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Sébastien GANDON
IUF / PHIER, Clermont Université

1- In a letter to the French historian of mathematics P. Dugac, dated 12/05/1984, the great mathematician J. Dieudonné wrote:

> The controversy between Poincaré and Russell is very enlightening; it reveals quite obviously how completely *invalid* the reasonings of the alleged "mathematician" Russell are about everything connected to mathematics; he could have been wholly self-taught on the subject, since what he says shows he apparently didn't know anything about the works on the foundation of geometry, from Cayley to Pasch, Klein, and the Italian school [...]. I think that Poincaré was too kind to find time to discuss this verbiage, and to explain the Erlangen Programme again and again (without quoting it, probably so as not to scare the readers of the *Revue de Métaphysique et de Morale*); this was, however, a waste of effort, since Russell did not understand anything about it, and continued to use the words "true" and "false" wildly in connection with mathematics and its relation to reality (words that Poincaré avoided with great care). [...] The moral is: philosophers should know something about mathematics before claiming to speak about it! (Dugac 2003, 221-222)

Even if it is usually formulated in a more diplomatic way, the idea that Russell's work is completely cut off from the mathematical research of his time is quite widespread among mathematicians and historians of mathematics. Russell's reductive program is often seen as a very abstract project, which has nothing interesting to say about mathematical practices. The notorious controversy between Poincaré and Russell would thus epitomize a more general opposition between an approach which takes into account the real mathematics developed by the working mathematicians, and a very abstract philosophical conception which creates a fictional picture of mathematics, and speaks about it as if it was the reality.
Surprisingly, this opinion of Russell's work could be confirmed by the interpretation of some Russellian scholars, who have stressed the fact that Russell's attempts to incorporate in his logicism some features of contemporary mathematics led him to weaken his general position. This is in particular the thesis of A. Coffa, who, in his influential « Kant and Russell » (1981), maintained that the emergence of non-Euclidean geometries posed a challenge for logicism. As Russell himself put it in his introduction to the second preface of *The Principles*, dated 1937,[1] it is "clear that Euclidean and non Euclidean systems alike must be included in pure mathematics, and must not be regarded as mutually inconsistent" (Russell 1937, x). But how can that be done if all these mutually incompatible systems are to be derived from the same logical premises?
To accommodate this case, Coffa explained, Russell proposed to change the meaning of logicism: "we must (…) only assert that the axioms imply the propositions, not that the (geometrical) axioms are true and therefore the propositions are true" (*Ibid.*, x). That is: p being a Euclidean theorem, and Q being the conjunction of the Euclidean axioms, Russell, in

[1] See as well Russell 1903, 373.

1903, would neither claim that p is a logical truth, nor that Q is a logical truth, but only that $Q \Rightarrow p$ is a logical truth. In other words, in *The Principles*, metrical geometries would be only conditionally reduced to logic – Russell would be a "if-thenist".[2]

Thus, according to this interpretation, the emergence of non-Euclidean geometries led Russell to weaken his logicism, and, for Coffa, the move was not a happy one. Indeed, Russell's conditional logicism falls prey to a trivialization argument: « if the claim that mathematics is logic is basically no more than the claim that each conditional that has an appropriate set of mathematical axioms as antecedent and one of its theorems as consequent is provable in logic, then we can establish the reducibility to logic not only of mathematics but of a large number of obviously non-logical disciplines as well » (Coffa 1981, 252). An axiomatic geography would then become a logical science![3]

For Coffa then, the wish to take into account a mathematical fact (the emergence of non Euclidean geometries, to be specific) induced Russell to blur the meaning of his original logicism. In this sense, Coffa's analysis meets Dieudonné's diagnosis: as soon as confronted with real mathematics, Russell was compelled to step back.

The aim of my talk is to attack this interpretation. I primarily intend to show that Coffa seriously misconstrues Russell's views of metrical geometry and logicism. There are four elements to Coffa's analysis. The first two concern metrical geometry. First, Coffa claims that metrical geometry is, for Russell, a field which exhibits various colegitimate alternatives; second, Coffa presupposes that Russell regarded metrical geometry as a logical science. The other two elements concern logicism. The third is the thesis that Russell's method can be applied to any axiomatic system, and the fourth that this trivialization deprives logicism from "its significance and its relevance to any traditional philosophical issues".[4] I would like to show here that only the third assumption is valid and that the three other ones are false. In particular, I will contend that the third point (the fact that logicism is the prey of a trivialization argument) does not imply the fourth point (the collapse of logicism).

By taking metrical geometry as a case-study and Coffa's interpretation as a target, I also have a second, more important, objective: I want to challenge Dieudonné's diagnosis. I would like to contend that, far from being severed from the concrete development of the mathematical sciences, the true meaning of Russell's logicism stands out only against a refined description of the contemporary mathematical practices. Of course, I would not be able to prove this here. But I hope that the evidence I will gather will convince the reader that this hypothesis is not completely empty.

2- In the secondary literature, it is usual to distinguish between a strong version and a weak version of logicism[5]. According to the former, to reduce mathematics to logic means that:

(1) all primitive mathematical terms are defined using only logical notions,
(2) all mathematical primitive propositions are deduced from logical axioms,
(3) all theorems of mathematics are deduced from logical axioms.

According to the weak version ("if-thenism"), logicism can be defined by the conjunction of (1), (3) and (2'), which is a weakened version of (2):

[2] It is Putnam who coined the term "If-thenism" in his 1967.
[3] This idea is foreshadowed by Quine in his 1936, 327.
[4] Coffa 1981, 252.
[5] See Putnam 1967, Musgrave 1977, Coffa 1981.

(2’) *either* an apparently primitive proposition of mathematics is deduced from logical axioms *or* it is not to be regarded as a primitive proposition at all but only as the antecedent of various conditional statements.[6]

Most of the scholars seem to consider that, in Russell 1903, the strong version held for arithmetic, while the weak version was applied to geometry. Russell, however, nowhere acknowledged such a distinction between two sorts of logicism. The standard move is then to attribute this lack of clarity to Russell’s confusions about implication. Musgrave, for example, claimed that Russell’s confusion concerning the two logicist programs was due to his “failure to distinguish a rule of inference, a conditional statement of the form ‘If *A* then *B*’, and a universal statement of the form ‘$(x)(Fx \supset Gx)$’’ (Musgrave 1977, 112). I am not convinced by Musgrave’s argument. My opinion is, indeed, that there is a genuine unity in Russell’s project – the issue being much too vast, I will not, however, argue for this reading here.[7] I would just like to point out, though, that whatever Russell’s conception of his own general project was, nothing in the logicist method itself forbids us from seeing axiomatic geography as a pure mathematical science.

In the last paragraph (§474) of *The Principles*, Russell offered a useful summary of his work and his method:[8]

> [In part II to VII of the book], it was shown that existing pure mathematics [...] can be derived wholly from the indefinables and indemonstrables of Part I. In this process, two points are especially important: the definitions and the existence-theorems. A definition is always either the definition of a class, or the definition of the single member of a unit class [...]. A kind of grammar controls definitions, making it impossible *e.g.* to define Euclidean *Space*, but possible to define the class of Euclidean *spaces*. [...] The existence-theorems of mathematics – *i.e* the proofs that the various classes defined are not null – are almost all obtained from Arithmetic.

According to Russell, then, a successful logicization of a given piece of mathematics must fulfil two conditions: (a) all the mathematical concepts belonging to the theory considered must be defined in logical terms; (b) for each defined concept, an existence-theorem should be provided. No mention is here made of the conditions (2) and (2’) about the proof of the mathematical axioms, and no distinction is made between a strong and a weak form of logicism. Question: does this basic structure allow us to reject the trivialization challenge?

Let me first consider Hilbert’s axiomatic system of Euclidean geometry, as it is presented in the *Grundlagen*. Hilbert’s system contained some non-logical constants: the predicates “to be a point”, “to be a line”, “to be a plane”; the relations of incidence, of order, of distance and of parallelism. Owing to these occurrences of non-logical constants, Hilbert’s axiomatic system cannot be regarded (according to Russell’s §474) as a logical definition of the notion of Euclidean space. Now, let us replace each non-logical constant occurring in Hilbert’s axioms by a variable (of the appropriate type), and let us form the existential closure of all these open formulas. The axiomatic system whose postulates are the new sentences is distinct from the initial one, in that it contains no non-logical constants. At the same time, there is equivalence between the initial and the resultant axiomatic systems, in the sense that both theories have exactly the same models. This is not surprising: as Hilbert emphasized, the non-logical constants have no meaning in themselves and are implicitly defined by the axioms. The method of systematically replacing the non-logical constants by some quantified variables

[6] I summarise here the excellent presentation given in Musgrave 1977, 101, 112.

[7] For a defence of the unity of Russell’s project, see Byrd 1999 and Griffin 2002.

[8] The same presentation is resumed and extended in chapter 1 of Whitehead 1906.

(sometimes called “ramseyfication”) can be thus seen as a move which gives expression to the fact that non-logical constants do not name anything.[9]

Now, the “ramseyfied” version of Hilbert’s system satisfies the two conditions set out at the end of *The Principles*. First, in the “ramseyfied” version, the notion of Euclidean space is defined in a purely logical way (no non-logical constants occur in the axioms). Second, as Hilbert himself showed, the class “Euclidean space” is not empty: there are some arithmetical models of the axiomatic.

It seems, then, that, owing to this procedure of “ramseyfication”, any axiomatization (provided that the system had a model) can be regarded as a genuine logicization of the science that is axiomatized. Here, then, Coffa’s trivialization’s challenge resurfaces: if this is what logicism consists in, then geography (if axiomatized) becomes a part of logic, and no distinction between logical and empirical theories can ever be made by the Russellian logicist.

In order to avoid the disaster, two ways out could be considered. But a moment of attention shows that both of them are dead-ends:

1) One could first focus on the existence-theorems and defend the idea that the difference between a mathematical and an empirical theory would come from the fact that one could never prove that an empirical theory does have a (set-theoretical)[10] model. For instance, what would distinguish elementary arithmetic from a formalized geographical theory would be that only the first has a set-theoretical model. This, however, does not work. From the completeness theorem, it follows that all consistent first-order theories have a model. If my geographical theory is coherent (which is the least to be expected) and couched in a first-order language, then a set-theoretical model of it exists. With a second-order theory, the situation does not change much. In reality, it gets even worse, since the loss of completeness compels us to assimilate the notion of a coherent theory to the concept of a theory having a model.

2) Secondly, one could try to reject the idea that Russell considered implicit definitions as genuine definitions. This is the way that Russellian scholars usually attempt to answer Coffa’s challenge.[11] And at first sight, the sacrifice of implicit definition appears as a good gambit. Indeed, Russell objected to Peano’s axiomatic characterization of $\angle$ as a ω-sequence (124-128) – a good definition of the whole numbers, he said, should provide us with the means to tell which ω-sequence the integers were. According to this reading, the belief that Russell endorsed a conditional program would spring from the neglect of a distinction between good (explicit) and bad (implicit) definitions.

I am not convinced by this reply. Indeed, implicit definitions were considered by Russell as genuine definitions. Russell never said that Peano’s axiomatic system did not define anything; he only said that it did not define what Peano believed it defined (that is, he said that his

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[9] For more on “ramseyfication”, and its relation to Russell’s late structuralism, see Demopoulos & Friedman 1985. In fact, Russell did not use the idea of “ramseyfication” in 1903. He seemed to regard the conjunction of the axioms as a propositional function, whose variables were the non-logical constants. The theorems were also viewed as propositional functions, whose variables were the non-logical constants. Instead of considering the existential closure of the axioms, he considered the universal closure of the implication whose antecedent was the conjunction of the axioms and whose consequent was a given theorem (see chap. 1 Whitehead 1906). Yet, despite this difference, the essential point remained: the non-logical constants were banished from the formulation of an axiomatic theory.

[10] A very deep problem lies, here, in the difference between logic and set theory. Briefly said, Russell believed in 1903 that set-theory belonged to logic; in particular, he claimed that that he could prove, by logic alone, that there are infinite classes (see §339). This was no longer the case in the *Principia*. In 1910, Russell thought that a special axiom was needed to warrant the existence of classes of any finite cardinality. This has some very important consequences on the issue concerning existence-theorem. In 1910, Russell had realised that it was not possible to prove, by logic alone (that is, without referring to the axiom of infinity, whose logical status is uncertain) the existence of an ω-sequence. For more on this question, see Musgrave 1977 (105-109).

[11] They follow the suggestions that Russell himself made in 1919 (chap. 1-2). For an example of such an answer, see Griffin 2002.

axiomatic system defined the notion of a ω-sequence, and not the series of the whole numbers). For Russell then, implicit definitions were not logically imperfect, and acknowledging this fact blocks the standard answer to the trivialization challenge.

The logicist project as it is presented at the end of *The Principles* does not provide us with an answer to Coffa's challenge. I believe then that Coffa is right: from a formal point of view, one could extend the logicist program to any formalized theory (to an axiomatic geography, for instance).[12] I do not believe, however, that Russell's logicism is, for this reason, deprived from "its significance and its relevance to any traditional philosophical issues". Until now, we presumed that Russell's logicism was a general method, which could be applied to any kind of topic in the same way. This is precisely the perspective taken by Coffa, whose belief is that Russell's method (or methods) could be defined without taking into account the field to which it is applied.

I will now try to show, that, in some important respects, Russell's logicism was not a logical method. In his reductive venture, Russell always took into account some non-formal constraints, linked with the specificities of the subjects under investigation. It is precisely Russell's consideration for these non-formal elements which allows us to reject the consequences Coffa draws from his trivialization challenge. Now, in order to make this apparent, I need to focus on Russell's treatment of specific topics. Following Coffa, I have chosen to deal with Russell's theory of metrical geometry.

3- Contrary to what Coffa suggests (and also contrary to what Russell sometimes said), the potential incompatibility between the various metrical geometries is not a problem for Russell's theory as presented in *The Principles*. In 1903, Russell's view on geometry was very influenced by the pure projective tradition of v. Staudt, summarised by Klein in his two papers *Über die sogennante Nicht Euklidiche Geometrie* (1871, 1873). This explains two things:

1) the fact that metrical geometry occupied in 1903 a relatively marginal position;
2) the importance Russell attached to the projective derivation of distance.

Let me first say something about the first point. At the end of the XIXth Century, many geometers, in the wake of Helmholtz, linked geometry to the operation of measure. Such was not, however, Russell's view. The aim of v. Staudt, which Russell claimed to support, was indeed to warrant the independence of projective geometry from every metrical consideration. In order to do that, the mathematician rejected the old definition of the anharmonic ratio as a ratio of two metrical ratios, and attempted to introduce the coordinates in a purely synthetic way. Problems, however, affected his construction, and, according to Russell, only the Italian mathematician M. Pieri, in 1898, succeeded in achieving v. Staudt's program. In *The Principles*, Russell, following Pieri, granted a central place to the projective theory, and regarded space as a pure incidence structure.[13] The emergence of non-Euclidean geometries was then not regarded by Russell as the fundamental change brought about by the explosion of geometry in the XIXth Century – much more important for him was the development of a pure projective geometry.[14]

[12] As I said at the beginning, I have not dealt, in this section, with the general issue concerning the relations between full-fledged and conditional logicism within Russell's project. The important point for me is not to argue *pro* or *against* the fact that Russell endorsed "if-thenism" – it is to examine the consequences of the trivialization argument, that is, to determine whether this argument has the devastating consequences Coffa claims it has. Now, I showed that the way Russell summarized his project at the end of *The Principles* made room for the trivialization challenge, and this suffices to pursue the examination of the subject, independently of the question of knowing whether Russell endorsed one or two sorts of logicism in 1903.

[13] For more on this topic, see Gandon 2009.

[14] Russell divided the history of Non Euclidean geometry into three periods (see Russell 1897, 17): the axiomatic period, with the discovery of hyperbolic geometry by Lobatchevsly and Bolyai; Riemann's first unification of the subject by the means of analytical methods; Cayley's and Klein's second geometrical unification of the subject by projective method. For Russell,

This point leads directly to the issue concerning colegitimate alternative geometries. Thanks to the works of Cayley and Klein, the projective theory provided the mathematicians of the time with a unitary framework from which the three classical metrical geometries (elliptic, Euclidean, hyperbolic) could be derived. Let me take the example of the linear distance between two points in the two dimensional complex projective plane. Klein began by fixing a fundamental form, that is, a fundamental quadratic curve Ω. He defined then the distance between two points A and B as the logarithm of the anharmonic ratio between these two points and the two intersections of the line (AB) with Ω. Klein showed that if the conic Ω was real, the metric obtained was hyperbolic; if the Absolute was complex, the metric was elliptic; and lastly, if Ω was a degenerate conics which took the form of a couple of complex points (the so-called circular points at infinity), then the metric was Euclidean. I have extremely simplified Klein's approach, so as to focus on the essential point: at the time, the various standard non Euclidean metrics appeared as the consequence of a choice between various fundamental forms.

This move represented a complete turnaround from the situation which prevailed in the first half of the XIXth Century, and in the new perspective, the alleged incompatibility between the different kinds of metric was reduced to the differences between the properties of various kinds of projective conics. Coffa completely misses this point. For Russell, saying that there were several different metrics was exactly as innocuous as saying that there were distinct figures (various sorts of conics) in a projective space. Moreover, as projective geometry was regarded as a purely mathematical theory, metrical geometries could very easily be included within the logical framework.

This is not the sole mistake that Coffa makes, however. Indeed, Coffa claims that Russell considered metrical geometry as a logical science – and this is not the case. In *The Principles,* Russell said many times that metrical geometry was an empirical science. Let me quote the conclusion of chapter 48: "metrical geometry, as an independent subject, requires the new idea of the magnitude of divisibility of a series, which is indefinable, and does not belong, properly speaking, to pure mathematics" (428). Thus, contrary to what Coffa claims, Russell did not regard metrical geometry as a part of mathematics.[15]

At this stage, however, a new difficulty arises. I have just shown that, contrary to what Coffa suggests (Coffa's first mistake), Russell had, in 1903, the technical means to regard metrical geometry as a logical theory. At the same time, I have claimed that, contrary to what Coffa asserts (Coffa's second mistake), Russell considered metrical geometry as an empirical discipline. But, if Russell had the technical means to derive metrical geometry from logic, why has he decided not to do so? Coffa thought that the problem was to explain how Russell's logicism should be changed in order to accommodate the problematic case of metrical geometry. The new problem is exactly the opposite: it is to explain why Russell, although he could have easily derived the various metrics in a uniform logical way, nevertheless claimed that metrical geometry was an empirical science.

Before confronting this question, let me give first some contextual elements. Russell's theory of metrical geometry, as it is presented in Part VI of *The Principles,* is closely connected with a still more neglected part of the work, Part III, devoted to quantity. In order to understand Russell's position about metrical geometry, it is then important to briefly outline Russell's doctrine of quantity.

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then, the issue relative to the existence of colegitimate geometries (he alluded to in 1903, 373) belonged to the bygone days of Lobatchevsky and Bolyai.

[15] It is likely that this second error comes from the same source as the first one. As Coffa does not seem to distinguish between metrical and projective geometry, he could have been led to believe that, when Russell claimed that projective geometry was a mathematical theory, metrical geometry was to be included in his claim.

In Part III of *The Principles*, Russell broke with the traditional conception of quantity, according to which two quantities of the same kind can always be both compared and added to each other. For Russell, only ordinal comparability belonged to the concept of quantity – additivity was not a necessary requisite.[16] In other words, Russell abandoned the privilege usually put on the so called extensive quantity. But despite this, he, in 1903, distinguished between two important kinds of extensive magnitudes: the "magnitude of divisibility" and "distance".[17]

Russell's conception of these two sorts of quantity is, in its detail, very esoteric and complicated. Roughly speaking, things which are said to have a magnitude of divisibility are things that could be divided in parts.[18] For instance, as an area could be divided, it was regarded as a magnitude of divisibility; as a pleasure could not be so divided, pleasure was not defined as a magnitude of divisibility. The other kind of extensive magnitude, distance, was not divisible – Russell characterized distance as a certain set of relations, satisfying certain formal conditions, and the addition between distances was defined as a relative product between relations.[19] Thus, the main difference between divisibilities and distances was that the former were divisible, while the latter, being relations, were not.

Finally, the question: to be or not to be a magnitude of divisibility was regarded as an empirical question, the answer to which could not be logically anticipated. For instance, that pleasure was not divisible was considered as a fact; nothing in the concept of pleasure itself could tell us that it was not divisible. On the contrary, distance, in 1903, was defined as a set (of relations) having some properties that could be defined in purely logical terms. Thus, if one managed to prove that a certain kind of entity satisfied the formal requirements of distance, then one would have demonstrated, using logic alone, that the magnitude in question was a distance. The question: to be or not to be a distance was then viewed as a logical matter.[20]

Russell's distinction between divisibility and distance came from an early and neglected work by Meinong, entitled *Über die Bedeutung der Weberschen Gesetzes*. Focusing on the notion of a segment, Meinong claimed that we always confused the length (essentially divisible) with the distance between the two extremities of the segment (which is a relation between two points).[21] Now, Russell's key idea about metrical geometry can be viewed as a generalization of Meinong's claim. People usually reasoned as if there were only one sort of metric – while, for Russell, two different notions of metric coexisted: one was a magnitude of divisibility, the other, a distance. As there were two different kinds of metrics, there were two different questions to ask:

1) Is the geometry which derives the metric from the magnitude of divisibility empirical?
2) Is the geometry which derives the metric from the notion of distance empirical?

Russell answered yes to 1), and no to 2). Klein's projective definition of the metric was precisely a way of showing how a distance, in the relational sense Russell gave to the word, could be defined on a projective space. As projective geometry belonged to pure mathematics, metrical geometry, developed as a theory of distance, belonged to logic. But in *The*

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16 See Russell 1903, 159.
17 *Ibid*., 170-175.
18 Properly speaking, this is not true – Russell made a distinction between quantity (the concrete items which were compared) and magnitude (the abstract concept shared in common by all equal quantities). Now, only quantities could be divided; magnitudes were said to be never divisible; see 1903, 157-168, 173. Thus, properly speaking, what should be said is that Russell based the addition between magnitudes on the divisibility of the corresponding quantities.
19 On the notion of distance, see *Ibid*. 171, 253. The meaning of the notion is very large; in particular, the concept is not to be restricted to the geometrical domain.
20 *Ibid*. 408, 412.
21 See Russell 1899.

Principles, there was another kind of geometry, which dealt with the magnitude of divisibility. The properties of this last sort of magnitude were not given *a priori*, but *a posteriori*. Indeed, that a certain segment (for example) was twice as long as another one (i.e. that it contained a divisibility twice as great as the other one) was taken by Russell to be a perceptual fact.[22]

We are now in a position to understand Russell's conclusion on metrical geometry (428):

> Although the usual so-called projective theory of distance (…) is purely technical, yet such spaces do necessarily possess metrical properties, which can be defined and deduced without new indefinables or indemonstrables. Metrical geometry, as an independent subject, requires the new idea of the magnitude of divisibility of a series, which is indefinable, and does not belong, properly speaking, to pure mathematics. (…) Thus there is a genuinely distinct science of metrical Geometry, but, since it introduces a new indefinable, it does not belong to pure mathematics in the sense in which we have used the word in this work. (…) On the other hand, projective and descriptive Geometry are both independent of all metrical assumptions, and allow the development of metrical properties out of themselves; hence, since these subjects belongs to pure mathematics, the pure mathematician should adopt their theory of metrical matters.

The difference between the statuses of the two distinct metrical geometries is here clearly drawn.

4- Our detour via *The Principles*, Part III, has not, however, given us what we were seeking, i.e. the means to answer our initial question: why did Russell not regard the metric as a purely mathematical notion? This detour has only allowed us to reshape its formulation: why did Russell claim that, in addition to the projective notion of distance, another more fundamental (empirical) concept of metric existed?

A first possible answer could take the following shape. That there is a perception of length is a datum that every theory of metrical knowledge has to take into account. So, even if a formal theory of distance were to be developed, it could not be taken as a complete theory of metrical geometry, because it is a fact that there is a perceptual intuition of length.

This argument would certainly be the one that Russell would have used to oppose Coffa's trivilization claim. One could perhaps axiomatize geography, and then find a set-theoretical model of it. But this would not mean that geography is logical, because geographical knowledge obviously relies on certain elaborate empirical data, and because it is obvious that this fact should be taken into account in the analysis of geographical knowledge. In other words, Coffa seems to believe that the distinction between *a priori* (logic) and *a posteriori* (empiric) is something that logicism should establish. But in many cases, the distinction itself is regarded as a datum that the logicist should just acknowledge.[23]

Thus, if every one were to agree that we perceive distances, the projective definition of metric would then be universally regarded as a mathematical description of an empirical concept,

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[22] Russell 1903, 178.

[23] This is the answer to the trivilization challenge that could be extracted from Musgrave 1977, 121-122: "Of course, no If-thenist *does* apply the If-thenist manoeuvre in such cases: even the most ambitious logicist baulks at assimilating sociology or economics or physics or Greek mythology to logic. Hence he must have an *independent* reason for treating mathematics differently. And the logical empiricists did, of course, have such a reason: their central dogma that there are empirical assertions and logical assertions, but nothing else." However, Musgrave views this feature as a weakness: "What does the philosophical work here is the logical/empirical dichotomy, not If-thenism itself. This is simply the philosophical weakness of If-thenism (…)." (*Ibid*.). On the contrary, I am trying here to view the articulation between logicism (if-thenism) and the "independent reasons" Musgrave alludes to as a great advantage of the program.

and not as a definition of a logical notion. The question would thus be easily solved. The problem is that not every one, at the beginning of the XXth Century, thought that metrical notions were abstracted from perception. As is well known, Poincaré developed a group of arguments purporting to show that the notion of a length was not derived from experience. It was then not obvious that we had an intuition of metrical facts. To defend the idea that we had, in addition to the logical notion of projective distance, an empirical concept of magnitude of divisibility, a new argument was needed.

In the passage quoted above, Russell attached much importance to what we could call the architectonic features of the various theories. He distinguished, thus, between metrical geometry as a part of the projective theory, and metrical geometry as "an independent subject", as "a genuinely distinct science". There lies, I think, the kernel of Russell's reasoning. Why exactly was it not possible, in 1903, to define distance as a logarithm of an anharmonic ratio? Because it would construe metrical geometry as a very marginal science. Let me quote Russell (425):

> It may well be asked, however, why we should desire to define a function of two variable points possessing these properties. If the mathematician replies that his only object is amusement, his procedure will be logically irreproachable, but extremely frivolous. He will however scarcely make this reply. We have, as a matter of fact, the notion of a stretch, and (…) we know that the stretch has magnitude. (…) Thus the (projective) theory of distance, unless we regard it as purely frivolous, does not dispense with the need of (the theory of magnitude of divisibility). What it does show (…) is that, if stretches are numerically measurable, then they are measured by a constant multiple of the logarithm of (a certain) anharmonic ratio.

The projective approach of metric did not explain why the notion of distance was so important in the geometrical sciences; it did not explain that "we have, as a matter of fact, the notion of a stretch [i.e. of a divisible segment]". From Klein's perspective, the various restrictions of the anharmonic ratio appeared as the result of a mathematical whim that nothing, except frivolity, could justify. How, then, can we explain the fundamental role that metrical geometry played and still plays in mathematical sciences?

Ironically, on this point, Russell completely agreed with Poincaré's diagnosis concerning the projective derivation of the metric (1898, 28):

> Here lies the weakness of (Staudt's) theory, as appealing as it appears. To define the notion of a length as a particular case of the anharmonic ratio is an artificial detour, which we are reluctant to make. This is obviously not that way that our geometrical notions developed.

Poincaré rejected v. Staudt's projective foundation of geometry because it gave metrical theory the appearance of a frivolous invention of some mathematical genius. Russell shared Poincaré's diagnosis but, at the same time, he stuck to the projective approach. His solution was to use Meinong's insight and to split metrical geometry into two distinct disciplines. A projective definition of distance remained possible but it was not viewed as a satisfactory approach because it did not explain the centrality of metrical geometry.

Of course, Russell's solution compelled him to admit, against Poincaré, that we have, after all, a perceptual access to the metric. I do not have here the time to expound his defence of this controversial position.[24] The point I would like to emphasize, however, is that the assumption

[24] Russell's clearest passage on this point is a letter to Couturat dated 4/04/1904.

that distances are perceived, far from being taken as a primitive datum, is only a consequence of Russell's acknowledgement of the independence and centrality of metrical geometry within the mathematical sciences. This point is crucial because it shows that, in his logical reconstruction of mathematics, Russell wanted to take into account the fine-grained structure of his target. If a logical derivation of a given theory blurred the inner organization of the field, then this reconstruction, even if "logically irreproachable" (as is Klein's derivation), should be abandoned. Contrary to what Coffa's trivialization argument presupposes, it is not the case that just because Russell had the technical means to annex a given field to logic that he believed he should therefore do so. The derivation could threaten the place that a body of knowledge had in the scientific architecture, and if this was the case, then the logicist had to renounce what appeared to be a mere formal trick.

To summarize, Coffa's argument construes Russell's program as a purely formal method, according to which what can be derived from logic should automatically be granted the status of a logical knowledge. But Russell's actual project did not resemble this caricature. Indeed, the logical reconstruction had to satisfy some not purely formal constraints. First, some epistemological constraints were to be taken into account: the fact that a target theory relied on empirical grounds had to be included in the data of the logical analysis. Second, and more interestingly, some architectonic constraints were also to be respected: the main division of the body of knowledge under consideration had to be preserved in the process of analysis.

5- As I said at the beginning, one can distinguish four elements in Coffa's interpretation: 1) the thesis that metrical geometry is a special field where we have to distinguish colegitimate alternatives; 2) the thesis that metrical geometry belongs to logic; 3) the thesis that the trivialization argument applies to Russell's logicism; 4) the thesis that this argument deprives Russell's program of any philosophical interest. In what precedes, I have defended the third claim, but objected to the other three. The easiest part of the task was to refute Coffa's idea that metrical geometry was seen by Russell as a very special domain where several non-equivalent theories coexisted: the whole story told in the part VI of *The Principles* amounted to present metrical geometry as a particularization of the all encompassing projective theory. To understand why logicists should not be afraid of the trivialization argument, one has to look again at the geometrical case. Russell had the means to derive metrical geometry in a purely logical way. However, he chose not to do so. Why? Because Russell thought that a good analysis of metrical geometry should explain why metrical geometry occupied a central role in mathematics. The projective derivation did not satisfy this constraint and that is the reason why Russell rejected it. Lesson: the fact that you can derive a certain scientific discipline from logic does not mean that you should automatically do so.

I suggested, in my introduction, that the true nature of Russell's logicism stood out only against the background of an informed description of the surrounding mathematical world. I hope to have made this assumption more palatable. Indeed, what emerges from what I have said is that some local considerations were always taken into account by Russell during his analysis, and that, accordingly, logicism should not be taken as a mere formal method, which would apply everywhere in the same way. The theory of metrical geometry is especially impressive in this regard, since it shows that Russell preferred limiting the domain of pure mathematics (i.e. preferred excluding metrical geometry from logic) to blurring the architectonic relations between the various branches of the mathematical sciences. Rather than a global and formal method of reduction, logicism should thus be seen as an effort to locate the different mathematical branches in the same common space, the logical space. Russell sought to account for mathematics as an *organized* whole; if a logical move led to distort the mathematical architecture, then he considered the move to be a mere formal trick, destitute of any conceptual value. Russell did not consider logic as a ground against which all the mathematics should be flatten – he viewed it, rather, as a space within which the various mathematical sciences should be embedded.[25]

Many historians of mathematics and many mathematicians seem to believe that Russell's logicism, had it succeeded, would have provided us with a uniform method of reduction, applicable to every parts of mathematics in the same way and blind to the specificities of the various mathematical disciplines. This picture is also shared by many Russellian scholars, who often focus their attention on the logical part of Russell's works. I have here pleaded for another view of Russell's program, according to which logical analysis, rather than be seen as a general *method*, should be viewed as an *art*, that is, as a way of adjusting the global requirements coming from the logical framework to the urgent demand of not losing the specific features of the structured data from which the analysis proceeds. If more works are needed to substantiate this reading, I hope, here, at least, to have furnished enough elements to give the claim a clear meaning.

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[25] For more on the relation between logic and differences within mathematics, see Gandon 2008.

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