbers [...]; and finally one defines the real number as a cut or a fundamental sequence [...]. We can call this method of introducing the concept of number the *genetical method*, because the most general concept of real number is *engendered* [*erzeugt*] by the successive extension of the simple concept of number.
>
> One proceeds essentially differently in the construction of geometry. [...] We raise the question whether the genetical method is in fact the only suitable one for the study of the concept of number, and the axiomatic method for the foundations of geometry.[12]

An axiomatic theory of the real field, taken from sections III-V of the *Grundlagen*, is then presented. Hence, unlike Burali-Forti, Hilbert did not question the legitimacy of the Dedekindian method. Instead, he just wanted to stress the possibility of using another approach, the axiomatic one, which he had already applied to geometry. The idea seems to be that adopting the same method to investigate the principles of both arithmetic and geometry can reveal many otherwise hidden 'analogies and relationships' between the two subjects. But why seek to stress the analogies between arithmetic and geometry?

To understand what appears to be the central tenet of this second line of thought, we must remind ourselves of the very important development, throughout the XIXth century, of the geometrical calculi. People like Grassmann, Möbius, von Staudt defined operations like addition and product directly on geometrical entities (points, bipoints, lines ...). The German mathematician

[10] [Burali-Forti, 1899] p. 34.

[11] In the same connection, Burali-Forti explained that he did not formulate the Archimedean postulate so as to keep the notion of magnitude separate from any reference to the whole numbers.

[12] [Hilbert, 1900] p??.

Christian von Staudt is here especially important because he was the first to use the algebraic properties of these operations to define an isomorphism between what we will call today the field of the real numbers and the points of the projective lines – for him and for his followers (*e.g.* Klein, but, among others, Russell as well), this key-result allowed the introduction of coordinates and numerical quantities in geometry, and thus legitimated, from a synthetical standpoint, the practice of the analytical geometers.[13] In this tradition, geometry and analysis must be kept apart: numbers and geometrical entities were not regarded as the same thing. But there was an analogy between the two fields, which explained the reason why numerical magnitudes could be used in geometrical investigations.

The reference to the 'many analogies and relationships' between arithmetic and geometry Hilbert makes at the beginning of his [Hilbert, 1900] seems precisely to point to the discussions about the coordinates introduction. Hilbert explicitly bought up the issue of the coordinates when he presented his segment's calculus, in the very section from which his *Über den Zahlbegriff* is extracted.[14] For him, as for the followers of von Staudt, the distinction between geometry and arithmetic (or analysis) seems to have been taken as a datum, that could not be denied. The question was then to explain how, given the deep difference between their respective subject-matters, numbers could be used in geometry. The genetic method, since it denies that there is an irreducible difference between numbers and magnitudes, appeared to be too strong to carry out the task; only the axiomatic approach could account for 'the many analogies and relationships' between what was still recognised as two different domains.

One of the central issues raised by all this kind of work concerned the nature of the algebraic structure common to both numerical and geometrical magnitudes. This question was especially difficult and widely discussed, in view of the fact that the mathematicians who elaborated these geometrical calculi tended to oppose geometrical to numerical magnitudes, and elaborated their calculus precisely in order to purify geometry from any 'numerical taint' (see [Russell, 1903] p. 421). Axiomatising the theory of magnitude seems to have constituted a first answer to this problem. Many things, as different as numbers, segments, bipoints, functions, sensorial intensities... were, were traditionally called 'magnitudes', and this common feature, shared by all of them, did not prevent them from being recognised as belonging to very distinct kinds of things. The traditional 'neutral', cross-disciplinary character of the idea of magnitude was then crucial: to say that numbers and segments (for instance) were magnitudes did not amount to assimilating numbers and segments to a third kind of thing, because magnitudes were not put on the same footing as numbers or segments. To claim, on the other hand, that segments can be reduced to numbers was a completely different affair, which led to blurring the traditional distinctions between the mathematical fields. It seems that for the mathematicians of the second group, the reasoning of the 'arithmetisers' was objectionable precisely on this point. Constructing

[13]For a modernised presentation of Staudt's *Würfe* algebra, see [Coxeter, 1949] pp. 145-158. For an account of von Staudt's work in real projective geometry and his influence on M. Pieri (whose research had a big impact on Russell's thought), see as well [Marchisotto, 2006].

[14]On the fundamental role played by the segment calculus in the *Grundlagen*, see [Rowe, 2000]. Hilbert's early project is usually exclusively viewed from the perspective of the later development of the model theory — in stressing the connection between the *Grundlagen* and the contemporary foundational research in projective geometry, Rowe invites us to change the way of looking at this epoch-making book; see [Rowe, 2000] p. 55: 'Whereas many of Hilbert's commitments and some of his germinal ideas can be traced back to the turn of the century, very little of his early work had any direct connections with set theory or logic. [...] A far clearer picture of Hilbert's views on foundations emerges, I believe, if we widen our lens a bit by considering how his "philosophical" leanings and "metamathematical" commitments related to concerns that arose in the context of Hilbert's own mathematical research.'

an axiomatic theory of magnitude represented the means to uncover 'the many analogies and relationships' between the various mathematical branches, without, however, destroying the usual boundaries between them.

To summarize: unlike Burali-Forti, certain mathematicians who developed axiomatic theories of magnitude, did not want to reduce numerical to geometrical magnitude; they only wanted to express the formal relationships between different mathematical areas without loosing the distinctive features of the mathematical subdisciplines. For the supporters of a purely synthetical approach of geometry, 'arithmeticisation' was a reductive stance, that represented the same kind of threat as the unrestrained use of the analytical methods, and was exposed to exactly the same criticism: to introduce inessential foreign elements (numbers) into the topic under investigation.[15]

Thus, at the time of the writing the *Principles*, the arithmeticisation of analysis was not the only available way to think about quantities. The mathematical world was more complex and less uniform than it is too often thought today, and to take into account the complexity of the situation is a first step toward a more balanced understanding of Russell's view. Around 1900, there was in fact a very deep tension between the arithmeticisation program and the tradition of synthetical geometry that aimed to expel numbers from the geometrical universe. Considering their previous commitments in the foundations of geometry, Russell and Whitehead could not have ignored this important issue. Cantor and Dedekind's works were far from being the first and only contact between Russell and the mathematical world. Remember that, in his first book on the foundations of geometry, Russell sides with Klein and von Staudt against the analytical (which Russell called Riemannian) conceptions, which regarded space as a numerical manifold. And likewise, remember as well the intense involvement of Whitehead with the Grassmannian tradition (Whitehead's *Treatrise of Universal Algebra* is a deepening of the *Ausdehnungslehre*). This geometrical common background, shared by the two logicists, must be taken into account when interpreting the *Principles*.

3 The several distinct uses of the theory of relation (I): the 'type' of a relation

I will here leave for a while the topic of quantity in order to confront directly the standard view, according to which Russell followed Cantor, Weierstrass and Dedekind in founding all mathematics on arithmetic. The usual interpretation attributes to Russell the following three-levelled schema (labelled, in the sequel, the 'umbilical schema' because arithmetic is presented there as the only channel through which logic and mathematics interact):

Analysis – Part III-VII
Arithmetic – Part II
Logic: theory of relations, of classes and propositions – Part I

[15]Today, thanks to the emergence of the purely formal theories of algebraic structure during the 1920s and the 1930s, we do not any longer require the idea of magnitude to analyse the common features between mathematical theories — magnitude is just, for us, a kind of ordered semi-group, *i.e.* a very particularised algebraic structure. But let us remember that at the beginning of the century, the main algebraic concepts (group, field, etc...) were still regarded as tools that could be used to resolve what was still conceived as arithmetical or geometrical problems. The investigation of the properties of a structure invariant under some extension or some restriction of the underlying set was thus not systematically undertaken: algebraic structures were not yet a proper mathematical object (For more on this topic, see [Corry, 1996] and [Sinaceur, 1999]). At the beginning of the last century, the old idea of 'magnitude' could have been a means (not the only one, for sure) to fill the gap.

At the first level, we would find logic[16] (part I of the *Principles*); at the second one, arithmetic (part II of the *Principles*), and at level three, analysis and the rest of mathematics (all the remaining parts). According to this view, Russell wished to secularize Kronecker's words 'God made the integers, all else is the work of men', since in the *Principles*, it seems as if the theory of relation and set theory could replace God in creating the natural numbers. But such an assertion of the power of logic has a drawback: once the whole numbers were 'created', nothing logically interesting seemed to happen in the remaining parts of Russell's work. This last point is important. The idea that arithmetic is the umbilical cord between mathematics and logic induces a certain reading of *The Principles*. The belief that logic intervenes only in the first two parts of the [Russell, 1903] seems to imply that the rest of the work constitutes a popularized summary of already done and well-known deductions — two-thirds of [Russell, 1903] would then be quite uninteresting, being already completely redundant at the time.[17] In other words, in this interpretation, the true import of the *Principles* would not be the logicist stance (according to which mathematics is nothing else than logic), not even the logical definition of the cardinal numbers in part II (since it has already been done by Frege), but only the realization that logic itself involves serious difficulties (*e.g.* the discovery of the paradoxes).

Of course, this reading could find some support from the way Russell directs his subsequent work (after 1903) exclusively toward logical topics. Nevertheless, it is a fact, which will soon be documented, that, in the *Principles*, the theory of relations, *i.e* the core of the new logic according to Russell in 1903 (see note 16), is not confined to the first and the second part of the book, but underlies the whole work. If logic is the Russellian God, it is then a Cartesian God, which intervenes not only once, but all the time and everywhere. The theory of relations plays a part in the construction of the finite cardinals; but it intervenes also in the doctrine of order in part IV and V ; it pushes in as well in part VI, in the definition of the concept of space as an incidence structure (for more on this point, see section 4 below) ; and, finally, in part VII, in order to construct the fundamental concept of occupation of a place at a definite time, Russell needs once again to come back to logic, to guarantee the logical possibility of three-terms relations.[18] This simple remark leads us to put forward an alternative schema (labelled 'tree schema', because the theory of relations appears there like the tree trunk from which all the various mathematical branches develop themselves):

[16] Russell does not in 1903 define precisely what logic is. Part I of the *Principles* is a collection of various formal theories: the propositional calculus, the calculus of class, the calculus of relations – and of various (sometimes very sketchy) 'doctrines' about the fundamental logical concepts: the doctrine of denoting, of classes, of propositional functions... Moreover, Russell does not yet seem to size the bearing of the contradiction up – that is, in 1903, he does not seek to separate what we would call today the predicate calculus from the theory of class (set theory is a part of logic for Russell at the time). Of course, this greatly confuses the situation for us. But despite all these complications, one thing seems clear: the great importance lent to the theory of relation (which Russell claims to have discovered) for the analysis of mathematics. Thus Russell explains that 'a careful analysis of mathematical reasoning shows [...] that types of relations are the true subject-matter discussed' and, hence, that 'the logic of relations has a more immediate bearing on mathematics than that of classes or propositions, and [that] any theoretically correct and adequate expression of mathematical truth is only possible by its means.' [Russell, 1903] pp. 23-24. Since it plays the essential role in logicism, we will in the sequel focus exclusively on the doctrine of relations.

[17] The fact that the mathematical parts of [Russell, 1903] are much less analysed than the first two parts of the book seems to be the direct consequence of the standard 'umbilical view'.

[18] See on this point [Russell, 1903] pp. 465-473; and especially, p. 472: 'We must examine the difficult [but fundamental, as far as the part VII is concerned (see pp. 465-466)] idea of occupying a place at a time. Here again we seem to have an irreducible triangular relation. If there is to be motion, we must not analyze the relation into occupation of a place and occupation of a time.'.

Arithmetic, II	Theory of magnitude, III	Analysis, IV-V	Geometry, V	Mechanics, VI
Logic: theory of relations, of classes and propositions, I				

Here, and contrary to the umbilical diagram, we no longer have various disciplines, first all reduced to arithmetic, and only subsequently reduced to logic. Instead, we have various mathematical branches, all independently and directly derived from logic. Before precising the relation between logic and the various mathematical fields in the 'tree view', and before presenting the reasons why we believe that this schema is more faithful to the structure of [Russell, 1903] than the previous one, let us make a few comments.

Firstly, this last view is still a kind of reductionism. In the 'tree schema' as in the 'umbilical' approach, all the various mathematical subjects are derived from the same fundamental theory (the theory of relations and set theory). From both points of view, Russell's project is a kind of reductionism. Secondly, only the first view fits well into the genetic arithmetical conception according to which all mathematics is reducible to arithmetic. Indeed, in the 'tree schema', the boundaries between the mathematical subfields are respected. If all mathematics is certainly derived from logic, each usual mathematical branch appears different from the other ones. In particular, arithmetic does not enjoy any special status. The very core of the arithmeticisation program, which consists in extending the field of arithmetic outside its traditional domain (toward analysis and geometry in particular) is here therefore missing. This is a simple but often overlooked point. Logicism need not endorse the arithmeticisation program (in its genetic version). Claiming that all mathematics can be reduced to logic and claiming that all mathematics can be reduced to arithmetic are two different assertions, neither of which implies the other.

Even better, and this will constitute my third and last preliminary remark, the 'tree view' not only respects, but in some ways legitimates, the traditional distinctions between mathematical fields. As we said above, in the *Principles*, the way in which Russell uses logic in part II, devoted to cardinal arithmetic, differs from the way he uses it in parts IV and V, dedicated to analysis — which itself differs again from the way Russell uses it in the part VI, devoted to geometry. The logic developed in the various parts is each time specific: equivalence relations are the subject-matter of part II, order relations are analysed in part IV, incidence relations (see below for more on this topic) in part VI, and three-terms relations in part VII. To each division of the *Principles*, that is to each box in the top line of the 'tree schema' corresponds a specific branch of the theory of relations. That is to say, the usual distinctions inside mathematics (between analysis and arithmetic for example) are, for Russell, based on some deep logical distinctions, and it would then be a logical mistake to reduce any one of them to some other (for example, to reduce analysis and order relations to arithmetic and equivalence relations). This point goes directly against the fundamental tenet of the arithmeticisation standpoint. To overstate, we could say that, in the 'tree schema', it is as if the reduction of mathematics to logic was a means to restore, against the threat represented by the reduction of all mathematics to arithmetic, the old boundaries inside mathematics.

Until now, we have only presented two ways of looking at the general structure of Russell's *Principles* (one standard, the other one more unusual), and made some remarks on the significance of the second one, without arguing for one interpretation against the other. It is now time to put forward our reasons to favour the 'tree view'. I will distinguish two different arguments. The first one is general, and aims at showing that Russell has got the means, in the *Principles*, to logically differentiate the several uses of the theory of relations he makes throughout his book. The second one, to which section 4 (and in fact, the two last sections as well) is dedicated, is

more specific: I will try to show that every part of the *Principles* (except the first one) can be seen as a theory of a specific kind of relation.

Let us first indicate the general feature that gives some support to the 'tree approach'. It is a fact that [Russell, 1903] is organised in accordance with the (at the time) usual division of mathematics into arithmetic, analysis, geometry and mechanics. The apparent structure of the *Principles* is, as a matter of fact, very traditional, and does not seem to support any reductionist view of mathematics. Compare for example the table of contents of [Russell, 1903] with the one of the Van der Waerden's 1930 *Algebra*: in the latter, a deep and complete upheaval of the traditional division of the mathematical field (which, by the way, has been said to have been foreshadowed by Dedekind[19]) is introduced. There is nothing comparable in the *Principles*, whose global structure keeps up the usual architecture of the mathematics, without offering anything new. In particular, Russell does not follow the genetic conception in presenting all the mathematical disciplines as some extensions of arithmetic.

But, as important as it is, this sole fact does not prove that Russell regarded the usual division of the mathematical whole as something that can be logically vindicated. Russell could just have found it convenient to take up the usual presentation, without being committed to a particular epistemological position. This interpretation is however ruled out by the introduction, at the very beginning of ([Russell, 1903], §8), of an important idea, the relational type:

> Whenever two sets of terms have mutual relations of the same type, the same form of deduction will apply to both. For example, the mutual relations of points in a Euclidean plane are of the same type as those of the complex numbers; hence plane geometry, considered as a branch of pure mathematics, ought not to decide whether its variables are points or complex numbers or some other set of entities having the same type of mutual relations. Speaking generally, we ought to deal in every branch of mathematics, with any class of entities whose mutual relations are of specified type; thus the class, as well as the particular relations considered, becomes a variable, and the only true constants are the types of relations and what they involve. Now a *type* of relation is to mean, in this discussion, a class of relations characterized by the above formal identity of the deductions possible in regard to the various members of the class; and hence a type of relations, as will appear more fully hereafter, if not already evident, is always a class definable in terms of logical constants.[20]

The 'type' of a relation does not here designate a kind of variable, as it will do in the *Principia* — it has nothing to do with the paradoxes and the type theory. A type, in the sense of the *Principles* §8, is a class of relations characterised 'by some property definable in terms of logical constants alone', that is, a class of relations characterized by some formal, purely logical, property. Thus for example, the equivalence relations (*i.e.* the symmetrical, reflexive and transitive relations) and the order relations (*i.e.* the transitive asymmetrical relations) are instances of relational types. A purely formal theory of such relations can be constructed, and Russell actually does present such a theory in part II and part IV of his book.

Now, in the passage just quoted, an explicit connection is made between the relational 'type' and the subject-matters of the various mathematical disciplines: 'speaking generally', says Russell, 'we ought to deal in every branch of mathematics, with any class of entities whose mutual

[19] On the link between Dedekind and the emergence of the modern algebra in the thirties, see [Sinaceur, 1999].
[20] [Russell, 1903], pp. 7-8.

relations are of specified type'. Thus, since 'the mutual relations of points in a Euclidean plane are of the same type as those of the complex numbers', Russell claims that 'plane geometry, considered as a branch of pure mathematics, ought not to decide whether its variables are points or complex numbers'. And indeed, complex numbers are tackled, in [Russell, 1903], in the chapter XLIV of part VI, devoted to geometry. From the same kind of reasoning, it follows as well that the distinctions between the different branches inside mathematics should be regarded as based on the differences between various logical types (in the sense of *Principles* §8): every mathematical discipline is characterised by a type.[21] Therefore, far from being only a convenient means to present mathematics, the division of the book in as many parts as there are different mathematical branches must be seen as a very significant feature. It means that the traditional boundaries between the various areas are turned into fundamental logical distinctions, *i.e.* into 'type' distinctions (distinction between equivalence and order relations, between order and three-terms relations, etc...). Thus, as we have suggested before, the logical reductionism of the *Principles* is not only compatible with a dismissal of the arithmeticisation program, but represents also a criticism of it: to base the difference between the mathematical disciplines on some type distinctions appears to preclude reducing any given mathematical doctrine to any other (for example, geometry to arithmetic). Indeed, such a move would involve a logical mistake, since it amounts to missing a genuine logical distinction between 'types' of relations.

Our first general argument in favour of the 'tree view' is thus the following: Russell developed the means, in his book, to logically differentiate the various mathematical branches. Every singled out discipline must match, says Russell in the *Principles*, a particular relational type. Logic (theory of relation) is thus not only the basis from which all mathematics can be derived — logic is as well the basis from which all the differences between the traditional mathematical subject-matters can be derived. The division Russell makes inside mathematics should not then be regarded only as a superficial and convenient way of presenting the mathematical content, but as expressing an important philosophical feature of Russell's 1903 book.[22]

4 The several distinct uses of the theory of relation (II): Analysis and Geometry in the *Principles*

But is Russell's claim true? Can we really relate every part in [Russell, 1903] (that is, in order of appearance, arithmetic in part II, theory of quantity in part III, analysis in part IV and

[21] It should be observed that the logical notion of type is not only defined in the passage just quoted as a class of relations, but also as a form of deductions. Hence, mathematical branches are distinguished not only by their primitive terms, but also by the way their proofs proceed — by their method. In other words, Russell seems to claim that the pursuit of the 'purity of method', *i.e.* the attempt to keep the proofs of a particular area free from 'foreign' elements, is a task that is met by his program: a type is a (logical) definition of what makes a specific branch so peculiar.

[22] I do not want to suggest here that Russell shared an architectonic view of mathematics, according to which the division of the mathematical field could be deduced from the theory of relations alone, a bit like, in Kant's philosophy, the categories should be deduced from the form of judgement alone. The relational types analyzed in the *Principles* are not the only ones possible, and the reasons why Russell chooses to focus on some rather than others are purely factual: it just happens that mathematics, at the times, puts certain relational types (equivalence relation, order, incidence, ...) forward, to the detriment of others. It could have been different. For instance, in the *Principles*, Russell says only a few words about the relational property of connectivity (see [Russell, 1903] pp. 239-240). But he does not really work out in detail the theory of such relational type. Why? Because it just happens that mathematics at the time was not required to develop such a theory. We could think that if the graph theory (for exemple) had been created, Russell would have built a much more refined doctrine of the relational connectivity – as in fact Carnap, in his *Aufbau*, does. The theory of relation does not in itself contain one particular 'system' of relational types. My contention is not that the theory of relation fits in particulary well with the traditional architecture of the mathematical sciences, but that Russell uses it to uncover the historically given differences between the mathematical disciplines.

V, geometry in part VI, mechanics in part VII) to a specific relational type? To answer this question, we have to leave out the overall structural analysis of the *Principles* and turn toward the architecture of each of its parts. We have already suggested that the theory of relations intervenes everywhere in [Russell, 1903]: in part II, with the doctine of equivalence relations; in part IV-V, with the order relations; in part VI, in the guise of incidence relations (see above); and in part VII, with the three-terms relations. These simple statements lead us to propose a correlation table between branches and relational types:

Arithmetic II	Magnitude III	Analysis IV-V	Geometry V	Mechanic VI
Equivalence relation	?	Order relation	Incidence relation	Three-terms relation

To nearly[23] each subdiscipline corresponds thus a type of relation. Of course, in order to prove that this schema is faithful to the actual working of the *Principles*, a lot of further work would be needed — we should study each book and verify that the correlations established fit the fact. Such a work would be a task beyond the limits of this paper, and I cannot hope better here than to make appear plausible what will remain a rough sketch.

It seems to me that the correspondence made between part II and equivalence relation is the easiest to vindicate. Furthermore, the relation between part VII and three-terms relation is explicitly acknowledged by Russell, and in any case, mechanics seem to be less essential to the logicist program than the other topics. We will then leave aside these two points. Three cases remain. In this section, I will focus on the relations between analysis and order, and between geometry and incidence. The last two sections of the paper will be devoted to the remaining correlation, *i.e.* to the neglected Russellian analysis of quantity.

Let us first consider analysis and order. The issue is crucial in respect to the arithmeticisation program, since the main claim of the genetic view is that analysis can be derived from arithmetic alone. Does Russell support the claim? At first sight, this seems to be the case. Part V of the *Principles* is indeed a very long plea for the Cantorian approach of continuity and infinity. But Russell accompanies these developments with some local criticisms. He is thus very careful in presenting the notion of continuity as an ordinal, not as a metrical one.[24] More generally, Russell seems very anxious to distinguish between cardinal and ordinal properties. For instance, contrasting his way to construct $\mathbb{N}$ from Dedekind's one, Russell says in a crucial passage:

> It is plain [...] that the logical theory of cardinals is wholly independent of the general theory of progressions, requiring independent development in order to show that the cardinals form a progression [...]. If Dedekind's view were correct, it would have been a logical error to begin, as this work does, with the theory of cardinal numbers rather than with order. For my part, I do not hold it an absolute error to begin with order, since the properties of progressions, and even most of the properties of series in general, seem to be largely independent of number. But the properties of number must be capable of proof without appeal to the general properties of progressions, since cardinal numbers can be independently defined, and must be seen to form a progression before theorems concerning progressions can be applied to them.[25]

If the cardinal numbers form a progression, the concept of a progression itself is independent of the set-theoretical definition of the cardinals. As the definitions of both the order type η of the

[23] For an explanation of the question mark under 'Magnitudes', see section 5 below.
[24] See for instance [Russell, 1903], chapter XXXVI.
[25] [Russell, 1903], p. 251.

rational numbers, and the order type θ of the real numbers, are founded on progressions (see [Russell, 1903] pp. 296-303), the entire doctrine of continuity is then independent of cardinal arithmetic. This means that if 'cardinal numbers can be independently defined', the properties of series in general (*i.e.* the notions of limit, continuity, ...) are 'largely independent of numbers'. The idea that analysis, as a theory of order, must not be derived from cardinal arithmetic is then explicitly vindicated.

Analysis, for Russell, can of course be applied to numbers — but it should not be limited to this case. What is essential to analysis is the ordinal relations between the elements, not their nature. Russell repeats exactly the same idea in the *Principia*. At the beginning of the section entitled 'On convergence, and the limits of functions' and specifically devoted to mathematical analysis, we found this noteworthy comment:

> In the definitions usually given in treatises on analysis, it is assumed that both the arguments and the values of the functions are numbers of some kind, generally real numbers, and limits are taken with respect to the order of magnitudes. There is, however, nothing essential in the definitions to demand so narrow a hypothesis. What is essential is that the arguments should be given as belonging to a series, and that the values should also be given as belonging to a series, which need not be the same series as that to which the arguments belong. In what follows, therefore, we assume that all the possible arguments to our function, or at any rate all the arguments which we consider, belong to the field of a certain relation Q, which, in cases where our definitions are useful, will be a serial relation.[26]

Here again, Russell emphasizes the idea that the real subject-matter of analysis is order, not numbers. If analysis can be applied to cardinal numbers, it is only because these numbers form a certain kind of series. Contrary to what is assumed in the usual treatises, analysis has essentially nothing to do with numbers *qua* numbers.

This distinction between two structures, the cardinal and the ordinal one, can be seen as a blow to the genetic view. Indeed, for Russell in 1903 as in 1912, analysis is not at all an outgrowth of cardinal arithmetic. Quite the contrary: constructing analysis requires an elaboration of the doctrine of order relations, a doctrine which is not needed to define the cardinal numbers. Thus, concerning analysis, our hypothesis seems to stand out: analysis appears to deal with a different type of relation than arithmetic.[27]

The case of geometry is, at first sight, more intricate. Before entering into the difficulty, let us note that at least one thing is clear: Russell does not regard space as a numerical manifold,

[26] [Russell and Whitehead, 1912], pp. 687.

[27] In order to account for the difference between Russell's and Dedekind's definition of $\mathbb{N}$, several scholars underline the fact that Russell, contrary to Dedekind, wanted to explain how we use numbers outside mathematics. In [Russell, 1903] p. 241, for example, Russell wrote: 'it is [the cardinal numbers defined by the equivalence relations] that are used in daily life, and that are essential to any assertion of numbers. It is the fact that numbers have these logical properties that makes them important. But it is not these properties that ordinary mathematics employs, and numbers might be bereft of them without any injury to the truth of Arithmetic and Analysis'. In so far as arithmetic is here viewed as a theory of ordinals, such a passage seems to go against my argument. However, even in this case, the essential fact that Russell seeks to differentiate two structures that Dedekind and the others confused, would still stand. The boundary would run inside arithmetic (which would be divided in a cardinal arithmetic and in a ordinal, purely mathematical, arithmetic), instead of separating arithmetic from analysis; but the idea that there is a distinction, overlooked by Dedekind, would still remain. But in fact, despite what Russell says in the passage just quoted, it is not quite true that arithmetic is, in the *Principles*, only a theory of ordinal numbers: addition and multiplication between numbers are defined in part II, and in many texts, Russell speaks about the cardinal arithmetic as a purely mathematical theory (see for instance [Russell, 1903] pp. 245-253).

and does not reduce geometry to analytical geometry. This fact is worth emphasizing. Indeed, it seems that, once the doctrine of continuity is completed, nothing would be lacking to develop geometry. We thus easily could have thought that Russell, in the *Principles*, defined the Euclidean plane as the two-dimensional manifold $\mathbb{R}^2$ endowed with the Euclidean metric, and regarded any point of the Euclidean space as a couple of real numbers. But that is not the case. The philosopher still sticks to his former opposition to analytical geometry, and still rejects the analytical methods (see section 2). Part VI of the *Principles* is thus not the continuation of part V — space is not defined there as a continuous n-dimensional numerical manifold.

The difficulty arises as soon as we leave this negative stance and try to understand what is the nature of geometry in the *Principles*. Russell says that geometry deals with space (see [Russell, 1903] p. 372); but he does not seem clear about what space is. I think, however, that a very interesting definition could be extracted from Russell's remarks.

Russell emphasizes the fundamental importance of the work of Pieri, a v. Staudt follower, on projective geometry (see [Russell, 1903] pp. 381-392). Now Pieri showed that the whole three-dimensional projective geometry can be developed solely from the incidence relations between points, straight lines and planes, and that, in particular, no primitive order relations were required in the projective framework — more exactly, order relations can be defined from the incidence relations (see [Pieri, 1898]).[28] I argued elsewhere at length[29] that Russell takes this result to mean that projective geometry is only concerned with incidence structures. According to Russell, in order to have a space, you need neither order, nor continuity (nor even to have a infinite numbers of points[30]) — you just need to have some incidence relations between at least two distinct sort of elements, a (not necessarily infinite) number of straight lines and points. In other words, space is defined, by Russell, as a multidimensional series, that is as a set of series which can cut each other.[31]

Whitehead will give voice to this line of thought in his *The axioms of projective geometry* from 1906,[32] where he defines geometry as a science of cross-classification, which is his name for incidence relations:

> Geometry, in the widest sense in which it is used by modern mathematicians, is a department of what in a certain sense may be called the general science of classification. This general science may be defined thus: given any class of entities K, the subclasses of K form a new class of classes, the science of classification is the study of sets of classes selected from this new class so as to posses certain assigned properties. For example, in the traditional Aristotelian branch of classification by species and genera, the selected set from the class of subclasses of K are (1) to be mutually exclusive, and (2) to exhaust K; the subclasses of this set are the genera of K; then each genus is to be classified according to the above rule, the genera of the various genera of K being called the various species of K; and so on for subspecies, etc. [...]

[28] For more on Pieri, see [Marchisotto, 1995] and [Coxeter, 1949].

[29] See [Gandon, 2004]; my conclusion is founded not only on Russell's use of Pieri's work, but on the analysis of the diachronic development of Russell's thought from the *Essay* to the *Principles*.

[30] Russell in 1903 does not allude to the finite geometries; but Whitehead in his *1906*, inspired by the *Principles*, explicitly does.

[31] See [Russell, 1903] p. 372: '*Geometry is the study of series of two or more dimensions*'.

[32] Whitehead refers explicitly to [Russell, 1903] in his preface: 'For a full consideration of the various logical and philosophical enquiries suggested by this subject, I must refer to Mr Bertrand Russell's *Principles of Mathematics*.'

> Geometry is the science of cross classification. The fundamental class K, is the class of points; the selected set of subclass of K is the class of (straight) lines. This set of subclasses is to be such that any two points lie on one and only one line, and that any line possesses at least three points. These properties of straight lines represent the properties which are common to all branches of the science which usage terms Geometrical, when the modern Geometries with finite numbers of points are taken account of.[33]

By contrasting the Aristotelian with the geometrical classification, Whitehead seeks to grasp what is logically essential in incidence relations — in other words (those used by [Russell, 1903] §8), he tries to uncover the relational type of this kind of relation. At the same time, Whitehead explicitly characterises geometry as the theory of incidence relations. We thus found explained here the very conception we attributed above to Russell.

But there is more. In the *Principles*, Russell differentiated two non-metrical geometries: the projective theory he favoured (exposed in the chapter XLV), and the descriptive geometry, based on the works of Pasch (see [Pasch, 1882]) and Peano (see [Peano, 1894]) (exposed in the chapter XVLI). Again following the path, Whitehead adds *ibid.*, p. 6:

> In Projective Geometry the subject viewed simply as a study of classification has great interest. Thus in the foundations of the subject this conception is emphasized, while the introduction of 'order' is defered. The opposite course is taken in Descriptive Geometry [...].

Here, Whitehead brings out the special connection between projective geometry and geometry itself. It is because projective theory can be developed as a pure doctrine of incidence that it expresses the essence of what makes a theory geometrical. We are thus brought back to a Russellian topic dated from the *Essay* onward: projective geometry is metageometry, *i.e.* it contains what all geometries have in common. But Whitehead explicitly contrasts as well the projective (Pieri's theory in [Russell, 1903]) with the descriptive geometry (Pasch's theory in [Russell, 1903]), and his remark helps to grasp the meaning and the great significance of Russell's complicated reasoning. In effect, from a purely mathematical perspective, projective geometry could have been founded on order relations. In 1903, Russell based descriptive geometry on ordinal postulates, and stressed, after Pasch, that projective geometry can be derived from the descriptive theory alone; see [Russell, 1903] pp. 400-403. If Russell (and Whitehead after him) chose to favour Pieri's approach, it was then not for technical reasons (Pieri's way was not the only available way to develop projective geometry), but only for epistemological ones: the projective method was much more closely linked with the nature of space, defined as multidimensional series, than the descriptive one. In Russell's *Principles* as in Whitehead's *Tract*, geometry (and projective geometry) is therefore knowingly set apart from any doctrine of order.

So to sum up: just as analysis, as theory of order, is different from arithmetic (based on equivalence relation), geometry, as science of cross-classification, is distinct from analysis. Geometry is not at all viewed in the *Principles* as an outgrowth of analysis. The logical gap between part VI and part V reflects the difference between two distinct relational types (in the sense of [Russell, 1903] §8): order relations and incidence relations.

[33] [Whitehead, 1906] pp. 4-5.

The last two analyses fit the 'tree view' well: each mathematical branch seems to really correspond to a particular logical relational type. Of course, a lot would be needed to rigorously prove the truth of this contention. But I hope to have made less unlikely the claim that Russell in 1903 did adapt his use of the theory of relation to the specificity of the mathematical discipline he was defining. And I hope as well to have made more probable the assertion that Russell did not adhere to the genetic point of view, according to which all mathematics is reducible to arithmetic.

It could however be objected that arithmetic had nonetheless one peculiar role, which is related to its use in the proof of existence-theorems. As Russell said:

> The existence-theorems of mathematics — *i.e.* the proofs that the various classes defined are not null — are almost all obtained from Arithmetic.[34]

To mention a typical example, Russell proved that the class of the series of type θ is not null by proving that the ordered set of the cuts on the rational numbers ordered field is a series of type θ.

There is a great temptation to look at the idea of existence-theorem from the standpoint of the model theory, still to come at the time. We must, however, resist it. The use of existence-theorems in the *Principles* is indeed very idiosyncratic.[35] For Russell, existence-theorems are needed because the logical definitions used in the derivation process are 'always either the definition of a class, or the definition of the single member of a unit class' *ibid.*, p. 497. The existence-theorems proved that 'the various classes defined are not null'. So in [Russell, 1903], the notion of existence-theorems is essentially linked with a special theory of definition, and not, as is more usual today, with the need to prove that a theory is free from contradiction. Furthermore, we should remark that, according to Russell, existence-theorems for cardinal arithmetic has to be provided ; these theorems are supposed to be obtained from set theory,[36] and so, contrary to what is suggested in the passage just quoted, arithmetic cannot be the only means to obtain existence-theorems. More generally, the whole issue is, in 1903, surrounded by the threat of the set theoretical paradoxes and the lack of a definite type theory, so that it is difficult to extract from the brief passage Russell dedicated to this question (§474) any precise characterisation of what is a proof of an existence-theorem.

Even if Russell's reasoning differs greatly from the model theoretical ones, we could use in both cases the same distinction: to say that a theory has a model (or that a certain class is not null), is not to say that the model (or the element picked out) is the true subject-matter of the theory. To single out one element of a certain class is not the same as to reduce the said class to this element. If, for example, cardinal numbers can be ordered, the very concept of order, and then analysis, has nothing essential to do with arithmetic. And the same is true of geometrical spaces. Therefore, the fact that arithmetic is said to play a special role in the proof of existence-theorems does not in any way contradict the picture of the *Principles* I have drawn.[37]

[34] [Russell, 1903] p. 497; see as well the letter of Whitehead quoted above.

[35] In [Russell, 1903], the existence-theorems are put aside from the main development, and confined to the last paragraph (§474) of the book. What is more, this passage was most probably written in May 1902, just before Russell sent the book off to the Cambridge University Press ; see on this point [Byrd, 2000] and [Grattan-Guinness, 1996].

[36] See for example [Russell, 1903] p. 497: 'The existence of zero is derived from the fact that the null-class is a member of it ; the existence of 1 from the fact that zero is a unit-class.'

[37] It is sometimes said that arithmetic enjoys a special status in [Russell, 1903] because it is the only theory that would give us access to logical objects. I must confess I do not understand this argument. Progression is,

5 Theory of magnitude and relative product (I): On the Logic of Relations

The preceding arguments had nothing to do with the concept of magnitude. They only aimed to show that the arithmetical genetic view, which imputes to Russell a conception according to which all mathematics can be reduced to arithmetic, misses the fact that Russell's theory of relation gives to the traditional boundaries between the mathematical branches a kind of logical justification. But a more thorough discussion of the Russellian theory of magnitude is needed for at least two reasons. First, in the 'tree schema' of section 4 above, I did not associate the theory of magnitude with any relational type. This constitutes a lack in the interpretation brought forward, that we have now to fill. Second, as the letter of Whitehead quoted in introduction shows, one of the main targets of the arithmetical genetic view was the Greek notion of magnitude, and the separation it induces between numbers and continuous quantities.[38] The sole fact of developing a theory of magnitude seems then to express a disagreement with the genetic standpoint. In order to back up our reading, we could thus not avoid, for these two reasons, a direct discussion of the Russellian theory of quantity.

I will first speak about the English version ([Russell, 1900]) of *Sur la Logique des Relations*,[39] not directly about part III of the *Principles*. As far as I can see, the doctrine exposed in both works are the same. But owing to the lack of symbolic notation, the discussion of quantity in [Russell, 1903] is made very difficult to follow, and the features which I would like to point out are better displayed in [Russell, 1900].

Briefly said, the project of Russell is to insert Burali-Forti's axiomatic (see section 2) of magnitude into his relational framework.[40] As Russell changed the symbolism and the definition of magnitude, Burali-Forti's theory, in its new guise, is hardly recognizable. Here is the definition of a kind of distance Δ (the equivalent of Burali-Forti's 'grandeur homogène') given by Russell:[41]

$$\Delta = FG \cap L \ni \{x, y \epsilon \lambda. \supset_{x,y} .\exists L \cap R \ni (xRy) : Q = R_L. \\ R_1, R_2, R_3 \epsilon L \ . \ R_1 Q R_2 \ . \supset_{R_1,R_2,R_3} . \ R_1 R_2 = R_2 R_1 \ . \ R_1 R_3 Q R_2 R_3\}$$ [42]

But Russell goes on:

> This is a definition of a kind of distance, *i.e.* of a class of distances which are quantitatively comparable. A kind of distance is a series in which there is a term between any two, and it is also a group. If any two terms belong to the field of this

for Russell, a logical object (that is a logically defined class of like series) as the number 1 is. And as it is needed to prove that the class of progression is not null, so it is needed to prove that the class 1 is is not empty.

[38] Let us quote this passage from the preface of [Dedekind, 1888]: 'All the more beautiful it appears to me that without any notion of measurable quantities and simply by a finite system of simple thought-steps man can advance to the creation of the pure continuous number-domain; and only by this means in my view is it possible for him to render the notion of continuous space clear and definite.'

[39] The passages I am interested in have been removed from the French published version. That is the reason why I consider here only the English original unpublished manuscript.

[40] A copy of Burali-Forti's article, annoted by Russell, can be found in the Russell Archives (McMaster University), and many details in the construction evoke Burali-Forti's approach. However there is one big difference between the two theories. The Italian mathematician starts with only one indefinable, the additive operation – order is derived (see section 2). For Russell, on the other hand, order is a primitive term, as addition is in the case of extensive magnitude. If the difference does not greatly change the shape of the formal structure, it deeply affects the general meaning of the notion of magnitude. Indeed, for Russell, magnitude in general is primarily defined by a 'capacity for the relation of *greater* and *less*', not by a capacity for divisibility (see [Russell, 1903] p. 159).

[41] F desgnates a dense series, G a group. L is a distance or a magnitude of a specific kind, *i.e.* as Burali-Forti's calls it, an homogenous magnitude. Q is the order relation of the magnitude ($Q = R_L$), and λ is the field of the magnitude (*i.e.* the field of the group and of the series).

[42] [Russell, 1900], p. 609.

group, there is a relation of the group which holds between them. If Q be the relation in virtue of which the relations of the group form a series, and if R_1, R_2 be relations in the group such that R_1QR_2, then $R_1R_2 = R_2R_1$, and the relation Q still holds when both sides are multiplied by any other relation of the group.

A distance (or magnitude) is here defined, as in all theories of magnitude at the time, and especially as in Burali-Forti's (see section 2), as a series which is a (semi-)group — as an ordered (semi-)group. And thus, the true import of the Russellian approach lies elsewhere, in the way Russell defines the group-structure. In another unpublished passage of the same manuscript ([Russell, 1900] p. 594), the notion of group is defined as a set K of bijective relations having the same field such that, firstly, if P belongs to K, the converse $\breve{P}$ belongs to K, and such that, secondly, if P and R belong to K, the relative product PR belongs to K.[43] In other words, Russell defines a group as a transformation group. Now, there is a well-known theorem of Cayley, which says that there is an isomorphism between the set of abstract groups and the set of transformation groups. Russell uses this result to rewrite Burali-Forti's more natural presentation. Technically, nothing prevents such a rewording. But what is the use of doing this?

Cayley's representation theorem allows Russell to present group theory as a development of the theory of relations. From Russell's standpoint, what could cause a problem in the group structure was, in effect, the group operation: how to explain the addition in Burali-Forti's axiomatic theory? What is a group operation? A three-terms relation, a combination of a relation with identity, or a new kind of term?[44] Russell's answer in 1900 is to say that a group operation is a relative product (a relation of relations) on a special set of relations.

We could go even further here. Just as analysis is defined as the theory of order, and geometry as the theory of incidence relations, the theory of magnitude seems to correspond, in Russell's thought, to a special part of the logic of relations: the doctrine of the relative multiplication. In part III of the *Principles*, owing to the fact that Russell emphasizes the order relation (see [Russell, 1903] pp. 158-159), the doctrine of relative product does not show up as in the English version of *Sur la Logique des Relations* — but its presence reveals itself in chapter XXI on measurement and extensive magnitude (see [Russell, 1903] pp. 176-182), and in part IV, chapter XXXI on distance (see [Russell, 1903] pp. 252-255). In these two passages, Russell follows closely the path taken in [Russell, 1900]. The theory of magnitude, which is a kind of algebraic doctrine, can therefore find a place in Russell's book and in Russell's relational logic. I could thus complete the 'tree schema', left unfinished, of the previous section:

Arithmetic II	Magnitude III	Analysis IV-V	Geometry V	Mechanic VI
Equivalence relation	Relative product	Order relation	Incidence relation	Three-terms relation

Part III of the *Principles* can be, after all, associated with a logical type — the relational product singles out the theory of magnitude as a special mathematical branch among the others. The doctrine of relative product gives to Russell the means to insert the doctrine of magnitude and the algebraic concept of operation in the logicist framework.

If a connection is made between the additive structure of the 'extensive' magnitude and the relative multiplication, the function assigned to the relational theory of magnitude in the *Principles* is difficult to grasp. Russell goes so far as to say that 'the whole of [part III] —

[43] The definition of the group is: $G = Cls'1 \rightarrow 1 \cap K \ni \{P\epsilon K. \supset_P .\breve{P}\epsilon K : P, R\epsilon K. \supset_{P,R} .PR\epsilon K.\pi = \rho\}$ Df.

[44] For a discussion of this problem, see [Sackur, 2005] pp. 143-209.

and it is important to realize this — is a concession to tradition' ([Russell, 1903] p. 158). It seems that the philosopher seeks here to deny any intervention of the notion of quantity in the definition of the reals. Russell then does not follow Burali-Forti in deriving natural, rational and real numbers from the concept of magnitude. If the doctrine of magnitude can be developed in the logicist framework as a theory of relative product, it is, in 1903, disregarded by Russell: real analysis can be developed directly from the theory of order relations. It is however to be remarked that the notion of magnitude seems to play a quite important, if complicated, role in relation to metrical geometry in the *Principles*. But I will leave this topic aside here, preferring to focus on the account of the magnitude given in the third volume of the *Principia*. Why turn toward [Russell and Whitehead, 1913]?

Even if the theory introduced in part VI of the *Principia* (completely devoted to this issue), is considerably more general and more abstract than the doctrine exposed in [Russell, 1900], its main features (especially the connection between addition of quantities and relative product) remain unchanged. There is thus a noteworthy continuity in the way Russell conceived the idea of magnitude, and this constitutes a first reason to have a look at the subsequent developments. But moreover, in [Russell and Whitehead, 1913], the role of magnitude changes: quantity becomes involved, even if in a very subtle way,[45] in the very definition of the rational and real numbers. The opposition to the genetic standpoint becomes then more apparent than before, and this is the second reason I have for looking into [Russell and Whitehead, 1913] part VI.

But to jump, as we are going to do, from 1903 to 1913 and from a Russellian book to a work mainly written by Whitehead[46] is of course a very perillous move: what to do with the intervening years and of the tremendous changes they have brought in Russell's thought? What to do with the great epistemological and philosophical differences between Whitehead's and Russell's philosophies?

I do not pretend to offer a panorama of the evolution of Russell from 1903 to 1913,[47] nor do I pretend to give an account of the many differences between Whitehead's and Russell's standpoints.[48] But, as we have said, the fact is that there is enough continuity between 1900-1903 and 1913 theory of quantity to make a local comparison possible. Furthermore, to my knowledge, no close examination of part VI of the *Principia* has yet ever been offered.[49] Even if an article wholly devoted to the subject could began to fill the gap, describing the main lines of the approach exposed in part VI could be, if only for this reason, usefull. I will thus, in the last section, describe the main lines of the 1913 theory of quantity, and the new function assigned to it.

6 Theory of magnitude and relative product (II): Principia, part VI.

Whitehead and Russell begin the first section of part VI by defining rational (and real) numbers as relations between two relations. Two relations R and S have the ratio μ/ν, 'if, starting from some [term] x, ν repetitions of R take us to the same point y as we reach by μ

[45] As it will soon become clear, whole numbers are still not defined as ratios of magnitudes, as was the case for Burali-Forti.

[46] Whitehead was supposed to write the volume IV of the *Principia*, and part VI was, as the last part of volume III, an introduction to volume IV.

[47] There are of course plenty of books on this issue: [Hylton, 1990], [Landini, 1998], [Grattan-Guinness, 2000].

[48] The studies devoted to the topic are much less numerous ; let us quote [Grattan-Guinness, 2002].

[49] As notable exceptions, we can mention a few paragraphs in Quine's 'Whitehead and the Rise of Modern Logic' (published in [Quine, 1995]) and in [Quine, 1962], and a few pages in [Grattan-Guinness, 2000].

repetitions of S, *i.e.* if $xR^{\nu}y.xS^{\mu}y$' (see [Russell and Whitehead, 1913] p. 260). The case of the real numbers, even if not fundamentally different, is more difficult and I will leave it out here.[50] The logicists define then the usual operations, addition and multiplication, on ratios.[51] If the axiom of infinity is assumed, both the order relation and the operations have the usual properties: the rational numbers thus defined form an ordered rational field — and the reals $(< \mathbb{R}, <, +, \times >)$, as usual, an Archimedian complete field. Let us remark that this approach assumes the natural numbers introduced in part II of the *Principia*, since, in their definition of the ratio, the two logicists allow themselves to speak about the 'ν repetitions of R'.

Russell and Whitehead's reasoning is at first sight very striking. The whole numbers having been defined, why not introduce the ratios as an equivalence class on the set of couples of whole numbers, as was usual at the time? Why adopt this very tortuous and contrived approach? In the case of Burali-Forti, things were different: the Italian mathematician wanted to show that arithmetic could be constructed on the basis of magnitude's theory, and was thus compelled to define numbers as a ratio of magnitudes. But for the logicists, such a project was out of date: natural numbers are given with the set apparatus, and thus, nothing, for Russell and Whitehead, seems to prevent the use of the standard definition of the rationals.

To grasp the meaning of this reasoning, we have to look at the last two sections of part VI. In section B, Russell and Whitehead resume and deepen considerably the sketch exposed in [Russell, 1900]. Instead of starting with a group (that is with a set of bijective relations on the same field such that, firstly, the product of any of them belongs to the set, and secondly, the converse of any of them belongs to the set), Russell and Whitehead start with a much weaker structure, the so-called vector family. In modern terms, a vector family is any commutative subset of the semi-group of the injective mapping on a common domain.[52] Notice that a vector family is not necessarily closed by the relative product, that the identity relation does not necessarily belong to a vector family, and that if a relation belongs to a vector family, it is not always the case that its converse belongs to it.

The essential point, for us, is that, instead of simply translating Burali-Forti's theory of magnitude in the relational framework (as in [Russell, 1900]), the logicists try to generalize it. It seems as if they wanted to find the most general sets of relations (more precisely: the most general vector families) which could be regarded as measurable 'magnitudes', in a very specific sense of the term.

At the beginning of section C, Russell and Whitehead specify this sense, and list the necessary and sufficient three conditions for a vector family to be a measurable magnitude:

1) 'No two members of a family must have two different ratios' and 'all ratios [except 0 and

[50] Put briefly, Whitehead and Russell introduce the reals, firstly, in the usual way, as segments of ratios; secondly, in order to enhance the link with measurement, as relations of relations that have the same type as the ratios. The trick is the following: 'instead of taking the series of the segments as constituting the real numbers, it is possible to take the series of their relational sums'. A real number is thus no more conceived of as a set of relations of relations, that is as a set of sets of couples of relations, but directly as a relation of relations, that is as the set of couples of relations which is the relational sum of the previous set. For more on this, see [Quine, 1962] pp. 127-130.

[51] The ratio $a/b + c/d$ is the set of relations R and S such that $R((a+c)/b)S$; the ratio $a/b \times c/d$ is the set of relations P and Q such that $P(ac/bd)Q$.

[52] The semi-group Φ of the injective mapping on a common domain κ is:

$$\Phi = \{f : \kappa \to \kappa \mid f \text{is an injective mapping}\}$$

We note that we have $f,\ g \in \Phi \Rightarrow f.g \in \Phi$, and $f,\ g,\ h \in \Phi \Rightarrow (f.g).h = f.(g.h)$. Of course, because one deals here with injective (not necessarily bijective) mappings, one has to generalize the definition of the composition and of the inverse of a function.

∞] must be one-one relations when limited to a single family';

2) 'The relative product of two applied ratios ought to be equal to the arithmetical product of the corresponding pure ratios with its field limited [to a single family]', that is to say 'two-thirds of half a pound of cheese ought to be $(2/3 \times 1/2)$ of a pound of cheese';

3) If X, Y are ratios, and T, R and S are members of the family κ such that RXT and SYT, we ought to have $RS(X+Y)T$, that is 'two-thirds of a pound of cheese together with half a pound of cheese ought to be $(2/3 + 1/2)$ of a pound of cheese, and similarly in any other instance'.[53]

When these conditions are all satisfied, vector families become measurable magnitudes, and ratios (restricted to the given vector family) become 'applied ratios'.[54] The underlying idea is that, in such a situation (and in such a situation only), arithmetic computations are mirroring the properties of the relative product between relations. In this restricted context, the arithmetic laws are not only logically valid — they express the structural relations, that is the formal properties of the relative product, between the elements (the relations) of the vector families under consideration.

We now have enough cards to answer our initial question: why Whitehead and Russell introduce, in 1913, the rationals as a relation between relations instead of defining them as an equivalence class on the set of couples of natural numbers? The logicists seek to explain why numbers can be used to measure magnitudes. As Whitehead says in the letter quoted at the beginning, the arithmetic definition of the real 'leaves the whole theory of applied mathematics (measurement etc) unproved', and the logicists want to fill this gap.[55] To embark on such a task is however a very dangerous undertaking for the logicists, because the most natural answer to this question is Burali-Forti's: numbers can measure quantities, because numbers are ratio of magnitudes, *i.e.* are derived from magnitudes. But now, in order to warrant the existence of all the real numbers, one has to postulate the existence of a sufficiently rich set of magnitudes.[56] But, as we all know, for Russell and Whitehead, arithmetical truths, and computation with rational and irrational numbers as well, do not depend on any empirical fact (we ignore here the issue raised by the axiom of infinity). That there are enough magnitudes is nevertheless an empirical fact, which seems thus completely foreign to mathematics. Therefore, Burali-Forti's answer cannot be sustained. But then, in avoiding the Scylla of empiricism, one falls again on the Charybde of the abstractness: the link between numbers and measures, in the arithmetical genetic approach is lost. How to explain the use of numbers in measurement without endorsing a kind of mathematical empiricism?

The neat logicist answer to this double peril is roughly the following: magnitudes can be

[53] see [Russell and Whitehead, 1913], p. 407.

[54] Russell and Whitehead prove (see [Russell, 1903] pp. 412-422) that every family connected, open and submultipliable is measurable. Translated in a more modern terminology, $\Im$ being a vector family defined on a basic set κ:

$\Im$ is connected iff $\exists x \epsilon \kappa, \forall y \epsilon \kappa, \exists f \epsilon \Im, fx = y$;

$\Im$ is submultipliable iff $\forall f \epsilon \Im, \forall n \epsilon \mathbb{N}, \exists g \epsilon \Im, f = g^n$;

$\Im$ is open iff $\forall x \epsilon \kappa, \forall n \epsilon \mathbb{N}, \forall g, h \epsilon \Im, g^{-1}h \neq Id \Rightarrow (g^{-1}h)^n(x) \neq x$.

[55] As Lebesgue puts it very nicely in [Lebesgue, 1975] p. 190: 'Les irrationnels se définissent à partir du continu géométrique grâce à la mesure des segments ; on peut aussi les définir à partir du continu arithmétique au moyen de la considération des coupures, par exemple. Chacun de ces modes de définition a ses avantages. La première est en quelque sorte concrète ; la seconde est abstraite. C'est pour cela que la première se raccorde bien, et elle seule, aux considérations de la géométrie élémentaire, dont elle découle presque inévitablement. La seconde [...] rappelle ces ballets sans rapports avec l'action, que l'on trouve dans tant d'opéras.'

[56] Frege develops the same kind of approach, which gives rise to the same kind of objections; see [Hale, 2000] and [Batitsky, 2002].

regarded as special sets of relations (as special vector families), and numbers can be defined as relations of relations. Defining rational or real numbers as relations of relations allows us to base the whole arithmetic on safe ground (once the axiom of infinity is admitted): all arithmetical laws can be derived from the logical primitive propositions. But, at the same time, if some constraints are put on the underlying set of relations, it can be shown that the arithmetical laws reflect the relations between the elements of the underlying set: to each couple of relations corresponds one and only one ratio; the relative product of two applied ratios is the arithmetical product of the corresponding two ratios; and the ratio of the relative product of two relations to a third one is the sum of the arithmetical ratios that each of the two relations have to the third one.

So we have here a very subtle two-levelled reasoning. At the first level, the relational definition guarantees the validity of the usual arithmetical laws. But at the second stage, the very same definition allows us to explain why, in certain circumstances, these laws can be applied to represent the relations between some elements in a given structure. To define rational numbers as relations of relations appears cumbersome only when one reduces, as the 'arithmizers' do, the foundational task to the justification of the usual arithmetical laws. But when, like Whitehead, one does not want to leave 'the whole theory of applied mathematics (measurement etc) unproved', what appeared, at first sight, as a superfluous detour, reveals itself as a neat device to logically explain the use of numbers in measurement. The true force of the relational theory of ratios lies in the fact that it explains the connection between numbers and magnitudes without endorsing any kind of empiricism. One again, this amazing feat is grounded on the notions of relation and of relative product.

We have here favoured the comparison between the logicist approach and Burali-Forti's one. But it would be interesting to compare the *Principia*, part VI, with the modern theory of measurement, developed by Suppes and others.[57] One point seems especially worth noticing. In the modern approach, measurement is defined as an assignation of numbers, so that the question: how it is that numbers are specially fitted to measurements? — cannot there even be asked. The link between numbers and measurement, being postulated, is here as well left unexplained. In other words, measurement theory does not answer the question: why is measurement, *i.e.* assignation of numbers, so important? In this regard, the logicist theory seems to be more ambitious: it is because rational and real numbers are relations of relations that they can represent so easily the relations between magnitudes (which are, for Russell and Whitehead, relations).[58]

So, to sum up: from the very beginning, Russell has inserted the theory of magnitude in his logical relational framework, and this trend has not disappeared in the *Principia*. Quite the reverse, in fact: Whitehead has strengthened the meaning of a doctrine that he explicitly opposes, in his letter dated 1909, to the arithmeticisation program. So it seems as if, in the logicism of Russell and Whitehead, the theory of magnitude gained more and more philosophical weight as the years went by. Of course, as we have said, this conclusion does not take into account the differences between Whitehead and Russell's epistemological and philosophical conceptions. And it is likeley that Whitehead's own idiosyncratic opinions have paved the way for a strengthening of the role played by the Greek concept of magnitude in the logicist account of the rational and real numbers. But it remains that the theory exposed in 1913 does constitute an extension of the doctrine presented in 1900-1903 — that Russell was informed of the changes (as the letter

[57]See for example [Krantz et al., 1971].

[58]For Russell and Whitehead, the problem raised by the measurement (why is measurement so important?) is neither metaphysical, nor anthropological. A genuine logical answer to this question can be articulated.

quoted shows) and that, since the published preface of part VI repeats roughly the same ideas, he did not disapprove of them.

7 Conclusion

After having distinguished three different meanings of the arithmeticisation program, I have claimed here that, contrary to a widespread view, the 1903 version of Russell's logicism should not be seen as an extension of the genetic arithmetical program, according to which all mathematics is reducible to arithmetic.

My first point (section 2) consisted in underlining a renewed interest, at the end of the XIXth century, in the old Eudoxian theory of magnitude. Russell and Whitehead were not alone in criticizing the genetic conception; quite the contrary, the same critical stance was shared by almost all the mathematicians who were involved in the development of the various geometrical calculi.

My second point (sections 3 and 4) aimed at showing that Russell's logical reductionism goes hand in hand with a conservative conception of mathematics and of its overall structure. Far from seeking to reduce mathematics to arithmetic, Russell, in the *Principles*, used the theory of relation, especially the key-concept of relational type, to restore the traditional separation between the mathematical disciplines.

In the last two sections (5 and 6), I showed how Russell and Whitehead inserted the theory of magnitude in their relational framework. The mere persistence of a doctrine of quantity is already a reason to cast doubt on the standard interpretation. But there is more. In 1913, the two logicists criticized the arithmeticisation program for leaving unexplained the relation between numbers and measurements — the raison d'être of the theory of magnitude was precisely to fill this gap, without falling into the empiricist trap.

In other words, Russell was not alone in the mathematical world, at this time, in opposing the arithmeticisation movement; moreover, he used logic, not only as a means to derive all the mathematical content, but also as a a means to justify the traditional distinctions inside this content; at last, he and Whitehead addressed in 1913 an issue (the connection between numbers and measurement) that is not even raised by the advocates of the arithmeticisation (in its genetic version). All these points lead me to conclude that the standard view, according to which Russell extended in 1903 the genetic arithmetical program, had to be qualified.

But, all this being granted, how are we now supposed to deal with the numerous texts where Russell explicitly introduces his logical reductionism as a deepening of the genetic view?[59] If Russell did not share the belief that all mathematics is reducible to arithmetic, why did he sometimes behave as if he did? This is a difficult question. It has first to be noted that, in the *Principles* themselves, no such oversimplified presentation of his program can be found.[60] We could thus be tempted to answer that we are facing here a not too unusual situation with Russell: it would not be the only time where the philosopher refused to do full justice to his early views. But in this case, there may be a more precise explanation.

[59]For an instance of such a text, see my introduction.

[60]See for example [Russell, 1903] p. 157-158, where Russell, in order to explain the reason why quantity is no longer an indefinable object, refers to two opposite mathematical developments: the 'purist' Weierstrassian reform of course, but also another trend, in which Russell puts all the mathematical theories that did not deal with numbers ('Logical Calculus, Projective Geometry, and – in its essence – the Theory of Groups'). See as well *Ibid.*, p. 381. It is to be noticed that the same idea is repeated in the last chapter of [Russell, 1921].

Russell used to divide his work into two parts: 'first, to show that all mathematics follows from symbolic logic, and secondly to discover [...] what are the principles of symbolic logic itself' (see [Russell, 1903] p. 9). As we all know, immediately after the publication of the *Principles*, Russell embarked on a purely logical research (aiming at resolving the paradoxes). From this time onward, the second issue (how to free logic itself from the contradictions?) became the central one, to the detriment of the first one (how to logically derive some mathematical theory or other?). In other words, immediately after the *Principles*, the link between logic and mathematics began to be foreshadowed by purely logical topics, and remained subsequently of secondary importance in Russell's mind. This, I think, is the source of the late Russellian oversimplification. The important point was, for Russell, to precisely define what the nature of logic was, not any more to delineate the form that the connection between mathematics and logic should take.[61] Given this shift in the order of priority, we could understand that to oversimplify the complicated construction of [Russell, 1903] and to come back to the arithmeticisation program (in its genetic version), did not constitute, for Russell at the time, a betrayal of his initial inspiration.

To conclude, I would like to point out the main target of my interpretation: Wittgenstein's reading of Russell's logicism. Wittgenstein described the Russellian project as a systematic endeavour to standardize and artificially unify the mathematics. 'Mathematics', he claims (see [Wittgenstein, 1956] p. 84), 'is a MOTLEY of techniques of proof.— And upon this is based its manifold applicability and its importance'. Wittgenstein viewed Russell's program as a denial of the essential variety of mathematics. Thus:

> If someone tries to shew that mathematics is not logic, what he is trying to shew? He is surely trying to say something like : — If tables, chairs, cupboards, etc. are swathed in enough paper, certainly they will look spherical in the end.
>
> He is not trying to shew that it is impossible that, for every mathematical proof, a Russellian proof can be constructed which (somehow) 'corresponds' to it, but rather that the acceptance of such a correspondance does not lean on logic.[62]

In my opinion, it would be difficult to be wider of the mark. Indeed, if I am right, Russell did not aim at reducing the differences between the numerous mathematical techniques. On the contrary, one of his main goals was to use logic for uncovering the deep logical differences between the various mathematical disciplines. The alternative is thus not between a kind of pragmatism, careful to the specificities of the mathematical practices, on one side, and a rigid logicism, blind to the richness of the motley mathematical sciences, on the other. There is absolutely nothing in logicism which would prevent accounting for the differences within mathematics, and, what is more: Russell, in the *Principles*, actually did use the notion of relational type to delineate the logical core of a specific method of proof. Thus, Russell, like Wittgenstein, attempted to 'teach us differences'[63] — and, to tell the truth, the way he did it seems to me more promising than

[61] Russell, suprisingly, did not like the long and detailled reviews Couturat devoted to the *Principles* (Couturat gathered them in a book [Couturat, 1905]). Couturat's analysis was precisely centred on the mathematical parts of [Russell, 1903], and this was the feature that Russell disliked most; for more on this topic, see [Schmid, 2001]. Thus, as early as 1905, the issue related to the connection between mathematics and logic, that is the issue dealt with in part II-VII of the *Principles*, had already lost its importance. In fact, my guess is that the quite subtle 'tree schema' dated from 1900, that is from the 'intellectual honey-moon' (see [Russell, 1959]) period during which Russell wrote the mathematical parts of his work.

[62] [Wittgenstein, 1956] p. 89.

[63] Wittgenstein used the Shakespearian line 'I'll teach you differences' as an epigraph of his *Philosophical Investigation*.

Wittgenstein's way.

Acknowledgements

This paper was first presented at the seminar of the 'Maison des Sciences de l'Homme' in Clermont, and at a workshop on Russell organised by the REHSEIS in Paris. I am grateful to these two institutions. I would like to thank Nicholas Griffin for having encouraged me to write this article, and to thank Brice Halimi, Steve Graham, Gregory Landini, Jérôme Sackur, François Schmitz, Elisabeth Schwartz, Ivahn Smadja for helpful discussions. Many thanks also to the two anonymous referee for their useful suggestions.

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