

**THE RUSSELL - POINCARÉ DEBATE**

THE RUSSELL - POINCARÉ DEBATE CONCERNING THE  
FOUNDATIONS OF GEOMETRY AND THE NATURE OF SPACE

By

TIMOTHY JOHN GROSS, B.A.

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AUTHOR: Timothy John Gross, B.A. (Marquette University)

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## PREFACE

The aim of this paper is to deal with one aspect of an exchange in the journals between Bertrand Russell and Henri Poincaré from 1896 to 1900, namely the question of "What, if anything, is the empirical content of Geometry?", as well as the question of "What the answer to this will shed upon the nature of Space?"

This exchange, hereafter referred to as the "Russell - Poincaré Debate", finds its root in Russell's first major philosophical work, his Cambridge dissertation, An Essay On The Foundations Of Geometry. Here Russell first proposes that there is an empirical content to Geometry. An overly flattering review of this Essay by L. Couturat, as well as Bertrand Russell's article "Are the Axioms of Euclid Empirical?", prompted Poincaré to publish a critical reply, "Des Fondements de la Géométrie, a propos d'un livre de M. Russell." To this Russell replied in an article entitled "Sur Les Axiomes de la Géométrie", to which Poincaré wrote "Sur Les Principes de la Géométrie, réponse à M. Russell". Finally, Poincaré included a chapter on "Space" in his Science and Hypothesis. Poincaré's position remains consistent throughout this exchange: that the axioms of geometry are conventionally, but not empirically, chosen.

The various arguments which Russell and Poincaré advanced on behalf of their respective "empiricist" and "conventionalist" theses are set forth at length in the first chapter of my thesis. Russell's early work on projective geometry is noted, and his views concerning the possibility of empirically investigating the nature of space are pulled out of various reviews written by him at this time. The interchange between Russell and Poincaré next presented has (to the best of my knowledge) never before been translated, nor has its import for the conventionalist thesis hitherto been analyzed.

The second chapter of my essay consists of a discussion of various philosophical problems concerning space, time, and geometry, which grow out of the Russell-Poincaré exchange. The philosophical consequences of the conventionalist viewpoint is assessed; here it is argued that Poincaré's conventionalism was more than merely "trivially linguistic". Discussion of this question is focused upon the role of co-ordinative definitions in certain astronomical experiments. Various objections to conventionalism are also discussed.

In the third and final chapter the validity of the conclusions contained within Russell's dissertation is examined in light of contemporary developments in mathematics and the physical sciences.

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## CHAPTER I

### THE RUSSELL - POINCARÉ DEBATE

#### A. An Essay on the Foundations of Geometry

The problem Russell tackles in his Cambridge dissertation can be stated: What geometrical knowledge must be the starting point for a science of space and must also be logically necessary to the experience of any form of externality.<sup>1</sup> Nineteenth Century non-Euclidean geometries had demonstrated, contrary to Kantian prejudices, that the mind is not restricted to thinking in Euclidean terms, thus casting doubt upon the Euclidean nature of space. Where it had once been held, following Kant, that the laws of Euclid were synthetic *a priori* judgements, and that the space of Euclid constituted a fundamental intuition, it became clear to the perceptive young Cambridge mathematician that perhaps not all of Euclidean geometry is logically necessary to the experience of space.<sup>2</sup> In his *Essay*, Russell distinguished projective and metrical geometry; he describes projective geometry as dealing with the descriptive or qualitative properties of space, while he describes metric geometry as dealing with the quantitative properties of space. Since Euclidean and non-Euclidean geometries are metrical, and since every metrical geometry can be resolved into projective geometry, Russell argues that the qualitative properties are logically prior to the Euclidean properties of space, or, in other words,

that both Euclidean and non-Euclidean geometries can be derived as special cases of projective geometry.

Perhaps before delving into an analysis of Russell's claims, a brief discussion of the nature of projective geometry is in order.<sup>3</sup> The undefined terms usually given in contemporary treatments of projective geometry are "point" and "line"; the undefined relation usually given is "incidence"; the two dual axioms underlying projective geometry are stated as "Each two distinct points are incident with a unique line" and "Each two distinct lines are incident with a distinct point". The co-ordinates of projective geometry are not to be identified with the Cartesian co-ordinates often used by physicists and engineers in dealing with applications of metric geometry. Projective geometry deals with the properties of a figure which are invariant under projection and section; these properties include the concurrence of lines, the collinearity of points, and the "cross-ratio". Thus in diagram A (see footnote 4), points A, B, C, and D on line L are projected onto line L' as points A', B', C', and D'. Morris Kline observes that

One might venture that perhaps the ratio of two segments, say  $A'C'/B'C'$ , would equal the corresponding ratio  $AC/BC$ . This conjecture is incorrect. But the surprising fact is that the ratio of the ratios - namely

$$(A'C'/C'B')/(A'D'/D'B')$$

will equal  $(AC/BC)/(AD/DB)$ . Thus this ratio of ratios, or cross-ratio as it is called, is a projective invariant... It is necessary to note only that the lengths involved must be directed lengths; that is, if the direction from A to D is positive, then the length AD is positive,<sup>5</sup> but the length DB must be taken as negative.

No sooner do we have the ointment than a fly appears; Kline continues:

If projective geometry is indeed logically fundamental to Euclidean geometry, then all the concepts of the latter geometry should be defined in terms of projective geometry. However, in projective geometry as described so far there is a logical blemish: our definition of cross-ratio, and hence concepts based on cross-ratio, rely on the notion of length, which should play no role in projective geometry proper because length is not an invariant under arbitrary projection and section... This blemish was removed by the nineteenth-century geometer Karl G. C. von Staudt, who showed how to define cross-ratio in terms of projective concepts.<sup>6</sup>

More will be said later concerning the relationship of projective geometry to Topology; it is hoped that the brief discussion of projective geometry given above will help the reader in his evaluation of the claims made by Russell in his dissertation.

In his Essay Russell agrees with Kant that the mind must possess some form of externality in order to experience space, but disagrees with Kant that the a priori properties of any form of externality are the laws of Euclid, holding instead that they, the a priori properties, are the laws of projective geometry. The four basic principles of projective geometry, according to Russell, are the homogeneity and relativity of space, the continuity of space, that two points determine a line (and three noncollinear points a plane, and so on to higher order figures), and that the dimensions of space must be of a finite number. To these laws of projective geometry Russell adds four corresponding laws of metric ge-

ometry, namely those of free mobility (equivalent analytically to the constant curvature of space), the distance axiom (of congruence, that two points determine a distance which remains unaltered in any motion of the two points as a single figure), the axiom of rigid transformation or motion, and, as kept from above, that the dimensions of space must be of a finite number. On philosophical grounds, arguing that space must be homogeneous, Russell limits our choices of physical spaces to one of Euclidean, hyperbolic, spherical, or single elliptic. Arguing from empirical grounds, Russell believes that our physical space is Euclidean and three-dimensional. It is this last claim which is now under close scrutiny.

In his Introduction to the Essay, Russell defines the problem of the a priori by its relation to logic, psychology, and mathematics. In section four he distinguishes between the a priori, which belongs to Epistemology, and the subjective, which belongs to Psychology; the a priori, Russell notes, comprises the proper subject of his investigation.<sup>7</sup> Russell claims that his test of the a priori will be purely logical: to discover what knowledge is necessary for experience.<sup>8</sup>

In the next section of his Essay, concerning the history of metageometry, Russell first discusses the difference between convention and empirical investigation in geometry; Russell titles section 33: "Hence Euclidean space"

appeared to give rise to all kinds of Geometry, and the question, which is true, appeared reduced to one of convention.<sup>9</sup> Note the use of "appears" in this discussion of the choice of geometries. Russell writes:

Since these systems are all obtained from a Euclidean plane, by a mere alteration of the definition of distance, Cayley and Klein tend to regard the whole question as one, not of the nature of space, but of the definition of distance. Since this definition, on their view, is perfectly arbitrary, the philosophical problem vanishes, and the only problem remaining is one of convention and mathematical convenience.<sup>10</sup>

This charge, that the choice of a Geometry for space by conventionalists was "perfectly arbitrary", was to be leveled several times in the upcoming exchange against Poincaré's position; on the other hand, Poincaré would reply that the choice is not altogether arbitrary, being guided by the principle of simplicity and of strength of explanation concerning the theories to be chosen from. Russell continues:

This view was then forcibly expressed by Poincaré: "What ought one to think", he says, "of the question: Is the Euclidean Geometry true? The question is nonsense." Geometrical axioms, according to him, are mere conventions: they are definitions in disguise.

Poincaré's contention, that the axioms of geometry are only "definitions in disguise", led Russell later in this exchange to discuss the general differences between mathematical and philosophical definition. In any case, Russell considers Poincaré's conventionalism to be the result of a confusion concerning the nature of the co-ordinates of metrical and projective geometry.

The view in question has arisen, it would seem, from a natural confusion as to the nature of the co-ordinates employed... But projective co-ordinates - so our argument will contend - though perfectly adequate for all projective properties, and entirely free from any metrical presupposition, are inadequate to express metrical properties, just because they have no metrical presupposition.<sup>12</sup>

In a following section Russell claims that projective co-ordinates, which he considers to be purely descriptive, yield no information as to metrical properties.

This confusion, I believe, has actually occurred, in the case of those who regard the question between Euclid and Metageometry as one of the definition of distance. Distance is a quantitative relation, and as such presupposes identity of quality.<sup>13</sup>

Poincaré is directly charged with misconstruing the nature of projective geometry, and of holding a confused position concerning the relation of distance to metageometry.

But the arbitrary and conventional nature of distance, as maintained by Poincaré and Klein, arises from the fact that the two fixed points, required to determine our distance in the projective sense, may be arbitrarily chosen, and although, when our choice is once made, any two points have a definite distance, yet, according as we make that choice, distance will become a different function of the two variable points. The ambiguity thus introduced is unavoidable on projective principles; but are we to conclude, from this, that it is really unavoidable? Must we not rather conclude that projective geometry cannot adequately deal with distance?<sup>14</sup>

First, it should be noted that Russell does not disagree with Poincaré's claim that the choice of a defining metric is conventional. What Russell does claim, however, is that projective geometry, because it does not contain metrical presuppositions, cannot adequately deal with metrical con-

siderations. However, this does not refute Poincaré's further claim that all of the axioms are conventionally chosen, that is, that in one system a choice might be made in favor of Euclid's parallel postulate, while in another system the opposite choice might be made. Second, in attempting to refute Poincaré's position regarding our choice of a defining metric, Russell relies upon "the irreducible metrical properties of space".

But the choice of the base-points, when we are discussing distance in the ordinary sense, is not arbitrary, and their introduction is only a technical device. Distance, in the ordinary sense, remains a relation between two points, not between four; and it is the failure to perceive that the projective sense differs from, and cannot supersede, the ordinary sense, which has given rise to the views of Klein and Poincaré. The question is not one of convention, but of the irreducible metric properties of space.<sup>15</sup>

Russell here appears to appeal to an intrinsic metric for space; the relation between four points, which Russell rejects as the foundation of the concept of distance, refers to the relation of superposition between the two endpoints of the length being measured in relation to the two endpoints of a chosen defining length. Russell points out, however, that to argue successfully in favor of the empirical nature of metrical geometry cannot be accomplished by merely holding that distance is a metric definition, for he notes that "the metrical definition of distance...is the same in Euclidean and non-Euclidean spaces; to argue in its favor is not, therefore, to argue in favor of Euclid."<sup>16</sup>

Later in his Essay, while considering recent French speculation concerning Geometry, Russell again mentions the views of Poincaré.

The foundations of Geometry have been the subject of much recent speculation in France, and this seems to demand some notice... The chief writers have been, from the mathematical side, Calinon and Poincaré, from the philosophical, Renouvier and Delboeuf; as a mediator between mathematics and philosophy, Lechalas.<sup>17</sup>

The following comprises the extent of Russell's discussion of Poincaré's contributions to the subject:

Poincaré maintains that the question, whether Euclid or Metageometry should be accepted, is one of convenience and convention, not of truth; axioms are definitions in disguise, and the choice between definitions is arbitrary. This view has been discussed in Chapter I, in connection with Cayley's theory of distance, on which it depends.<sup>18</sup>

First, in Poincaré's defense, it must be noted that Poincaré is using "definition" in an unusual sense when he speaks of the axioms as "definitions in disguise". Next, Poincaré would not claim that the choice between systems is purely arbitrary, but rather that it is dictated by simplicity when the strength of explanation is equal. Finally, Poincaré is not arguing against anyone's use of Euclidean terminology in talking about real space; what Poincaré would argue against is anyone's claim that Euclid's geometry is the only true Geometry, the only possible description of our space.

In the following section Russell proposes his own view, namely that all homogeneous spaces are a priori possible, but that the decision between them is empirical.

In his Essay Russell provides no clear statement of what he means by "empirical choice" or "empirical investigation".

Russell states that

...we saw that there are two senses in which Meta-geometry is possible. The first concerns our actual space, and asserts that it may have a very small space-constant; the second concerns philosophical theories of space, and asserts a purely logical possibility, which leaves the decision to experience... we conceded that Geometry, when applied to mixed mathematics or to daily life, demands more than this; demands, in fact, some means of discovering, in the more concrete manner of Mechanics, either a rigid body, or a body whose departure from rigidity follows some empirically discoverable law. Actual measurements, therefore, we agreed to regard as empirical.<sup>19</sup>

Thus Russell is aware of the fact that, if an empirical determination is to be possible, rigid rods, or rods which deform according to a known law, are needed to effect the measurements of such experiments. On the next point Russell and Poincaré agree: that all spaces are a priori possible; they disagree, however, on how one space is to be chosen as representative of real space.

Finally, we discussed the question of absolute magnitude, and found in it no logical obstacle to non-Euclidean spaces. Our conclusion, then, in so far as we are yet entitled to a conclusion, is that all spaces with a space-constant are a priori justifiable, and that the decision between them must be the work of experience.<sup>20</sup>

Much later in the Essay, (section 140), Russell returns to the contention that metric geometry contains an empirical element, for he writes:

The Euclidean and non-Euclidean spaces give the various results which are a priori possible; the axioms peculiar to Euclid - which are not properly axioms, but empirical results of measurement - determine, within the errors of observation, which of these a priori possibilities is realized in our actual space.<sup>21</sup>

Russell held that the co-ordinates of projective Geometry, far from being spatial magnitudes, were only convenient names for points.<sup>22</sup> On the other hand, the co-ordinates of metric Geometry deal with magnitudes:

But projective Geometry, in spite of its claims, is not the whole science of space, as is sufficiently proven by the fact that it cannot discriminate between Euclidean and non-Euclidean spaces. For this purpose, spatial measurement is required; metrical Geometry, with its quantitative tests, can alone effect the discrimination.<sup>23</sup>

Metrical Geometry is, then, a necessary part of the science of space, and a part not included in descriptive Geometry. Its a priori element, nevertheless, so far as this is spatial and not arithmetical, is the same as the postulate of projective Geometry, namely, the homogeneity of space<sup>24</sup> or its equivalent, the relativity of position.

Russell thus argued that the choice between metrical geometries is empirical. Riemann had introduced spaces with a variable metric, that is, with a metric which varies from point to point. Russell, however, believed that the only possible spaces would have a constant measure of curvature, referred to above as the "space-constant". Russell went on to advance three arguments against Riemann's conception of a variable space-constant.<sup>25</sup> First, a constant change of metric has no significance: were geometrical objects to change in size due to changes in position, we could still superpose figures that are congruent in one position when they are both in

another position. Second, the surface of a space of non-constant curvature may be described in terms of a series of infinitesimal Euclidean planes. Finally, the denial of the fundamental property of congruence is, according to Russell, pointless, senseless, and impossible to know.

### B. Russell's Views on Space and Time circa 1896

Russell's review of Georges Lechalas' Etude sur l'espace et le temps<sup>26</sup> provides a further insight into Russell's views concerning the empirical nature of the choice between Euclidean and non-Euclidean space. First, Russell agrees with Lechalas that, in metageometry, all of Geometry flows from the definitions of space, "and definitions do not involve the existence of their objects. The justification of a definition lies in the absence of contradiction in its results. Thus Geometry is apodeictic, but the decision between Euclid and non-Euclid is empirical."<sup>27</sup>

Russell also mentions Lechalas' problem of similar worlds and the reversibility of the material universe. Russell states, on his own authority, that the "former problem is meaningless, since a proportional change of all temporal and spatial magnitudes would be no change. As to the latter, a reversed world would be unstable and improbable."<sup>28</sup>

C. Couturat's Review of Essay on the Foundations of Geometry

Couturat's review of Russell's Essay, as will be seen shortly, was responsible in part for Poincaré's initial critique of Russell's position. This review, presented in the leading French journal of philosophy and mathematics, Revue de Métaphysique et de Morale, begins by noting that Russell does not distinguish, as Kant did, between the a priori and subjective aspects of space.<sup>29</sup> This view is not entirely correct; Russell considered the a priori to be the proper concern of Epistemology, and the subjective the concern of Psychology, yet he intended in the Essay to discuss only the first of these. Couturat also endorses Russell's claim that the laws of Euclid are open to empirical verification:

...the theorem, "that the sum of the angles in a triangle equal two right angles", that all of the metaphysicians from Descartes to Kant have considered to be a necessary truth, has become for the modern geometers a truth of experience, contingent and approximate.<sup>30</sup>

Couturat hits in his own brief review the sparse mention which Russell makes of Poincaré in his Essay:

(Russell) passes briefly in review of the French work relative to this subject, discussing...the nominalism of M. Poincaré...<sup>31</sup>

D. Two Reviews by Russell in 1898

Between Couturat's review of the Essay and Poincaré's first reply to Russell, two reviews of works on Mechanics were put forth by the young Englishman.

In July, 1898, Russell reviewed A.E.H. Love's Theoretical Mechanics<sup>32</sup>; Russell took this occasion to mention again whether the empirical results obtained are exact, or are only approximats. He then discusses, for several pages, the philosophical import of the views of motion as absolute or relative; Russell here argues for the absolute view.<sup>33</sup>

In October, 1898, Russell reviewed Edmond Goblot's Essai sur la classification des Sciences,<sup>34</sup> taking this opportunity to again jibe at Poincaré's doctrine of conventionalism in geometry:

After a somewhat rambling discussion of projective and metrical geometry - which, by the way, contains several mistakes, as for example, that the axiom of parallels is independent of metrical considerations (p.91) - the author proceeds to non-Euclidean Geometry, which he declares, following Poincaré, to be equally capable of explaining phenomena, and only to be rejected as being less convenient.<sup>35</sup>

Russell's position in this review clearly is that the choice between geometries is based, not upon convenience, but upon empirical observations (the "metrical considerations").

#### E. Russell and the Exact Empirical Investigation of Geometry

The next link in this chain of argumentation was provided by Bertrand Russell, whose article of November, 1898,<sup>36</sup> was intended as a demonstration that the axioms of Euclid were empirical, and in fact were more than just "close approximations". Russell opens "Les Axiomes Propres à Euclid: sont-ils empiriques?" with mention of Couturat's review,

described by Russell as "very penetrating and very flattering":  
 In this review, Russell remarks, "showed that the reasons given by me (in the Essay) for considering the axioms proper to Euclid as empirical, and not just close approximations, as correct."<sup>37</sup> Russell is hedging his bets, for he cautions:

I am very far, nevertheless, from holding this position as correct; it seemed to me still, considering all points, the most probable position, but I know of all the difficulties of dogmatically affirming one solution for all, and, on important points, I admit the correctness of the objections of my critic.<sup>38</sup>

Russell next states clearly what he hopes to accomplish in this present article:

The study by M. Couturat raises some important questions, but I intend to limit myself to the most important among them, to the question of knowing if the axioms proper to Euclid are empirical. I propose to present my arguments in three parts. In the first, I will discuss in detail the criticisms running through the study of the question... In the second, I will present some brief and general remarks on the a priori, and in particular on the a priori intuitions. Finally, I will discuss the positive reasons for which I think the axioms of Euclid are empirical.

Russell begins by directly attacking Poincaré's position, mentioning that scientist-philosopher by name; in this passage it is claimed that a decision for or against the Euclidean axioms can be made, since Russell considers them to be either true or false; this stand rejects Poincaré's claim that no such decision can be made since the axioms are neither true, nor false, but chosen by convention.<sup>40</sup>

Russell raises two questions which must be examined in any discussion relative to the empirical proof in favor of one geometry:

The first, which is purely philosophical...In all possible or imaginable experience, to what limit is it possible to assign to magnitude a spatial constant? Is it possible to determine empirically...the nature of our space? The second question, which is scientific, is this: What are the best experiences, and what are the most exact determinations, in which one comes in contact with the magnitude of the spatial constant?

Russell notes that his colleague and teacher, A.N. Whitehead, suggested to him that, rather than arriving at a great degree of accuracy in determining the space-constant, it is sufficient to determine the narrow limits in which it must lie.<sup>42</sup> Russell now proposes an experiment, such as might be used as a prototype, by which the limits of the constant for the curvature of space might be discovered:

Take an ordinary disk - such as a piece of money - with a point marked on it. Turn it one complete revolution along a straight line, and measure the length of the line and the disk's circumference. In this manner one may determine the relation of the circumference of the circle to its diameter, and so the value of the space-constant can be deduced.

Russell continues this with the statement that this experiment does not depend, at least in theory, upon the measurement of distances - for it is the ratio which we are seeking - yet he concludes this train of thought with the concession that his experiment presupposes the possibility of determining straight lines. Russell's apology for this is

that

The possibility of determining straight lines is implied, it is true, but this possibility is also a condition for measurement...<sup>44</sup>

yet he has just stated that measurement is not necessary for the success of the experiment. To put this another way, Russell first argues that measurement is not strictly necessary for the success of the experiment, yet he justifies his presupposition that straight lines can be determined by citing their role as a condition for the measurement he has just stated is not strictly needed. In any case, Russell's conclusion is that

If the ratio of the circumference to the diameter, resulting from experience, also is found as near to PI as the errors of observation we conduct could foresee, this would show that the spatial constant is considerably greater than the diameter of our disk.<sup>45</sup>

Russell's conclusion will be discussed during the course of the following critique of Poincaré's reply to Russell.

Russell now states that an exact determination that our space is Euclidean will probably never occur, yet, as with other laws of science, he claims that a certitude can slowly be established as more and more empirical evidence is found to support the Euclidean hypothesis; this hypothesis is also noted as being the simpler choice among geometries.

This experience suffices to show that with these *a priori* axioms alone, we are able to obtain that empirical evidence relative to the nature of our space... That we will ever be capable of determining, with certitude, that our space is Euclidean, on the contrary, is to be regarded as entirely impossible.<sup>46</sup>

Again,

We are assured, by the reasons of empirical order, that our space is approximately Euclidean; that it is rigorously Euclidean is something we do not know and will never know.<sup>47</sup>

By way of commentary on this, it might be pointed out, in agreement with Russell's last statement, that our world does indeed appear Euclidean, at least when only a small scale is considered; two "parallel" lines, such as the top and bottom of a picture frame, might intersect one another if extended for a million miles on either side (in the plane in which they are assumed to lie - we wish to avoid the case of skew lines), yet this fact would never be known.

However, and this point will be returned to in the concluding stage of this paper, experimental evidence from astronomy has, to a degree, confirmed the opposite of Russell's conclusions, or at least provided counter-examples. Russell, however, shunned dogmatism in his work; he acknowledges the possibility of discovering evidence in favor of non-Euclidean models for space, yet he considered this unlikely, holding instead that, as more evidence piled up, Euclidean space would be recognized as the limiting case. First,

Euclidean space is a limiting case, that, by agreement with measured distances, the other types of space approach indefinitely.

Next,

We may prove that this space is not Euclidean, but we can never hope to prove that it is rigorously Euclidean, just as we can never hope to prove the law of gravitation.

Finally,

...this is not a vicious circle, but only an ordinary form of inductive reasoning.<sup>48</sup>

Following this note of caution, Russell interjects one of the many brief observations concerning the philosophy of science which begin cropping up in this exchange:

Having seen that one series of hypothetical consequences is entirely compatible with resulting observations, and that we cannot prove that the entire system of hypotheses will not do, we accept that system of coherent hypotheses, for the time, which is sufficient for explanation.<sup>49</sup>

Adopting this view of scientific choice, taking the phrase "sufficient for explanation" to mean "minimally sufficient" (thus ordaining simplicity coupled with strength), Russell carries over Ockham's razor (that unnecessary entities, or hypotheses, should not be postulated) in favor of Euclid.

If the measures of the surface of the world are compatible with a null spatial constant, and if the supposition of the null spatial constant permits us to extend the laws of the earth to the movement of the celestial bodies, then this supposition is justified by its simplicity.<sup>50</sup>

Russell's conclusion is straightforward:

In summary, I have shown that the axioms unique to Euclid are susceptible to empirical proof, in the sense that the ordinary laws of science are susceptible... they constitute the most simple hypotheses for the explication of facts, such that we could imagine other facts which would render slightly more simple a non-Euclidean space. We have not proven with certitude that our space is rigorously Euclidean, and we cannot, if it is slightly non-Euclidean, prove with certitude that it is not Euclidean. But such uncertainty is inevitable in (all other laws) such as the law of gravitation, or the Copernican system, or all other scientific laws.<sup>51</sup>

Finally, Russell admits that his proposed empirical proof is very weak:

As for the (axiom of) three dimensions and the axiom of parallels, they are not to be called a priori if we conserve a psychological element in the a priori; for the a priori is defined, not by relation to our knowledge, but uniquely by relation to truth and necessity; there does not remain any reason for considering these axioms as a priori. That the proof of their empirical nature is weak and inconclusive, I admit without pain; but the proof of the contrary seems to have more flaws.

### Poincaré's First Reply

In May, 1898, Poincaré published his initial reply to Russell's charges. Opening his "Des Fondements de la Géométrie, à propos d'un livre de M. Russell,"<sup>53</sup> with reference to the Essay on the Foundations of Geometry and to Cournot's review, with the latter described as "un éloge peu banal", Poincaré describes the Essay as a work which is partly critical (noting the first two chapters of that work) and partly dogmatic (citing chapters three and four). Poincaré, in summary form, sets forth Russell's distinction between metrical and projective geometry: 1st, that projective geometry is entirely a priori, whereas metric geometry is in part empirical; 2nd, that projective geometry is independent of the concept of movement, which is implied in metric geometry. (by the need to superimpose supposedly congruent figures); 3rd, that projective geometry is qualitative whereas metric geometry is quantitative.<sup>54</sup>

Poincaré cites the three axioms which Russell ascribes to projective geometry, and then raises two questions:

1st, Are these axioms sufficient to constitute projective geometry, or are others needed?

2nd, Are these axioms, as Russell claims, the necessary consequences of a form of exteriority indispensable to all of our experience?<sup>55</sup>

Poincaré notes that the first three of Russell's axioms are set forth in the Essay as a priori, while the fourth, that space has three dimensions, is purported to be empirical; on the other hand, Poincaré notes, all that Russell is justified in claiming is that the dimensions of space are of a finite whole number.<sup>56</sup>

Poincaré next proposes to modify Russell's list of axioms for the purpose of eliminating vague or ambiguous expressions. Poincaré's axioms are these:<sup>57</sup>

1st, That space is a continuous manifold having three dimensions;

2nd, The points of this manifold are qualitatively indiscernible from each other;

3rd, Through two points pass one line, and one only;

4th, Through three points, not in a line, pass one plane, and only one, which contains the three points and which joins the three points two by two;

5th, A plane and a line always intersect.

Regarding this fifth axiom I might add that it is

not always true of metric geometries (for it is not true of Euclidean geometry); in projective geometry, however, an imaginary point is assumed to exist "at infinity".

Regarding the above list of axioms, Poincaré claims that none can be deleted.

Poincaré returns to his second question; his conclusion is that, contrary to Russell, "the axioms of projective geometry are not the indispensable conditions of our experience."<sup>58</sup> Poincaré agrees with Russell that some form of exteriority is necessary for the possibility of perceptual experience; that this form can be reduced to a collection of relations; and that this form of exteriority is homogeneous. Then, Poincaré states, Russell searches for a proof that this form cannot be homogeneous without being continuous and divisible to infinity; Poincaré says that "I do not understand the first word of his reasoning." Poincaré agrees with the argument that a form of exteriority which is continuous is an indispensable condition to certain forms of experience. Poincaré then states: "I will not quibble over the number of dimensions", referring to the fact that he believes Russell to have proven that our space has a finite whole number of dimensions (although Poincaré is not conceding that Russell has proven this number to be three). In concluding this section, Poincaré agrees that every two points can stand in a relation to each other, yet he questions one of Russell's presuppositions: "Why must this relation be a line, that is"

to say, a collection of other points?" Poincaré's argument, that the unique relation between two points forms a collection of other points only by convention, seems strong at this stage of the exchange.<sup>59</sup>

Later in his paper, in a section entitled "Empiricism and Geometry", Poincaré directly attacks Russell's claim that there is an empirical content to metrical geometry. Poincaré opens this section by citing his previous review of the axioms:

We have reviewed the axioms which Russell considers the indispensable conditions of experience...but they establish nothing; employed in them are vague terms and incorrect definitions.<sup>60</sup>

Poincaré then states his ground clearly:

But if these axioms are not indispensable conditions of experience, to what do we owe the belief that they are empirical?... That thought is far from my mind; on the contrary, I think that this is a mistake when M. Russell attributes the empirical character to other axioms, such as those of Euclid.<sup>61</sup>

Poincaré next attacks Russell's use of the word "empirical" as being too vague. He then argues, first, that there are no direct experiences of Geometry, but only of Optics, or of some other branch of Physics, and second, that any experimental confirmation of one type of geometry, were such confirmation to arise, could alternatively fall prey to explanation within the other systems of geometry.

Who can describe to me...the experience of an abstract line or circle. Where is the physicist who has seen a straight line, and with what instrument?...He has had an experience of optics, not of geometry.

Again,

Were we to discover a star for which parallax was negative, could we conclude that (Euclidean) geometry is false? No, it would be just as natural to conclude that the rays of light emanating from this star are not rigorously propagated in a straight line.<sup>62</sup>

Furthermore, Poincaré argues, our experiences from physical experiments comprise data to be explained, yet, he seems to feel, Russell is taking them to be explanations in themselves:

Is it by experiences of the movements of solid bodies that we are able to demonstrate the postulates fundamental to Geometry?... The knowledge which we have of the movements of solid bodies can not be the foundations of geometry; it has given us only the occasion of that founding. The role psychology<sup>63</sup> has is considerable, the role logic has is null.

(The above points Poincaré considers to be the most important.

In the following section, a continuation of this line of inquiry, Poincaré argues that no phenomenon is unique to one type of space or the other, but rather that the various geometries interpret or explain, in different manners, whatever data are observed.<sup>64</sup> Poincaré next asks if there are any phenomena which could only be interpreted in one metric geometry but not in others. This idea he rejects, for he claims that such a situation would be analogous to measuring a quantity in the laboratory in feet and inches but being unable to measure it in meters.<sup>65</sup>

Returning to his argument that an experience of negative parallax for a star could fall prey to alternative ex-

planations (either that space is non-Euclidean or that the rays of light were not propagated in a straight line).

Poincaré examines the general case of the possibility of discovering some property whereby a straight line could be distinguished from all other such lines; if such a criterion were available, a choice would have to be made in the above problem for one geometry or the other; of course Poincaré claims, and correctly I think, that such a property or criterion can never be discovered (since it does not exist).

He argues:

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Examine a question of great similarity. I suppose that the straight line in Euclidean space possesses two properties which I call A and B; that in non-Euclidean space they possess only property A, but do not possess property B; I also suppose... (that) in both spaces the straight line is the only line which possesses property A.

So may the experience be (described) which is apt to decide between the hypothesis of Euclid and that of Lobatchevsky. We establish that such a concrete object, accessible to experience, such as a pencil of light rays, possesses the property A; we conclude that it is rectilinear, and then seek to discover whether or not it possesses the property B.

But it is not so, there does not exist a property that powerful, with this property A, (which) is an absolute, permanent criterion for recognizing the straight line and for distinguishing that line from all other lines.<sup>66</sup>

Poincaré goes on to demonstrate why such a property, even if theoretically valid, is not workable, i.e. would not provide an experimental basis for decision; were such a property to be discovered it would have to be confirmed by measuring

distances, yet the latter, although known to a great degree of precision, are still known only approximately.<sup>67</sup> Such a charge could be levelled against any physical experiment, yet Poincaré seems to think that here it bears more weight, since an experiment to determine the property A would question the preconditions of measurement.

Poincaré challenges his critics to provide a concrete experience which could only be interpreted in a Euclidean system, and never in a non-Euclidean system:

No experience is ever in contradiction with the postulate of Euclid; in turn, no experience is ever in contradiction with the postulate of Lobatchevsky.<sup>68</sup>

Poincaré points out that it is not sufficient to state that Euclidean, or non-Euclidean, geometry can never be contradicted by experience; rather Poincaré goes on to show, by what he calls his "law of relativity", that all experiences are equally able to be included in the interpretations set forth by Euclidean or non-Euclidean systems of explanation.<sup>69</sup>

We have maintained the interpretations in the non-Euclidean hypothesis; this is always possible; only the non-Euclidean distances of different bodies in this new interpretation are not generally those of the Euclidean distances in the initial interpretation.<sup>70</sup>

Yet, Poincaré asks, would not this difference in distances serve to confirm or disconfirm non-Euclidean geometry? No, Poincaré replies, for his law of the relativity of observations can be applied rigorously to the entire universe, so that "If a law is true in the Euclidean interpretation, it

is also true in the non-Euclidean interpretation.<sup>71</sup>

In conclusion Poincaré voices an appeal for a reply from Russell to his objections:

I do not know if M. Russell desires to reply to these objections, which without doubt appear to him absurd, but if he does so, he will probably not wish to treat all of the questions I have touched upon... I demand from him in part to explain to me by what concrete experience he would demonstrate Euclid's postulate, and on the other hand to give me a definition of 'distance' and of 'straight line' independent of that postulate, and exempt from ambiguity and vicious circles.<sup>72</sup>

### G. Russell's Rejoinder

Russell opens his reply to Poincaré, entitled "On the Axioms of Geometry"<sup>73</sup>, by discussing what can be meant by stating that no experiment can be proposed that would show Euclid to be true or false:

1st, That the truth, or falsehood, of Euclid is a priori, and thus without need of verification;

2nd, That one Geometry is true and the others false, but it is impossible to know which is which; regarding this Russell states that:

M. Poincaré seems to accept (in section 13 of his paper) the Newtonian argument in favor of absolute motion. This necessarily implies, although I doubt whether M. Poincaré admits this implication, that we should also recognize absolute position. But no one would maintain that the absolute position of a body can be known.

3rd, That nothing can show Euclid to be true or to

be false, for the simple reason that Euclid (or any other geometry) is neither true nor false. The third position, Russell notes, via the view held by Poincaré.

Russell claims that many of Poincaré's arguments, put forth in support of point three above, also serve to support point two. Russell then draws the following distinctions:

1st. Is Euclidean geometry a convention, or is it true or false?, and,

2nd. Can we discover whether it is true or false?<sup>74</sup>

He then states that Euclidean geometry is either true or false, and that, in fact, this is discoverable. Russell also holds that the answer to the first question is philosophical, and a priori, never admitting the possibility of a conventional choice between geometries for the purpose of describing this or any other space.<sup>75</sup>

Russell, agreeing with Poincaré, cites the question at hand as one of the meaning of any statement in which the results of observations are set forth:

It is to be observed that the question is not as to the correctness of the results of measurement, but as to the meaning of the propositions in which these results are embodied.<sup>76</sup>

Russell next defines what he means by the phrase "the distance from A to B":

If certain rules are followed, the distance in question may be brought into a certain relation to a certain standard distance.<sup>77</sup>

Russell notes that these rules for the determination of distance relations must be "quite arbitrary"; he now admits that both he and Poincaré had made a mistake on this account:

Distance itself must be created by the conventional rules... This confusion existed in my own account of congruence, and exists also in M.Poincaré's article.<sup>78</sup>

In short, Russell accuses Poincaré of circularity in his definition of distance; to define two distances, AB and CD, as equal, depends upon knowing the distance of each, but this amounts to defining distance as a relation between distances.

This is very important, since on it turned the view, which I gather to be that of M.Poincaré, that distance depends upon measurement. This leaves<sup>79</sup> it an open question what it is that we measure...

Russell seems to think that, although measurement is required to discover equality or inequality in Poincaré's account, there cannot be equality or inequality without measurement, yet "what can be discovered by an operation must exist apart from that operation ... M.Poincaré, on the contrary, holds that measurement creates equality and inequality."<sup>80</sup> This final claim against Poincaré is not quite accurate; Poincaré does not claim that measurement "creates" equality or inequality, but rather would hold that two distances might be said to be equal if one system of explanation is adopted, yet might be said to be unequal if another such system were to be put into effect. Russell next considers a possible rejoinder on Poincaré's part, namely that it is not geometri-

cal figures but bodies which are measured:

But this implies that bodies have sizes and shapes and mutual distances concerning which something is known - an implication which must be false unless measurement applies to the geometrical figures of bodies, and gives approximately correct results as to these figures.<sup>81</sup>

Thus, Russell claims, any measurement of physical bodies involves a measurement of properties which are geometrical, and which involve a co-ordinate system, since every experiment in Dynamics involves matter changing position.

Russell continues by providing a correspondence-definition of distance, claiming that the true meaning of the proposition "the distance AB is a meter" is that "the two points A and B have a distance which is equal to another distance which we call a meter." Russell proposes the logical point: "Are there any existential propositions which would be true in a Euclidean world and false in a non-Euclidean world, or vice versa?" Three possible propositions are advanced:

1st, There exists a distance, namely that from London to New York, or from the earth to the sun, which is greater than one millimeter;

2nd, There exists three points, in Paris, Greenwich, and New York, which determine a triangle whose area exceeds one square millimeter;

3rd, There exist bodies, such as the earth, whose volume exceeds one cubic millimeter.<sup>82</sup>

What Russell is looking for are existential proposi-

tions which would be true under one interpretation and not under another, yet whatever geometry is chosen (note that I have not said "whatever definition by correspondence of a millimeter is given") it seems to me that these three propositions would keep the truth-value they have for our world.

Concerning the first two points, it may be imaginable that the surface of space is "folded over" in such a fashion that London and New York now lie extremely close together, etc., yet this does not mean that an empirical determination would be possible, since the observations required might not be obtainable. What relation would such an example of a topologically deformed space have to our ordinary notions of distance? If space were indeed "folded over" (as in Figure B of footnote 83), so that the distance from New York to London was decreased, would we be able to take such a short-cut in our travels from one city to another, or would we still have to travel the original distance? If space was "folded over" in some sense, it seems to me that taking the short-cut would somehow involve "jumping out" of our space. To me, then, such a short-cut seems to make no real difference with regard to our ordinary conception of the distance between two points in our space.

Russell replies that "These propositions, I say, are existential, and are either true or false... the admission of the alternative is all that I demand."<sup>84</sup> On the con-

trary, Russell has demanded more than that: he has demanded an existential proposition true in one geometry and not true in another geometry; yet these propositions, admittedly existential, and admittedly true or false within any given system, are not necessarily true in Euclidean geometry while false in a non-Euclidean geometry, i.e. each of these could be true in both or false in both types of geometry. If I understand Russell's argument correctly, he believes that his three propositions state matters of fact concerning intrinsic amounts of space such that they are true in a world governed by Euclidean geometry but might be false in a "highly distorted" non-Euclidean world (I mean by that a world with an extremely large degree of curvature for space). Russell, it appears to me, had failed to see that these statements do not involve choices of geometry but choices of systems of measurement. As noted above, life in a "highly deformed" non-Euclidean world would not cut the travelling distance, as we ordinarily understand it, between two points.

Russell again appeals to the concept of limiting values, claiming, in reference to his article of November, 1898, that all he desired to discover by experiment were those limits, however large, within which the space-constant must lie. On the other hand, Russell concedes to Poincaré the point that empirical evidence can never decide certainly in favor of any one position: "It shows that a certain hy-

pothesis accounts for the facts, but cannot show, as a rule, that no other hypothesis can account for them.<sup>85</sup> Again, Russell concedes Poincaré's point that alternative interpretations are possible in every case of geometrical experiment.<sup>86</sup> Regarding the experiment with a coin to determine the ratio of  $\pi$ , Russell claims: "The experiment illustrates either the nature of space, or the nature of bodies, whichever is considered as unknown."<sup>87</sup> This reply will not suffice, for what was to be investigated was the nature of space, yet this experiment might only concern the nature of bodies. Russell notes that this objection was conveyed privately to him by Poincaré concerning the coin experiment. Russell, however, believes that the coin experiment illustrates the geometrical properties of bodies:

Either solid bodies have no properties illustrated by such experiments, or else they have geometrical properties which are definite and not arbitrary.<sup>88</sup>

Russell still maintains that the space-constant lies within certain more or less definable limits: 1st, a billiards game is only possible if the volume of the universe exceeds that of a billiard ball; 2nd, a three-sided figure inscribed in the vertices of a Euclidean triangle has an area less than that of the triangle. These considerations, it seems to me, deal not with the curvature of space, nor with any intrinsic metric for space, but rather with common-sense considerations.

which would hold in Euclidean and non-Euclidean universes alike.

Russell also considers Poincaré's contention that there are no properties which belong to the Euclidean straight line but not to the non-Euclidean straight line. Russell fears that Poincaré would then hold the two to be indiscernible; Russell claims that the difference between Euclidean and non-Euclidean straight lines is analogous to the difference between red and blue. If we object that red and blue have different wave-lengths, Russell replies that the Euclidean and non-Euclidean straight lines might have different properties.

<sup>89</sup> Russell suggests "length" to be such a property, yet this, I maintain, does not answer the wave-length objection; the wave-lengths of colors are their defining characteristics, while the lengths of lines are not. Russell appears to me to be claiming that there is a difference between the lines in various geometries; Poincaré denies this: lines are lines, or rather, in any one system lines are undefined terms whose behavior is specified by certain axioms. Finally, Russell claims that all experiments in Dynamics hinge upon Euclid being either true or false, and that, experiments with bodies being possible, a large number of such experiments will serve to place limits upon the space-constant. This, however, is not the stand Russell began with; furthermore, experiments with bodies, such as those performed in Dynamics, would still

provide information to the observer when performed in non-Euclidean spaces. That Russell has shifted his position can be seen in his conclusion:

Though we can never prove experimentally that space is strictly Euclidean, no theoretical limit can be assigned to the degree of approximation which is possible towards such a proof.<sup>90</sup>

In the third and final section of this article Russell attempts to answer Poincaré's request for definitions of distance and of straight line which do not appeal to Euclidean geometry. This section digresses from the topic of the empirical content of geometry, instead discussing what it means to define a term. This section anticipates sections of Russell's Principles of Mathematics, and provides an interesting insight into the development of this train of Russell's thought.

A mathematical definition consists of any relation to some specified concept which is possessed only by the object or objects defined... Philosophically, a term is defined when we are told its meaning, and its meaning cannot consist of relations to other terms.<sup>91</sup>

Again, Russell talks of the impossibility of the definitions which Poincaré has requested:

He will perhaps be surprised at my replying that such a request ought not to be made, since whatever is fundamental must be indefinable.

That some terms must be indefinable is evident after a moment's reflection. For terms are defined by means of other terms, and to attempt a definition of all terms must therefore involve a vicious circle.<sup>92</sup>

### H. Poincaré's Reply

The above article prompted a brief reply by Poincaré, "Sur les principes de la Géométrie, réponse à M. Russell." After opening plaudits to Russell's intentions, Poincaré immediately voices the following objections:<sup>93</sup>

1st, That he has never accepted the reasoning of Newton in favor of absolute motion;

2nd, That he considers the axiom of dimensions, as well as the other axioms of Euclid, as conventional;

3rd, That he rejects the first two of Russell's theses while accepting the third, namely that all geometries are neither true nor false.

Poincaré's paper is divided into considerations of the definition of distance, of empiricism in Geometry, of the question of absolute distance, and of bodies and space.

Only one page of this article is given over to our topic, empiricism and geometry. Poincaré's position remains the same, that the only experiences open to investigation are those dealing with the relations among bodies in space, and not with the actual nature of that space itself, and that if we abandon Euclidean geometry Dynamics is still possible, but with minor changes.<sup>94</sup>

## I. Science and Hypothesis

The Introduction to Poincaré's Science and Hypothesis,<sup>95</sup> as well as his chapter dealing with "Space", present in a final and polished form the views which he had defended from the beginning of this exchange.<sup>96</sup> Poincaré writes in his preface (italics mine):

Space is another framework which we impose upon the world. Whence are the first principles of geometry derived? Are they imposed upon us by logic? Lobatchevsky, by inventing non-Euclidean geometries, has shown us that this is not the case. Is space revealed to us by our senses? No, for the space revealed to us by our senses is absolutely different from the space of geometry. Is geometry derived from experience? Careful discussion will give the answer - no! We therefore conclude that the principles of geometry are only conventions; but these conventions are not arbitrary, and if transported into another world (which I shall call the non-Euclidean world, and which I shall endeavor to describe), we shall find ourselves compelled to adopt more of them.<sup>96</sup>

(I would like to point out that, rather than adopting more conventions in a non-Euclidean world, we could drop the rigid-rod convention and adopt a new system of co-ordinative definitions.)

Russell reviewed Science and Hypothesis several times; one of his better reviews, aimed at the philosopher rather than the general public, can be found in Mind, July, 1905.<sup>97</sup> In this review Russell takes a slightly different tack in his criticism of Poincaré's conventionalism. First, Russell attacks Poincaré's position, rather than merely defending his own: Russell argues that it may not always be the case

that Euclidean geometry will be the most convenient. This, however, does not serve to show that geometry is empirical, nor does it show that geometry is not conventional; rather it shows, as Poincaré would concede, that possibly someday a non-Euclidean choice would be made on the grounds of convenience. Second, Russell argues that there are ordering relations in every space which would yield the following situations: points adjudged near to one another under one ordering relation would, under another ordering relation, appear widely separated. Russell claims that only one of these orderings is immediately perceived by us, or rather, that at one and the same time no one sees both cases; he then claims that matter is arranged by perception in a spatial order, and on this ground claims that "this suffices to prove that geometry is not wholly conventional, as M. Poincaré contends."<sup>98</sup> Poincaré's contention, if I understand it correctly, is that perception does play a part and must be accounted for in a choice of spatial representations; Poincaré's claim, that the choice of geometries is wholly conventional, has not then been tarnished by this appeal to the psychological aspect of the perception of spatial arrangements. (However, at this stage in his career I doubt that Russell accepted Poincaré's distinction between perceived and geometrical space.)

### J. Russell's Later Views

Fortunately several written accounts have been left by Russell which serve to reveal how he regarded the topic of an empirical content to geometry through the years.

In his Autobiography Russell states that the Essay on the Foundations of Geometry was "my first experience of serious original work...when my dissertation was finished, I fully believed that I had solved all philosophical questions connected with the foundations of geometry."<sup>99</sup>

His youthful confidence remained undimmed well into the twentieth century, and Russell, although he felt he had lost some battles to Poincaré, still considered that he had won the war. He writes in a letter to Meinong, Feb. 5, 1907:

With what you write concerning non-Euclidean Geometry I am unfortunately not in sympathy. My own opinions I have often defended in the Revue de Métaphysique et de Morale against Poincaré; also in the Principles of Mathematics, Part VI, and briefly in Mind, July 1905, pp. 414-5...As pure mathematics all geometries are equally true; they assert merely what follows from certain premises - they are all equally hypothetical. But there is also one space that exists... Whether this space is an example of Euclidean or non-Euclidean Geometry can, in my opinion, only be decided empirically. That two parallels cannot intersect is indubitable; but it has to be asked whether the real world admits of parallels or not.<sup>100</sup>

Finally, in My Philosophical Development, Russell reminices about this period of his career, calling his Kantianism "somewhat foolish"; here he also admits that "the geometry in Einstein's General Theory of Relativity is such as I had declared to be impossible."<sup>101</sup>

## CHAPTER II

### PHILOSOPHICAL CONSEQUENCES OF THE RUSSELL - POINCARÉ DEBATE

#### A. The Axioms of Pure Geometry: Conventional versus Empirical Choice

Throughout his exchange with Russell, Poincaré correctly maintained the impropriety of the question "Which is the true Geometry?", clearly the axioms of pure geometry are not open to empirical investigation, as Russell himself came to realize towards the conclusion of the debate. As Russell finally admitted, it can never be proven that this space is Euclidean; experimental evidence which appears to confirm the Euclidean hypothesis actually confirms the conditional statement that if such and such a physical object is co-ordinated with the mathematical concept of "straight line" then Euclidean results obtain. While evidence might appear which would force us either to reject certain of the laws of physics or our belief in the Euclidean nature of our space, evidence could never be adduced for the claim that this space is strictly Euclidean. What could serve as such evidence? Not the existence of "parallel lines" in our visual field, for what seems parallel might intersect far out in space; not the case that, provided we travel in a straight line constantly we should never return to our starting place, since we could not ascertain whether space was infinite, whether we had not travelled in a straight line long enough.

or even whether we had indeed been travelling in a straight line. What few deem important to mention, however, is that the shoe can be put on the other foot: at this stage in geometry, before co-ordinative definitions have been introduced, it also cannot be adduced whether or not our space is non-Euclidean. As Poincaré correctly maintained, it is not the case that it is true that our space is Euclidean, but it is also not the case that our space is non-Euclidean; rather, the laws of Euclid, as well as the axioms of other geometries, are in themselves neither true nor false. If we so choose, we may interpret our space within a self-consistent set of Euclidean ordering relations, but this does not preclude an equally consistent interpretation under the application of certain non-Euclidean systems. That geometry is neither true nor false does not imply that we can choose any geometry; however, we are allowed to work with any self-consistent interpretation when we are forming co-ordinative definitions. As D. M. Y. Sommerville has summarized the outcome of this exchange:

(Facts) can still be described on the non-Euclidean hypothesis, with suitable modifications of the physical laws. To ask which is the true geometry is then just as unmeaning as to ask whether the old or the new metric system is the true one.<sup>102</sup>

Certain limits to conventionalism should be noted at this point. If a choice is made in favor of a non-Euclidean interpretation of our space, appropriate co-ordinative definitions must be found; it is an empirical matter whether or

not the co-ordinative definitions which will make this interpretation a workable one can be found.

Prior to Poincaré's statement of his doctrine of the conventional content of pure geometry, several attempts had been made by notable mathematicians and scientists to determine empirically a correspondence between a particular system of geometry and Space. Ernst Cassirer notes that Lobatchevsky, the inventor of one of the alternative geometries,

(had) used a triangle E<sub>1</sub> E<sub>2</sub> S, whose base E<sub>1</sub> E<sub>2</sub> was formed by the diameter of the orbit of the earth and whose apex S was formed by Sirius, and believed that he could, in this way, prove empirically a possible constant curvature of our space.<sup>103</sup>

Poincaré correctly pointed out, however, that were a non-Euclidean parallax supposedly to be discovered, Euclideanism could be saved via an alteration in the laws of optics; rather than ascribe to space a curvature other than zero, Poincaré noted that we might consider light rays to be curved. No experiment can tell us anything about ideal structures, since no measurement is concerned with space itself but only with the empirically given physical objects in space. Cassirer thus endorses Poincaré:

The propositions of geometry are therefore neither to be confirmed nor refuted by experience... For granted, that some experiment could show us a variation in the sums of angles of certain very great triangles, then the conceptual representation of this fact would never need to consist in, and methodologically could not consist in, changing the axioms of geometry, but rather in changing certain hypotheses concerning physical things. What we would have experienced, in fact, would not be another structure of space, but a new law of optics.<sup>104</sup>

The work of the nineteenth century geometers, in their development of non-Euclidean systems of geometry, removed from the realm of the synthetic a priori the assertion that the structure of our space is properly described as Euclidean. Included by Kant among those principles of knowledge which were necessary but non-empty were the laws of Euclidean geometry, the principle of causality, and the postulate of absolute time; the work of Einstein and others has shown that the first of these principles of knowledge, which Kant had regarded as a priori, is a posteriori, and verifiable through experience only in the restricted sense of an empirical hypothesis (tested after suitable co-ordinative definitions have been specified.)<sup>105</sup>

Since physical theory does not deal with the character of space, but rather with the properties of physical objects in space, the question arises: are the objects of geometry in some sense copies of reality? On the one hand, the forecasts which result from physically interpreting geometric systems are empirically verifiable; Cassirer notes

Whether they (the axioms of geometry) fulfill their task as moments of empirical knowledge can be decided always only in the indicated indirect way: by using them as building-stones in a theoretical and constructive system, and then comparing the consequences, which follow from the latter, with the results of observation and measurement.<sup>106</sup>

From this one might be led to the conclusion that the objects of geometry ensure the possibility of such forecasts by being

copies of reality. Such an inference, however, would be unwarranted. As Max Born noted,

The fact that Euclidean geometry hitherto was placed above physics was due to the fact that there are light rays which behave with very great accuracy like the straight lines of the conceptual scheme of Euclidean geometry, and that there are approximately rigid bodies which satisfy with considerable accuracy the Euclidean axioms of congruence... The objects of geometry which are actually applied to the world of things are thus these things themselves regarded from a definite point of view.<sup>107</sup>

Thus Russell's original position, once turned "inside-out", has found a home in contemporary physics; where Russell held that the axioms of geometry were open to empirical investigation in themselves, the contemporary physicist now holds that the correlations between the results of axiom-choices and experiential data are open to empirical verification. Poincaré's conventionalism receives its validity precisely from the fact that the objects of geometry are not copies of the parts of space, but instead must be physically interpreted; Cassirer writes that

It is thus vain to expect instructions about the "essence" of space from a procedure which, according to its whole tendency and disposition, is directed upon entirely different questions. Since the objects with which experience deals with are of an entirely different sort from the objects of which the assertions of geometry hold, since the investigation of material things never directly touches the ideal circle or straight line, we never gain in this way a decision among the different systems of geometry.<sup>108</sup>

As Russell claimed in his Essay on the Foundations of Geometry that "externality" is necessary to any perception of external

relations, so Cassirer claims that the true a priori of space concerns not any particular spatial structure, but "spatiability", that is, the logic of spatial relations. Since the objects of geometry are not correlates of real objects, the truth of geometric systems "differs" from the truth of empirical claims; Cassirer writes that

The structures of geometry, whether Euclidean or non-Euclidean, possess no immediate correlate in the world of existence. They exist as little physically in things as they do psychically in our "presentations" but all their "being", i.e., their validity and truth, consists in their ideal meaning. (Therefore) there can be no copy or correlate in the world of sensation and presentation for what the points, the straight lines, and the planes of pure geometry signify.<sup>102</sup>

What can Cassirer mean here when he speaks of an "ideal meaning" for the objects of geometry? First, he explicitly mentions the fact that he is considering pure geometry. Now in pure geometry, as in any branch of mathematics, there are certain undefined terms. In some axiomatizations of Euclidean geometry the undefined terms are "point", "line", and "plane", and the undefined relation is "intersects". In this context the word "point" is a symbol to be manipulated within a system of rules or "axioms", and as such pretends to have neither a physical nor a psychological import. On the other hand, Cassirer's remarks are inapplicable when applied geometry is under consideration. In that case (and I will try to explain this point for the rest of the paper), certain choices are made as to the meanings of the previously

undefined terms of our geometry. These conventionally assigned meanings have been called "co-ordinative definitions" by Hans Reichenbach. This term is appropriate, for in order to move from pure geometry (wherein as Cassirer notes the undefined terms possess no physical import) to applied geometry (wherein we attempt to solve geometric problems couched in terms of rays of light, or rows of flowers, or what have you) we must co-ordinate specific physical objects with various hitherto abstract mathematical terms. Thus we may wish to consider "straight line" to mean "ray of light". Conventionality thus enters into our choices of both the axioms of pure and applied geometry. With regard to pure geometry, we may conventionally choose one axiom system or another; in applying this pure system to a problem, that is, in moving to an applied geometry, we must conventionally determine which objects in our world will stand for which undefined terms in our axiom-system. In any case, it is externality or spatiality in general which necessarily grounds our perceptions, and not a particular geometric system. (See the concluding chapter of this thesis for a discussion of how the mathematical theory which is directly concerned with this general spatiality, while considered to be Projective Geometry by Russell when he wrote his Essay, may in fact be the new branch of geometry known as Topology.)

### B. Interpretation and the "Truth" of Geometric Systems

In The Principles of Mathematics Russell maintains that the axioms of geometry are only assumptions, and are not themselves asserted. What are asserted, he claims, are the inferences from the axioms to the theorems. The axioms of geometry would be primitive propositions if they were asserted; instead we have

if the axioms of Geometry G1 are true,

then the theorems of Geometry G1 are also true.

Russell writes:

The so-called axioms of Geometry...would be primitive propositions if, as in applied mathematics, they were themselves asserted; but so long as we only assert hypotheticals (ie., propositions of the form "A implies B") in which the supposed axioms appear as protasis, there is no reason to assert the protasis, nor, consequently, to admit genuine axioms. <sup>110</sup>

By "genuine" axioms, as against "so-called" axioms, Russell seems to mean true but unprovable assertions about the real world.

If axioms are nothing other than building-blocks, albeit of a highly abstract kind, in what sense can a geometry be said to embody "truth"? As a pure system of thought, any geometry must be self-consistent; furthermore, all theorems proposed by that geometry must be able to validly be derived from its axioms. Obviously, then, an applied geometry differs from the theoretical in that a further criterion arises; the empirical confirmation or falsification of claims resulting from the application of that applied geometry to a scientific

theory. 111

How does the transition occur from the theoretical to the applied examples? Through the introduction of the above mentioned co-ordinative definitions, the theoretical geometry is interpreted as an applied geometry. Consider the case in which we decide to work with Euclidean theorems for an experiment in optics which is to take place in our laboratory. We have already picked which system of theorems we would prefer to use in our work; and may now conventionally co-ordinate which physical objects will stand for "line", "point", and "plane". We may co-ordinate a light ray with the term "line", substituting the first term wherever the second appears in one of our Euclidean theorems. This interpretation may now be verified or disconfirmed. We have if we co-ordinate "light ray" with the term "line" then the axioms of the geometry with which we are working take on such and such an empirical meaning. While we may choose any geometry to begin with from the class of self-consistent geometries, often our choice will be that of the Euclidean system. In my opinion this is what Poincaré had in mind when he proclaimed that

In reality, space is therefore amorphous, a flaccid form, without rigidity, which is adaptable to everything; it has no properties of its own. To geometrize is to study the properties of our instruments, that is, of solid bodies... What we have said of space is applicable to time... The properties of time are merely those of our clocks just as the properties of space are merely those of our measuring instruments. 112

Various attempts were made during the nineteenth century to determine experimentally what the nature of our space is. Gauss carefully measured the angles formed by a triangle with its vertices on three mountain peaks; Lobatchevsky attempted the same experiment on an astronomical magnitude. Why would they do this? In the geometry developed by Lobatchevsky and Bolyai it is an axiom that more than one parallel line can be drawn through a point not on another line; in light of this the sum of the angles of a triangle is always less than 180 degrees, and the amount by which it is less is proportional to the size of the triangle. Again, in their geometry the ratio of the circumference of a circle to its diameter is always greater than  $\pi$ , and this ratio increases proportionally to the size of the circle considered.

In Riemann's geometry there are no parallel lines, the sum of the angles of a triangle is always greater than 180 degrees, and the ratio of a circle's circumference to its diameter is always less than  $\pi$ .<sup>13</sup> Poincaré notes (in The Value of Science) that such parallax experiments would not confirm or invalidate the thesis that our space is Euclidean; rather such experiments confirm or disconfirm various correlations between certain co-ordinative definitions and certain geometric interpretations of space. Suppose that we conventionally co-ordinate the concept "line" with the object "light ray", and then perform a parallax experiment. The results of that experiment will either be consistent with Euclidean

geometry or one of the non-Euclidean geometries described above, since the sum of the angles of the triangle in our experiment either will or will not equal 180 degrees. Suppose that the result was non-Euclidean. Poincaré notes<sup>114</sup> that we are now faced with the situation of retaining the conventionally chosen co-ordinative definition "light rays: line" and working with a non-Euclidean model of space in our physical calculations, or of conventionally deciding to modify the co-ordinative definition in order to work with a Euclidean system.

### C. The Ambiguous Criterion of "Simplicity"

As theoretical systems one geometry cannot be said to be more true than another, since, as we have seen, the axioms of the various geometries are neither true nor false. Are there, over and above this, methodological considerations for choosing one geometry over another? One criterion has often been proposed, namely that of "simplicity"; unfortunately, this criterion itself is open to several interpretations.

One type of appeal to simplicity was set forth both by Poincaré and by Cassirer, who, each for his own reasons, claimed that Euclidean geometry was simpler in structure than any other. Since this claim is wrong but often repeated, I will take a moment to investigate the source of this confusion.

"Simple in structure", when applied to pure geometry, sometimes centers around claims concerning the axiom of parallels, or lack thereof; when this label concerns applied geometry what is in question is usually the corresponding constant for the curvature of space. Thus in the first instance, Ernst Cassirer, while not yet converted to relativity theory, argued in Substance and Function that

Logical consistency, such as belongs to all these systems, is a merely negative condition, which they all share among themselves. But within the group thus established the differences in fundamental structure and in relative simplicity are not extinguished.<sup>115</sup>

First, what might be alluded to in this passage is the fact that in Euclidean geometry we are told that through a point not on a line there exists a unique parallel line to our initial line, while in some non-Euclidean geometries an infinite number of such parallels is postulated, and in other geometries no such parallels occur. From this, however, we must not conclude that the logical structures of these geometries differ in simplicity: each has its own unique axiom concerning the occurrence and behavior of parallel lines.

As pure mathematics, then, what other purely internal characteristics might serve to distinguish Euclidean geometry? Since Euclidean space is homogeneous and isotropic, the relations between the elements of a configuration are unaffected by that figure's orientation. However, in non-Euclidean geometries (such as the geometry which would occur

on the surface of a basketball), the qualitative (eg., non-metric) relations between the elements of a configuration would also remain invariant. Such was Russell's thesis in his Essay on the Foundations of Geometry, wherein he described all metric geometries as reducible to projective geometry.

As congruence geometries the various geometrical systems are classified according to their resulting space-constant, that is, according to the degree of curvature which occurs in their respective spaces. The constant of curvature under Euclidean geometry is zero; it is positive for hyperbolic geometries and negative for spherical and elliptic geometries.<sup>116</sup> This, again, is not an internal criterion of simplicity, for each geometry possesses a unique, though different, constant of curvature: there is no logical reason to prefer the case where this constant has the numerical value of zero as to the case, say, where it instead has the value of negative .357.

A third criterion of simplicity, proposed by Ernst Cassirer, comes from the fact that infinitesimal regions of non-Euclidean spaces behave as if they were Euclidean, just as a small arc of a circle can be thought of as a very short straight line. Cassirer claims:

This logical simplicity belonging to Euclidean space... is shown in the fact that we can make any 'given' space, that possesses any definite curvature, into Euclidean by regarding suf-

ficiently small fields from it from which the difference conditioned by the curvature disappears. Euclidean geometry shows itself herein as the real geometry of infinitely small areas.<sup>117</sup>

Cassirer's claim that Riemannian geometry is locally Euclidean is beyond reproach. Granting that point, I would first say in reply to Cassirer that it is one thing to work with a given area "as if" it were Euclidean, and it is quite another to conclude from this that in fact it (the whole space composed of these tiny regions) no longer is non-Euclidean; thus we can "make" a given space (that is, the entire given space) Euclidean only in a metaphorical sense. Of course, Cassirer means only that a non-Euclidean geometry may be considered to be locally Euclidean, yet what bars us from mathematically dealing with the local situation in non-Euclidean terms? While we may conventionally choose to work with small regions in terms of Euclidean geometry, we might also work with small regions in terms of one of the non-Euclidean geometries. Our inability to visualize such small regions is not an insuperable problem, since most people do not visualize large regions in non-Euclidean terms either. Furthermore, one model of a non-Euclidean geometry is actually a small region, namely the interior of a circle (since it is possible for two chords of a circle to intersect without either of them intersecting the diameter of the circle). I am not arguing that the

geometry of small regions is non-Euclidean, but rather am trying to counter the claim that the geometry of any region (large or small) must only be describable in terms of Euclidean geometry. Thus the claim that Euclidean geometry is the "real" geometry of infinitely small areas appears specious to me, since in considering infinitely small areas of a non-Euclidean space we may, rather than resulting in a Euclidean component, instead find ourselves working with a region whose constant of curvature is very close to but which does not equal zero! Since these cases could not be discerned, where Cassirer merely asserts that the "real" geometry of infinitely small regions is Euclidean I will merely assert that on odd-numbered days I will consider the "real" geometry of such regions to be hyperbolic, with a constant of curvature infinitely close to but not equalling zero, and on even-numbered days I will consider the "real" geometry to be spherical, with a negative constant of curvature close to but not equalling zero, reserving elliptic geometry for leap years (arguing my position on grounds that statements made without proof can be denied without proof):

Again, Poincaré proposes that Euclidean geometry

is, and will remain, the most convenient first, because it is the simplest, and it is not so only because of our mental habits or because of the kind of direct intuition that we have of Euclidean space; it is the simplest in itself, just as a polynomial of the first degree is simpler than a polynomial of the second degree.

Regarding Poincaré's claims concerning mental habits and perception, more will be said later. With regard to his claim that the degree of a polynomial defines its simplicity, hence Euclidean geometry (I suppose because it possesses a constant of curvature of zero) is simpler than all others, my remarks above, concerning first the space constants of various geometries and second the geometries of small regions, apply with full force.

Again, Poincaré argues in Science and Hypothesis:

We therefore conclude that the principles of geometry are only conventions; but these conventions are not arbitrary, and if transported into another world (which I shall call the non-Euclidean world, and which I shall endeavor to describe), we shall find ourselves compelled to adopt more of them.<sup>119</sup>

To this I would note that, in a non-Euclidean world (such as that described by Reichenbach on pages 10 to 14 of his Philosophy of Space and Time) we would be compelled to adopt more conventions (such as the principle of universal forces, etc.) only if we wish to retain our old co-ordinate definitions, specifically that of "rigid body". A more general argument, stemming from Poincaré's position, is proposed by Ernst Cassirer:

...among all possible self-consistent geometries the Euclidean possesses a certain advantage of 'simplicity' since it defines the minimum of those conditions under which experience is possible in general.<sup>120</sup>

I think that Russell refuted this 'option' in his Essay on the

Foundations of Geometry, wherein he notes that it is not any particular geometry which grounds our perceptions, but rather "externality" in general. Experience in a non-Euclidean world, I would add, if at all possible, would entail that there is no Euclidean pre-requisite for our experience. From this I would conclude that it is spurious to maintain any one geometry as presenting the minimum requisite conditions for human experience. Furthermore, what we should be concerned with is not the relative simplicity of a pure system of geometry, since such a criterion is ambiguous at best; we should instead focus upon the workability of different metric geometries when paired with various possible co-ordinative definitions.

I would like to note in passing that while attempts to describe one geometry as more simple than all others seem doomed, some geometers are not giving up. Karl Menger has proposed that

the recent development of hyperbolic geometry indicates that Euclidean geometry lacks even the distinction of logical simplicity... Hyperbolic geometry is the only one which can be developed from a few simple assumptions concerning "joining", "intersecting", and "continuity" alone.<sup>121</sup>

For my own part the considerations of the conventionalist thesis apply with equal force both to attempts to proclaim Euclidean geometry as "true" and to attempts to champion some other specific geometry.

### D. Grunbaum's Interpretation of Poincaré's Conventionalism

Adolf Grunbaum has charged both Hans Reichenbach and Albert Einstein with mis-interpreting the conventionalism of Henri Poincaré by their identification of it with Duhem's proposal of the interdependence of geometry and physics.<sup>122</sup>

Grunbaum classifies the claims of Duhem concerning the retention of Euclidean geometry in the face of any observational evidence as an epistemological problem; in turn, he characterizes Poincaré as claiming that, given new evidence, we can choose a different metric in order to save Euclidean geometry - this Grunbaum considers to be a linguistic problem. Thus Grunbaum writes that

Corresponding remarks apply to Poincaré's contention that we can always preserve Euclidean geometry in the face of any data obtained from stellar parallax measurements: if the paths of light rays are geodesics on the customary definition of congruence ... and if the paths of light rays are found parallelistically to sustain non-Euclidean relations on that metrization, then we need only choose a different definition of congruence such that these same paths will no longer be geodesics and that the geodesics of the newly chosen congruence are Euclideanally related. From the standpoint of synthetic geometry, the latter choice effects a renaming of optical and other paths and thus is merely a recasting of the same factual content in Euclidean language rather than a revision of the extralinguistic content of optical and other laws. The retainability of Euclideanism by remetrization, which is affirmed by Poincaré, therefore involves a merely linguistic interdependence of the geometric theory of rigid solids and the optical theory of light rays.<sup>23</sup>

Thus Grunbaum claims that Poincaré's conventionalism involves the change of semantic rules, specifically those concerning

the definition of congruence. Grunbaum writes:

Poincaré argued that the ascription of the congruence relation to non-coinciding pairs of intervals of physical space and to intervals of physical time is a matter of convention rather than of fact.<sup>124</sup>

On the other hand, Grunbaum claims that Duhem's conventionalism involves the retention of such semantic rules but the alteration of the factual content of auxiliary assumptions.<sup>125</sup>

Similarly, A.S.Eddington objected to Poincaré's conventionalism on the grounds that it was merely linguistic (and hence trivial), applying only to the uses of words.<sup>126</sup> In his recent writings, however, Grunbaum has claimed that Eddington's objections, although similar to his own, miss Poincaré's point. According to Grunbaum, while Poincaré's thesis of the conventionality of congruence was a claim concerning the structural properties of physical space and time, Eddington's objections were raised at the level of the semantic corollary of that thesis (which concerns the language of the description of such structural relations).<sup>127</sup>

The explanation of Poincaré's conventionalism attacked by Grunbaum was set forth by Hans Reichenbach; Reichenbach writes:

The view that every spatial and temporal metric presupposes co-ordinative definitions has been generally accepted and is known as conventionalism. For spatial measurements it originated with Helmholtz and Poincaré; Einstein extended it to temporal measurements (simultaneity and uniformity).<sup>128</sup>

Against this Grunbaum writes that

The central theme of Poincaré's so-called conventionalism is essentially an elaboration of the thesis of alternative metrizability whose fundamental justification we owe to Riemann, and not the radical conventionalism attributed to him by Reichenbach.<sup>129</sup>

Grunbaum's position has enjoyed a famous following:

Rudolf Carnap wrote that:

I am happy to find that Grunbaum clarified Poincaré's conception of geometry, thus countering the widespread misunderstanding that Poincaré championed a pure conventionalism. Grunbaum shows on the basis of reference and quotations... that Poincaré, while emphasizing the importance of conventions, quite clearly upholds an empiricist position in regard to physical "geometry".<sup>130</sup>

What does Carnap mean here by "pure conventionalism"? Certainly Poincaré felt that, with regard to theoretical geometry, we are free to choose any self-consistent system for use in our models. Again, with regard to applied geometry I do not think that Poincaré was an "empiricist"; rather, as I have noted previously, Poincaré argued that we may conventionally co-ordinate the undefined terms of our geometric system with physical objects in our experiments. Then and only then, Poincaré argues (and I agree), does empirical confirmation enter the picture. Again, as I have noted previously with regard to parallax experiments, when the results of experimentation are in we may yet decide to co-ordinate conventionally other objects with our mathematical terms in order to work with a specific geometric system.

Only after co-ordinative definitions have been introduced can the empiricist position alluded to by Carnap take effect, and this is the view of Reichenbach:

Another confusion must be ascribed to the theory of conventionalism, which goes back to Poincaré. According to this theory, geometry is a matter of convention, and no empirical meaning can be assigned to a statement about the geometry of physical space. Now it is true that physical space can be described by both a Euclidean and a non-Euclidean geometry, but it is an erroneous interpretation of this relativity of geometry to call a statement about the geometrical structure of physical space meaningless. The choice of a geometry is arbitrary only so long as no definition of congruence is specified. Once this definition is set up, it becomes an empirical question which geometry holds for physical space.<sup>131</sup>

Reichenbach claims further that:

The conventionalist overlooks the fact that only the incomplete statement of geometry, in which reference to the definition of congruence is omitted, is arbitrary; if the statement is made complete by the addition of a reference to the definition of congruence, it becomes empirically verifiable and thus has physical content... Instead of speaking of conventionalism, we should speak of the relativity of geometry.<sup>132</sup>

Einstein's theory of relativity, Reichenbach notes, rests partly upon the work of Poincaré:

The logical basis of the theory of relativity is the discovery that many statements, which were regarded as capable of demonstrable truth or falsity, are mere definitions.<sup>133</sup>

In the above passage I think Reichenbach has correctly struck upon the essence of Poincaré's conventionalism. As Reichenbach notes, these remarks apply not only to the first axioms of geometry, but also to the axioms by which we conduct

measurements.<sup>134</sup> Thus what imparts an empirical content to geometry, in the sense that the outcome of the application of the axiom-systems is empirically verifiable, are the co-ordinative definitions employed. Reichenbach states:

The relativity of geometry is a consequence of the fact that different geometries can be represented on one another by a one-to-one correspondence. For certain geometrical systems, however, the representation will not be continuous throughout, and there will be singularities in individual points or lines. For instance, a sphere cannot be projected on a plane without a singularity in at least one point.<sup>135</sup>

How similar Reichenbach's claims are to the passage from the work of L. Rowziier which Grunbaum cites in support of his own, supposedly more correct, reading of Poincaré's conventionalism:

The conventions fix the language of science which can be indefinitely varied: once these conventions are accepted, the facts expressed by science necessarily are either true or false... Other conventions remain possible, leading to other modes of expressing oneself; but the truth, thus diversely translated, remains the same. One can pass from one system of conventions to another, from one language to another, by means of an appropriate dictionary. The very possibility of a translation shows here the existence of an invariant. Conventions relate to the variable language of science, not to the invariant reality which they express.<sup>136</sup>

In any case, Albert Einstein's philosophy of science includes an epistemological conventionalism, which he claims is due in part to the influence of Henri Poincaré. Einstein describes Poincaré's epistemological conventionalism thus:

Geometry ( $G$ ) predicts nothing about the relations of real things, but only geometry together with purport ( $P$ ) of physical laws can do so. Using symbols, we may say that only the sum of ( $G$ ) plus ( $P$ ) is subject to the control of experience. Thus ( $G$ ) may be chosen arbitrarily, and also parts of ( $P$ ); all these laws are conventions. All that is necessary is to choose the remainder of ( $P$ ) so that ( $G$ ) and the whole of ( $P$ ) are in accord with experience. Envisaged in this way, axiomatic geometry and the part of natural law which has been given in a conventional status appear as epistemologically equivalent.<sup>137</sup>

Again, E.A.Milne's interpretation of Poincaré's conventionalism agrees with Rouzier's interpretation; both read Poincaré as holding that (what Reichenbach later called) co-ordinative definitions are open to conventional choice and then, once chosen, yield statements which are empirically verified or falsified. In comparing Helmholtz's conventionalism with that of Poincaré, Milne writes that:

His (Helmholtz's) conclusions were in line with later views of Poincaré, namely that the axioms of geometry, including the axioms of non-Euclidean geometries, are compatible with any geometric content whatever, but that, as soon as a framework has to be found for the principles of mechanics we obtain a system of propositions which has real import, capable of verification or disproof by empirical observation.<sup>138</sup>

While Helmholtz had proposed what he considered to be physiological evidence that our space is Euclidean, with respect to the axioms of geometry Helmholtz clearly believed that empirical confirmation was possible only after something like Reichenbach's co-ordinative definitions had been established. Helmholtz concludes a lecture given at Heidelberg:

*berg in 1870 with:*

First, the axioms of geometry, taken by themselves out of all connection with mechanical propositions, represent no relations of real things... Second, as soon as certain principles of mechanics are conjoined with the axioms of geometry, we obtain a system of propositions which has real import, and which can be verified or overturned by empirical observations.<sup>139</sup>

Summary: Regarding Grunbaum's distinction between the "linguistic" and the "epistemological" interdependence of physics and geometry, I take it he has in mind the following: upon receiving certain empirical evidence, if Poincaré did not wish to relinquish Euclidean models for space, then he must say that light rays really are not straight; against this would be Duhem's position, as Grunbaum sets it forth,<sup>140</sup> that decisive falsifiability or crucial verifiability never comes about due to the inextricable position an empirical hypothesis maintains with respect to the entire network of physical hypotheses. Reichenbach, however, correctly points out that Poincaré's conventionalism applies to the choice among axiom-systems of geometry before a co-ordinative definition has been chosen; it is a matter of empirical verification (we "sit back and wait") whether or not our conventional choice of an axiom-system, coupled with ours conventional choice of a co-ordinative definition, will produce contradictions in our physical theories; if the latter occurs, we are free to adopt either a new co-ordinative definition,

or a new axiom-system, or both. My conclusion is that it is only after the fact that Grunbaum is able to claim that Poincaré's position in the Russell-Poincaré exchange in some manner included a trivial or totally non-empirical brand of conventionalism.

### E. Russell's Gradual Conversion

An account of the Russell-Poincaré exchange would not be complete without recounting Russell's gradual disengagement from the position which he had tendered against the claims of Poincaré. Instructive are four articles written by Russell while the debate was in progress, as well as an article on non-Euclidean geometry which was first printed in the Encyclopaedia Britannica in 1902 by Russell, and which was reprinted in 1910 after several passages from the original article had been deleted! By means of these sources, as well as a letter from Russell to Meinong, I think that the date of Russell's conversion can safely be located as sometime in 1908, or, at the latest, in 1909.

The Russell-Poincaré debate, it must be understood, was not an isolated phenomenon during the earliest stage of Russell's philosophic career; rather, it was part of a flurry of replies which Russell was forced to present in defense of various positions he had proposed in his dissertation essay. In January of 1896 Russell published "The Logic of

<sup>top</sup>  
Geometry";<sup>141</sup> in it he directly considered the problem of congruence, asking first how we obtain equality among solids, and second how we can measure space using congruence rules when such rules do not strictly apply to the measure of time.

His conclusion in this article closely resembles that proposed in "The A Priori in Geometry", an article which Russell published shortly thereafter.<sup>142</sup> Russell's solution to the problem of congruence rests upon three axioms: the axiom of free mobility; the generalized axiom of dimensions; and the generalized axiom of the straight line. In turn, the truth of these axioms involves the homogeneity of space and the complete relativity of position. Russell concludes that:

The remaining axioms required to define Euclidean space will remain, for Geometry, empirical, since no geometrical principle and no possibility of experience of an outer world can prove them to be necessary.<sup>143</sup>

Russell here agrees with Kant that some form of externality is necessary for experience, but denies that a Euclidean externality is an a priori prerequisite for experience.

So far with Kant. But Kant extended this a priori necessity to the whole of Euclidean space, and in this, I think, Metageometry proves that he went too far. For all that his argument proves is, that the properties which must belong to any form of externality are a priori. Now Euclidean Space, as we have seen, has properties which are not necessary to any form of externality, and are not shared by non-Euclidean spaces.<sup>144</sup>

In these articles the influence of Kant and Hegel, which Russell had received from his Cambridge mentors, may still

be seen; by July of 1901, however, in his article "Is Position in Time and Space Absolute or Relative?", Russell's revolt against Bradley can be seen to be in full swing:

We have all been taught to believe that time and space consist wholly of relations, and that moments and points are mathematical fictions. It is this opinion that I wish to challenge.<sup>145</sup>

Russell's 1902 article in the Encyclopaedia Britannica contains, in a sub-section entitled "Criticism of Riemann", an argument that space must have a constant curvature:

If we are to be able, without a vicious circle, to presuppose co-ordinates in discussing distance, it is evident that our co-ordinates must not presuppose distance... Moreover, if our co-ordinates are to represent any kind of spatial magnitudes, we must assume the possibility of equal quantities in different places, and hence, it will be found, we shall be compelled to regard the measure of curvature as constant... (If the constant of curvature were to vary) a metric co-ordinate system would become impossible. Moreover, Geometry would become akin to Geography; it would not consist of general theorems, but of descriptions of various localities.<sup>146</sup>

In the 1910 rewrite of this article the section entitled "Criticism of Riemann" is deleted, and Russell writes in its place:

If  $a$  (the constant of curvature) be positive, space is finite, though still unbounded, and every straight line is closed - a possibility first recognized by Riemann. It is pointed out that, since the possible values of  $a$  form a continuous series, observations cannot prove that our space is strictly Euclidean. It is also regarded as possible that, in the infinitesimal, the measure of curvature of our space should be variable.<sup>147</sup>

Russell also included a sub-section, entitled "rigid bodies

"unnecessary in Geometry", in the 1902 article, but deleted this section from the 1910 rewrite. In this section Russell argues that:

The whole confusion appears to be due to not distinguishing between the process of measurement, which is of purely practical interest, and the meaning of equality, which is essential to all metrical Geometry.<sup>148</sup>

The influence of Poincaré's arguments against Russell is evident in the 1902 article, where Russell's remarks are very close to those of his former adversary. As Poincaré had claimed that we can either change our initial geometry or our physical laws, were an astronomical experiment not to yield anticipated Euclidean results, Russell now writes that

astronomical distances and triangles can only be measured by means of the received laws of astronomy and optics, all of which have been established by assuming the truth of the Euclidean hypothesis.<sup>149</sup>

Russell also explicitly gives up any hope of empirically investigating the axioms of geometry:

Finally, it is of interest to note that, though it is theoretically possible to prove, by scientific methods, that our space is non-Euclidean, it is wholly impossible to prove by such methods that it is accurately Euclidean... A triangle might be found whose angles were certainly greater, or certainly less, than two right angles; but to prove them exactly equal to two right angles must always be beyond our powers. If, therefore, any man cherishes a hope of proving the exact truth of Euclid, such a hope must be based, not upon scientific, but upon philosophic considerations.<sup>150</sup>

He still holds but hope, however, for the investigation of

our space, at least to the extent that empirical evidence will force us to choose between keeping our geometry or our physical laws.<sup>151</sup> These passages are repeated, almost word for word, in the 1910 article.<sup>152</sup>

Also included in the first but missing from the latter article is a section entitled "Supposed a priori grounds for Euclidean space", in which Russell notes that the mere construction of non-Euclidean geometries does not counter Kant's claims concerning geometry and Space; rather, Russell claims, one of three tactics must be adopted:

(To claim) (1) We have an intuition of non-Euclidean spaces; (2) in the sense in which Kant uses the word, we have no intuition of Euclidean space; (3) our intuitions are irrelevant in a logical inquiry concerning space. No one of these three propositions follows from non-Euclidean geometry alone... Both the second and the third seem to be capable of proof, but their discussion, which properly belongs to the criticism of Kant, would carry us too far from our subject to be attempted here.<sup>153</sup>

While Russell rewrote some of the first article in a light more favorable to non-Euclidean geometry, he still felt that there was only one space, hence one "true" geometry. As noted earlier, Russell voiced this view in a letter to Meinong as late as 1907.<sup>154</sup>

The first and only place where I could find an explicit endorsement of Poincaré's work (the 1902 Encyclopædia Britannica article does not mention Poincaré by name) comes in Russell's signed preface to the 1924 publication

of Jean Nicod's Geometry and Induction. Russell begins his remarks by noting that the problem which Nicod addresses himself to, that of the relation between geometry and sense-perception, is of importance both with regard to the work of Kant and of Whitehead. Russell writes:

The history of this problem in modern times is well known. Kant asserted that geometry is based on an a priori intuition of space and that experience could never contradict it because space constitutes a part of our manner of perceiving the world. Non-Euclidean geometry has led most thinkers to abandon this opinion; although from the logical point of view it might be easy to maintain that Lobatchevsky's work did not conflict with Kant's philosophy.<sup>155</sup>

Russell then mentions Poincaré's work as one possible solution to the problem of the relation of geometry to experience:

...for example, it was the viewpoint assumed by Henri Poincaré, who maintained that Euclidean geometry is neither true nor false, but that it is simply a convention. In a certain sense, this point of view may still be possible: in all experiment or physical observation, it is the group of applicable physical laws which constitutes the object of study, and if the results do not correspond to our expectation, we have a certain choice as to which of these laws should be modified.<sup>156</sup>

Russell does not completely endorse Poincaré's work, however, for he notes that Nicod's thesis presents "a different criticism, more fundamental than the theory of Henri Poincaré."<sup>157</sup>

v. The Consequences of Conventionalism for Philosophical Cosmology

One interesting consequence of conventionalism concerns the constant of curvature for space. Where cosmologists had previously assumed that the curvature of space is of a constant value, theorists conscious of the impact of conventionalism have proposed models of the universe wherein either the constant of curvature for space changes with time (so that there is a constant  $k_1$  attributed to all of space at a time  $t_1$ , and another constant  $k_2$  at time  $t_2$ , etc.), or where the curvature for space at any one time is not uniform (ie., the case where a space is not homogeneous). Thus Ernst Cassirer advanced the interesting proposal that the geometry of our space may not be uniform. After Cassirer realized the importance of Einstein's theories, he wrote that:

One can no longer speak of an immutably given geometry of measurement which holds once and for all for the whole world. Since the relations of measurements of space are determined by the gravitational potential, and since this is to be regarded as in general changeable from place to place, we cannot avoid the conclusion that there is in general no unitary 'geometry' for the totality of space and reality, but that, according to the specific properties of the field of gravitation at different places, there must be found different forms of geometrical structure.<sup>158</sup>

Due to the revolution in thought wrought by conventionalism, Cassirer claims that the "problem of space" has passed from

the realm of ontology, or of asking "What is space?", to the realm of epistemology, or of asking "What do we know about space and how do we know this?". Cassirer writes:

The relativity of places involves that of geometrical truth. And yet this view is, on the other hand, only the sharpest expression of the fact that the problem of space has lost all ontological meaning in the theory of relativity... We are no longer concerned with what space 'is' and with whether any definite character, whether Euclidean, Lobatchevskian, or Riemannian, is to be ascribed to it, but rather with what use is to be made of the different systems of geometrical presuppositions in the interpretation of the phenomena of nature and their dependencies according to law.<sup>159</sup>

Russell, in his exchange with Poincaré, of course was concerned with "ontological" questions; specifically, Russell attempted to come to grips with the question "Is space really curved?". Poincaré's counter-claim, that geometry (as much so as number theory) is founded upon conventionally chosen interpretations of basic axioms and relations, appears to me to have influenced Russell's later philosophy of mathematics. In his Essay on the Foundations of Geometry, however, Russell embraced the neo-Kantian view that, if measurements in space are to be at all possible, the constant of curvature must a priori be known to be invariant. H.P. Robertson discusses Russell's earliest position on this matter:

On this modified Kantian view, which has been expounded at length by Russell, it is inconceivable that  $k$  might vary from point to

point - for according to this view the very possibility of measurement depends upon the constancy of the space-structure as guaranteed by the axiom of free mobility.<sup>160</sup>

This position of Russell stemmed in part from his reaction to the work of A. Calinon; in 1889 Calinon had proposed the possibility that the space-constant  $k$  might vary with time.<sup>161</sup>

Cassirer, after having been influenced by Einstein's theories, argued that  $k$  might vary with place, according to the presence and magnitude of gravitational fields.

#### G. Some Objections to Conventionalism Considered

Two objections to conventionalism, both stemming from considerations of our everyday experience, will now be considered. First, some neo-Kantians may claim that, while geometries are relative, all of our sense-experience is Euclidean; thus they object that Euclidean geometry in some sense retains its claim to be the one geometry corresponding to our space. H.J. Paton writes:

...one of the pure geometries does apply to the physical world, and it seems to be assumed that one of them must so apply: we appeal to experience only to discover which. If one of many geometries does apply, and still more if one must apply, to the physical world, we have Kant's problem before us in a more subtle form.<sup>162</sup>

Paton also tells us that part of the a priori character of our ideas of space and time is that "we can determine, independently of experience, the spatial and temporal conditions to which all objects of experience must conform."<sup>163</sup>

Here, then, is the germ of the second objection as I understand it: that not only have all of our past experiences been Euclidean, but that all of our future experiences will continue to be Euclidean, hence (idle talk of conventionalists to the contrary) we may believe on the basis of a uniformity of all possible and actual sense-experience that the structure of space is in fact Euclidean.

First, our sense-experience is not acute enough to determine that the geometry of our space, or even of a limited region of our space (such as this planetary system), is strictly Euclidean. It may be the case that we never experience two Euclidean parallel lines (the lines formed by the top and bottom of a picture frame may intersect 100,000 miles away in space); this would neither prove nor disprove the claim that the structure of our space corresponds to, say, Euclidean geometry. If our space were Euclidean, it might be that parallel lines simply never occur; that is, that physical examples do not exist; a strict Newtonian might suggest that potential parallel lines do pervade that space, however, via the eternally existent relations among the points of space. Furthermore, if our sense-experience is only more or less Euclidean, those factors which make for little if any noticeable difference for the calculations of terrestrial physics may show up on an astronomical magnitude. Third, we are not on a vendetta against those who be-

ieve that Euclidean geometry adequately describes our world;

however, Hans Reichenbach has argued that although it is

possible to use Euclidean geometry to adequately describe

every limited region of space, that space as a totality would

still fall prey to non-Euclidean interpretations (much as a

circle, although more or less described in terms of a mil-

lion-sided figure, is none the less a circle).<sup>164</sup>

Fourth,

Reichenbach argues that if we hold on a priori grounds that

our space is Euclidean, we may be forced to contradict

another a priori principle: that of uniform (ie., normal)

causality.

If the principle of normal causality, ie., a con-

tinuous spreading from cause to effect in a finite

time, or action by contact, is set up as a neces-

sary prerequisite of the description of nature,

certain worlds cannot be interpreted by certain

geometries. It may well happen that the geometry

thus excluded is the Euclidean one; if Einstein's

hypothesis of a closed universe is correct, a

Euclidean description of the universe would be

excluded for all adherents of a normal causality.<sup>165</sup>

Fifth, it may be that our experience of the world as Euclidean

is a deep-rooted habit, liable to change in the future, as

our experience becomes broader. Against those neo-Kantians

that would claim that we can only (or have only) visualized

Euclidean spatial relations, we must consider how we can ever

visualize non-Euclidean relations in the manner of ease and

frequency of occurrence with which we presently visualize

Euclidean relations. As noted first by Helmholtz (and here

posit by Reichenbach), force of habit may allow us to

### visualize non-Euclidean relations.

To imagine geometrical relations visually means to imagine the experiences which we would have if we lived in a world where those relations hold ... The philosopher has committed the mistake of regarding as a vision of ideas, or as laws of reason, what is actually the product of habit.

In short, since our experiences have been more or less Euclidean, so have been our visualizations of spatial relations; were our experiences to change radically, so would our visualizations have to change if we were to function in such a drastically altered environment.

### H. To What Extent Does Empirical Investigation Enter In Once Co-ordinative Definitions Have Been Chosen?

While every self-consistent system of theoretical geometry can potentially be used to describe the world, Hans Reichenbach has noted that only one geometry will be empirically verified once the theoretical system has been transformed via the introduction of co-ordinative definitions.

Reichenbach writes:

Although every geometrical system can be used to describe the structure of the physical world, the geometrical system taken alone does not describe the structure completely. The description will be complete only if it includes a statement about the behavior of solid bodies and light rays.

Through the co-ordination of physical objects with the undefined terms of a geometry that geometry is "applied" to problems of the world; prior to that co-ordination of object

and term the geometric system is nothing more than a system of undefined terms and relations combined to form various theorems under the governance of the asserted axioms (and, of course, the laws of logic). Thus it is seen that empirical investigation crucially applies only to conditional statements, such as "If we consider a straight line to be the path described by a ray of light, then the results of our experiments with light rays will be in accord with a Euclidean interpretation of the structure of our physical space"; furthermore, such investigation, if I understand it, operates only in a negative fashion. In the example stated, the results of our experiments either will or will not be reconcilable with a Euclidean interpretation; if they are not reconcilable, we must either deny that light rays are straight or consider the case wherein our universe is non-Euclidean.

On the other hand, were the results of experimentation to concur with a Euclidean interpretation we would be left logically in mid-air, unable to claim that light rays indeed describe Euclidean straight lines. Such a decision could be reached ~~if~~ we dealing with an if and only if situation; unfortunately any attempt to establish the conditional statement "If experimental evidence concurs with a Euclidean interpretation, then the path of a light ray describes a Euclidean straight line" would consist of illicitly backwards ~~enumeration~~ since the "then-clause in this case

consists of the co-ordinative definition required for the discovery of the truth of the if-clause. Perhaps this is what Albert Einstein had in mind when he proposed that "as far as the laws of mathematics refer to reality, they are not certain; and as far as they are certain, they do not refer to reality."<sup>169</sup> In this context Einstein considers how the axiom of the straight line for a physical geometry would be interpreted: up to and including the 19th Century, most people would claim that anyone knows what a straight line and a point are; under the more modern interpretation, a scientist about to engage in an exercise of applied rather than theoretical geometry would be conscious of his need to specify what physical objects are to be conventionally regarded as straight lines, points, etc.<sup>170</sup>

Of course most scientists do not do this, either whenever they are about to work on a problem of physics, or even once in their life; in such cases, however, the tacit assumption that they are accepting light rays as fulfilling the role of straight lines, etc., is obvious: if questioned on the matter, perhaps they would react with a surprised "but everyone knows that, for small regions light rays can be considered to be straight lines", or even with an annoyed "no one presumes to work with straight lines in reality, but only in the ideal (mathematical) case"; in any event, such presuppositions can be seen to have been conventionally chosen. (Perhaps in this case I should say that

such conventional choices were programmed into these non-philosophical scientists during their early training.)

### A review of the Factors Which Influenced Poincaré in the Development of His Theory of Geometry

A list of the most important factors which influenced the development by Henri Poincaré of the theory of geometry known as conventionalism reads like an abstract of a thesis concerned with the history of modern geometry.

First and most important, alternative geometries were developed during the Nineteenth Century by theorists who tampered with Euclid's axiom of parallel lines. That an alteration in one of the axioms could still result in a consistent mathematical theory served to call into question the status of the axioms themselves. For centuries Euclid's geometry had served as a paradigm of indubitable truth; the axioms of that geometry had been considered self-evident truths which neither could nor should be called into question. Attitudes towards the status of these axioms changed, however, once the alternative geometries of Lobatchevsky and others were published. The axioms were called "hypotheses" by Riemann, and "facts" by Helmholtz; where Kant had claimed that the axioms derive from pure intuition, Helmholtz claimed that they grew out of idealized experience, and Riemann spoke of them as hypothetical judgements concerning reality.<sup>171</sup>

Various attempts to discover what it may be that grounds our Euclidean perception of the world, such as the work of Helmholtz in physiology and mathematics, influenced Poincaré's later position in favor of the possibility of visualizing non-Euclidean geometries. (Darwin's theories thus indirectly influenced Poincaré, since the latter was familiar with talk about Euclidean visualization as a selectively evolved process favoring human survival.) A second trend at this time consisted in various attempts to prove that the structure of our space in fact corresponds to the description given by Euclidean geometry. Two tacks were usually taken: some maintained that the axioms of Euclid's geometry are synthetic *a priori* statements about the world; others attempted to discover an empirical import for these axioms. Lobatchevsky and Gauss, among many others, attempted various parallax experiments in a vain attempt to empirically confirm or disconfirm the Euclidean hypothesis concerning the structure of our world. Russell's dissertation contained this general experimentalist viewpoint; Russell's explicit attacks upon Poincaré gave the latter the opportunity to air those views which he had developed concerning the import of the above-mentioned parallax experiments. Finally, when Poincaré became acquainted with David Hilbert's *Grundlagen der Geometrie* in 1902, the French mathematician realized that his remarks concerning the conventional status

of axioms applied not only to the axiom of parallels, but to all of the axioms (including the axiom of the dimensions of space).

The development of alternative geometries, then, called for an explanation of the status of the various axioms of these geometries. According to Poincaré's theory, the axioms of Euclidean geometry were neither analytic a priori judgments, nor synthetic a priori judgments, nor experimental facts. Instead he created a fourth option for the status of the axioms of any geometric system: that such axioms are "definitions in disguise", that is, conventions. Our choice among these conventions is guided but not determined by the results of experimentation. Poincaré thus rejected the question "Is Euclidean geometry true (of the world)?" as without sense, and chose instead to maintain that the axioms of geometry in themselves are neither true nor false. For Poincaré one geometry was not more true than another; rather our choice among various geometries for application to the problems of the physical sciences is guided by considerations of self-consistency and utility.

The question may arise: when Poincaré proposes that the axioms of geometry enjoy no more than a conventional status, does he have in mind what is now known as pure geometry, or physical (i.e., applied) geometry, or both? L. Reigier has interpreted Poincaré as holding that the axioms of Euclidean geometry as a theoretical system are

"nominal" conventions, while these axioms as an applied system are "instrumental" conventions.<sup>172</sup> J.J.A. Mooij, however, rejects Rouzier's interpretation, and claims that Poincaré was not concerned with distinguishing pure from applied geometry. While Poincaré was aware of the importance of the distinction between theory and practice, Mooij claims, he was almost exclusively concerned with the impact of his conventionalist thesis upon the disciplines of mechanics and astronomy.<sup>173</sup> As Mooij points out, for Poincaré the acceptance of Euclidean geometry for use in the physical sciences went hand in hand with the acceptance of certain systems of measurement (and co-ordinative definitions). Hence Mooij concludes that, although Poincaré's caution that Euclidean geometry is neither true nor false can certainly be applied to this geometry when it is taken as a theoretical system, the conventionalist thesis achieves its potency when applied to the practical problems of the physical sciences.<sup>174</sup>

C.P.

### CHAPTER III

## RUSSELL'S DISSERTATION IN LIGHT OF RECENT DEVELOPMENTS IN MATHEMATICS AND THE PHYSICAL SCIENCES

In a lengthy section of his Essay on the Foundations of Geometry Russell set forth what he considered to be the axioms of projective geometry. At that time this was a highly laudable accomplishment, for as Russell himself notes the foundations of projective geometry had not been studied in detail. As he modestly put it, "but unfortunately, the task of discovering the axioms of projective Geometry is far from easy. They have, as yet, found no Riemann or Helmholtz to formulate them philosophically."<sup>175</sup> In this concluding chapter I hope to at least partially answer the question: have recent developments in mathematics resulted in the modification or elimination of some of Russell's proposals concerning projective geometry? Perhaps this may best be accomplished by the consideration of a related question: are the axioms of projective geometry as proposed by Bertrand Russell still considered to be valid?

Although Russell's axioms for projective geometry have already been mentioned briefly, I shall recapitulate them before attempting to criticize them. First, Russell proposed that all of the parts of space are qualitatively similar, or, in other words, that space is homogeneous.

Second, he proposed that space is continuous and infinitely divisible (such that successive divisions of space approach a limit in the point). Third, Russell proposed that the dimensionality of space must be of a finite integral number. As has been noted, he further believed that experimental evidence could be adduced to demonstrate that ours is a three-dimensional Euclidean space. As Morris Kline notes in his "Foreward" to Russell's Essay, these axioms for projective geometry have received a serious challenge during this century from the development of the theory of relativity.

First, let us consider the impact of these developments upon Russell's assumption that space is homogeneous. We have already noted the attempt of Calinon to advance a theory which incorporates a variable constant of curvature for our space. Calinon, it was noted, suggested that this space-constant may vary from time to time. In pursuit of this theory Calinon produced three alternatives:

First, our space is and remains rigorously Euclidean; second, our space realizes a geometrical space which differs very little from the Euclidean, but which always remains the same; and third, our space realizes successively in time different geometric spaces; otherwise said, our spatial parameter varies with time, whether it departs more or less away from the Euclidean parameter or whether it oscillates about a definite parameter very near to the Euclidean value.

Again, the theory of relativity proposed by Albert Einstein introduces the concept of a space-metric which varies from

place to place. Morris Kline relates Einstein's theory to the work of Riemann:

Riemann had pointed out that the belief in the homogeneity of space did not take into account the existence of matter; when matter is taken into account, homogeneity disappears. This is precisely what occurs in the theory of relativity; the matter in space becomes absorbed by the geometry of space-time so that the nature of space-time varies from one region to another in accordance with the matter in it.<sup>177</sup>

Russell gives a colorful description of the absorption of the principle of gravitation into the geometry of space-time in his section on relativity theory in Human Knowledge:

According to Einstein, space-time is full of what we may call hills; each hill grows steeper as you go up, and has a piece of matter at the top. The result is that the easiest route from place to place is one which winds round the hills. The law of gravitation consists in the fact that bodies always take the easiest route, which is called a "geodesic".<sup>178</sup>

Characteristic of his open-mindedness, Russell became a disciple of relativity theory later in his career. At the time of his Cambridge dissertation, however, Russell rejected on logical grounds the proposition that space is nonhomogeneous, that is, that space possesses a variable metric. In his Essay Russell argued that if the distance function for our space varied from point to point, we would have to deny the fundamental property of congruence to our space, yet this he cautioned us would be absurd. On the one hand Russell argued that such a change in metric would not be detectable; if an object shrunk when transported from one

place to another, so would the measuring device used to verify its length. On the other hand, Russell argued, (I quote Morris Kline's summary).

that the metric of a space of nonconstant curvature builds up finite distances or lengths on the basis of infinitesimal distances... (for which) the Euclidean distance-function holds and congruence or superposition is available.<sup>179</sup>

Next, we may discuss Russell's contention that our space is Euclidean and three-dimensional. Since Russell's position that the geometric structure of our space is open to direct empirical investigation forms the focus of my own thesis, I will proceed in passing that such an experimentalist position falls prey to Poincaré's theory of conventionalism and Reichenbach's theory of co-ordinative definitions. Furthermore, with Einstein's theory non-Euclidean geometry was introduced into the foundations of physics. Karl Menger describes Einstein's hypothesis about the physical world: "First, that a light ray always follows the shortest path; and second, that these shortest paths have the properties of lines in a non-Euclidean space."<sup>180</sup> Again, Menger notes that "Einstein, in carrying out his program, went so far - and essentially no farther than - to assume that the space-time of the physical world is a four-dimensional Riemann space."<sup>181</sup>

The third of Russell's ideas to be affected by

developments during this century was that (in the oft-quoted words of Cayley) "projective geometry is all geometry". This belief, which gave rise to Russell's emphasis on projective geometry as fundamental to any metric geometry, has in one sense been superseded by the development of a new branch of mathematics, namely Topology; Morris Kline explains the claim that projective geometry is more general than (and hence fundamental to) any particular metric geometry:

The properties which are invariant under projection and section deal with the collinearity of points... with the concurrence of lines (that is, when a set of lines meet in one point), with cross-ratio, and with the fundamental roles of point and line as exhibited by the principle of duality. On the other hand, Euclidean geometry deals with the equality of lengths, angles, areas. A comparison of these two classes of properties suggests that projective properties are simpler than those treated in Euclidean geometry. One might say that projective geometry deals with the very formation of the geometrical figures whose congruence, similarity and equivalence (equality of areas) are studied in Euclid. In other words, projective geometry is more fundamental than Euclidean geometry.<sup>182</sup>

In contrast with projective geometry, Courant and Robbins describe Topology thus:

The new subject, called analysis situs or topology, has as its object the study of the properties of geometrical figures that persist even when the figures are subjected to deformations so drastic that all their metric and projective properties are lost.<sup>183</sup>

Again, they define "topological transformations" thus:

A topological transformation of one geometrical figure  $A$  into another figure  $A'$  is given by any correspondence  $P-P'$  between the points  $P$  of  $A$  and

the points  $p'$  of  $A'$  which has the following two properties: 1. The correspondence is biunique. This means that to each  $p$  of  $A$  there corresponds just one point  $p'$  of  $A'$ , and conversely. 2. The correspondence is continuous in both directions. This means that if we take any two points  $p, q$  of  $A$  and move  $p$  so that the distance between it and  $q$  approaches zero, then the corresponding points  $p', q'$  of  $A'$  will also approach zero, and conversely. Any property of a geometrical figure  $A$  which holds as well for every figure into which  $A$  may be transformed by a topological transformation is called a topological property of  $A$ , and topology is the branch of geometry which deals only with the topological properties of figures.<sup>184</sup>

Since during the period while Russell was a Cambridge student projective geometry was indeed the most fundamental "science of space" (if I may use that metaphor), it seems natural that he should have considered the projective properties to be the necessary preconditions for our experience of the world. At our present stage in the history of mathematics, however, it can be seen that Topology studies transformations more general than projection and section; hence it now appears that Topology is logically prior to projective geometry.

The twin Twentieth Century creations of the theory of Topology and the theory of relativity in effect undermine Russell's proposals concerning the axioms of projective geometry. The creation of Topology demonstrated that projective geometry is not the most fundamental science of body and space, as Russell had believed the latter to be. The formulation of the theory of relativity demonstrated that,

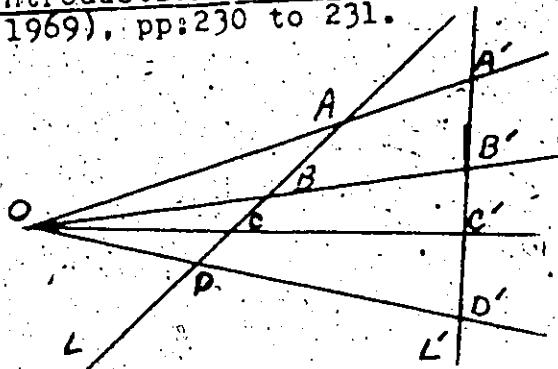
contrary to the early claims of Russell, a theory which incorporates a model of space which is non-homogeneous, non-Euclidean, and of a dimensionality greater than three is not necessarily unintelligible.

FOOTNOTES

1. Morris Kline's "Foreword"; p.i., to B.Russell, An Essay on the Foundations of Geometry (New York: Dover, 1956).
2. Russell pointed out in his Autobiography that his concern for certainty in the foundations of geometry went back to his first encounter with Euclid under the tutelage of his older brother.
3. For an introductory treatment of projective geometry see R.Courant and H.Robbins, What Is Mathematics? (New York: Oxford U.P., 1941), p.217; also D.Pedoe, An Introduction to Projective Geometry (New York: Pergamon Press, 1963), p.5; H.M.S.Coxeter, Introduction to Geometry (New York: John Wiley and Sons, 1969), pp:230 to 231.

4.

(Diagram A)



5. Morris Kline, "Projective Geometry", in Men and Numbers, edited by J.R.Newman (New York: Simon and Schuster, 1956), p.632.
6. Ibid.,p.639. For a rigorous treatment of projective co-ordinates see section 14.2 of H.M.S.Coxeter's Introduction to Geometry.

7. B.Russell, An Essay on the Foundations of Geometry (New York: Dover, 1956), p.3.

8. [Ibid.,p.3.]

9. Ibid.,p.30.

10. Ibid.,p.30.

11. Ibid.,p.30.

12. Ibid.,pp.30 to 31.

13. Ibid.,pp.33 to 34.

14. Ibid., p.35.
15. Ibid., pp.35 to 36; italics mine.
16. Ibid., pp.36 to 37.
17. Ibid., p.112.
18. Ibid., p.113; Russell, in a footnote to this section, cites H.Poincaré, "Non-Euclidean Geometry", Nature, XLV, 1891-2; H.Poincaré, "L'espace et la géométrie", Revue de Métaphysique et de Morale, Nov. 1895; H.Poincaré, "Réponse à quelques critiques", Revue de Métaphysique et de Morale, Jan. 1897.
19. Ibid., p.115.
20. Ibid., pp.115 to 116.
21. Ibid., p.147.
22. Ibid., pp.118 to 119.
23. Ibid., p.147.
24. Ibid., p.148.
25. Morris Kline, op.cit., p.14; he states that "those who would like to pursue the arguments initiated by the publication of this work (Russell's Essay) will find stimulating articles in journals such as the Revue de Métaphysique et de Morale, Mind, Philosophical Review, and Nature for the years immediately following 1897.
26. B.Russell, "Review of Étude sur l'espace et le temps by Georges Lechalas", Mind, 5(17), Jan. 1896, p.128.
27. Ibid., p.128.
28. Ibid., p.128.
29. L.Couturat, "Review of An Essay on the Foundations of Geometry by B. Russell", Revue de Métaphysique et de Morale, May. 1898, pp.354 to 380.
30. Ibid., p.354; translation is mine.
31. Ibid., p.361.

32. B.Russell, "Review of Theoretical Mechanics: An Introductory Treatise on the Principles of Dynamics by A.E.H.Love", Mind, 7(27), July 1898, pp.404 to 411.
33. Ibid., p.405.
34. B.Russell, "Review of Essai sur la classification des Sciences by Edmond Goblot", Mind, 7(28), Oct. 1898, pp.567 to 568.
35. Ibid., p.567.
36. B.Russell, "Les Axiomes Propres à Euclid: sont-ils empiriques?", Mind, Nov. 1898, pp.753 to 776.
37. Ibid., p.759; translation is mine.
38. Ibid., p.759.
39. Ibid., p.759.
40. Ibid., pp.759 to 760.
41. Ibid., p. 760.
42. Russell cites A.N.Whitehead, "The Surfaces of Non-Euclidean Spaces", Proceedings of the London Mathematical Society, March 10, 1898.
43. B.Russell, "Les Axiomes Propres à Euclid", p.760.
44. Ibid., p.761.
45. Ibid., p.761.
46. Ibid., p.761.
47. Ibid., p.762.
48. Ibid., p.761.
49. Ibid., p.761.
50. Ibid., p.762.

51. Ibid., p. 776.

52. Ibid., p. 776.

53. H. Poincaré, "Des Fondements de la Géométrie, à propos d'un livre de M. Russell", Revue des Métaphysique et de Morale, May, 1879, pp. 251 to 279; all translations are my own.

54. Ibid., p. 251.

55. Ibid., p. 252.

56. Ibid., p. 254.

57. Ibid., p. 254.

58. Ibid., p. 254.

59. Ibid., p. 254.

60. Ibid., p. 264.

61. Ibid., pp. 264 to 265.

62. Ibid., p. 265.

63. Ibid., p. 265.

64. Ibid., p. 265.

65. Poincaré felt that this point was important enough to include it almost verbatim in Science and Hypothesis (New York: Dover, 1952), p. 73.

66. H. Poincaré, "Des Fondements de la Géométrie", p. 266.

67. Ibid., p. 266.

68. Ibid., p. 267.

69. Ibid., pp. 267 to 268.

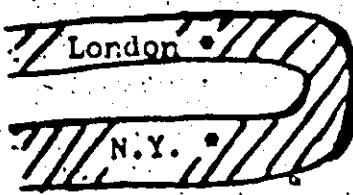
70. Ibid., p. 268.

71. Ibid., p. 268.

72. Ibid., p. 279.

73. B.Russell, "Sur les Axiomes de la Géométrie", Revue de Métaphysique et de Morale: Nov. 1899, p.685; all quotations are taken from Russell's handwritten manuscript of this article, courtesy of the Russell Archives, McMaster University, Hamilton, Ontario; all pagination cited from the Revue de Métaphysique et de Morale.
74. Ibid., p.685.
75. Ibid., p.686.
76. Ibid., p.686.
77. Ibid., p.687.
78. Ibid., p.687.
79. Ibid., p.687.
80. Ibid., p.688.
81. Ibid., p.688.
82. Ibid., p.689.
- 83.

(Figure B)



(The shaded area represents our space, so that to "jump" from N.Y. to London would involve leaving our space)

84. Ibid., p.689.
85. Ibid., p.690.
86. Ibid., p.691.
87. Ibid., p.692.
88. Ibid., p.693.
89. Ibid., p.695.
90. Ibid., p.696.
91. Ibid., p.700.

92. Ibid., p.700.
93. H.Poincaré, "Sur la principes de la Géométrie, réponse à M.Russell", Revue de Métaphysique et de Morale, Jan. 1900, p.73.
94. Ibid., p.79.
95. H.Poincaré, Science and Hypothesis (New York: Dover, 1952).
96. Ibid., p.xxiv.
97. B.Russell, "Review of Science and Hypothesis by H.Poincaré", Mind, 14(55), July 1905, pp.412 to 418.
98. Ibid., p.415.
99. K.Blackwell, "An Essay on the Foundations of Geometry", Russell: the journal of the Bertrand Russell Archives, VI, summer, 1972, p.3; Blackwell cites Russell's Autobiography, I, p.130.
100. Russell: the journal of the Bertrand Russell Archives, IX, spring, 1973, pp.16 to 17.
101. B.Russell, My Philosophical Development (London: George Allen and Unwin Ltd., 1959), pp.39 to 40.
102. D.M.Y.Sommerville, The Elements of Non-Euclidean Geometry (New York: Dover, 1958), pp.209 to 210.
103. E.Cassirer, Einstein's Theory of Relativity (New York: Dover, 1953), p.430.
104. Ibid., pp.430 to 431.
105. Hans Reichenbach, "The Philosophical Significance of the Theory of Relativity", in Albert Einstein: Philosopher-Scientist, "The Library of Living Philosophers", edited by Paul Arthur Schilpp (Illinois: Open Court Press, 1969), p.307.
106. E.Cassirer, Einstein's Theory of Relativity, p.438.
107. Max Born, Einstein's Theory of Relativity (New York: Dover, 1962), p.333.
108. E.Cassirer, Substance and Function (New York: Dover, 1953), p.108.

109. E.Cassirer, Einstein's Theory of Relativity, p.433.
110. B.Russell, The Principles of Mathematics (Great Britain: George Allen and Unwin Ltd., 1903), p.430.
111. H.P.Robertson, "Geometry as a Branch of Physics", in Albert Einstein: Philosopher-Scientist, "The Library of Living Philosophers", edited by P.A.Schilpp (Illinois:Open Court Press, 1969), p.316; see also H.Peigl, "The Orthodox View of Theories", in Analysis of Theories and Methods of Physics and Psychology, "Vol.IV, Minnesota Studies in the Philosophy of Science", edited by N.Radner and S.Winokur (Minneapolis: University of Minnesota Press, 1970), p.4.
112. H.Poincaré, Mathematics and Science: Last Essays, translated by J.W.Bolduc (New York: Dover, 1963). (First published as Dernières Pensées in 1913.)
113. S.P.Barker, "Geometry", in The Encyclopedia of Philosophy, vol.3, edited by Paul Edwards (New York: Macmillan and Co., 1967), p.287.
114. H.Poincaré, The Foundations of Science, translated by G.B.Balster (New York: The Science Press, 1929), pp.235 to 236. Poincaré opens the third chapter of The Value of Science with such a discussion of the meaning of "straight line", for astronomical experiments. Again, on pp.72 to 73 of his Science and Hypothesis Poincaré directly discusses the parallax experiment, concluding there that if we were to discover negative parallaxes we would be faced with a choice: "we could give up Euclidean geometry, or modify the laws of optics, and suppose that light is not rigorously propagated in a straight line."
115. E.Cassirer, Substance and Function, p.108.
116. H.P.Robertson, op.cit., pp.316 to 317.
117. E.Cassirer, Einstein's Theory of Relativity, p.436.
118. H.Poincaré, Science and Hypothesis, p.50.
119. Ibid., p.xxiv.
120. E.Cassirer, Einstein's Theory of Relativity, p.431.

121. Karl Menger, "The Theory of Relativity and Geometry". In Albert Einstein: Philosopher-Scientist, "The Library of Living Philosophers", edited by P.A. Schilpp (Illinois: Open Court Press, 1969), p.464.
122. A. Grunbaum, Geometry and Chronometry in Philosophical Perspective (Minneapolis: University of Minnesota Press, 1962), p.120. The only reasonably complete and for the most part fair treatment of the Russell-Poincaré debate which I have found in the literature is that presented by Adolf Grunbaum, particularly in his Geometry and Chronometry in Philosophical Perspective, but also in his Philosophical Problems of Space and Time (New York: Alfred A. Knopf, 1963). On the other hand, Bas C. van Fraassen, in his Introduction to the Philosophy of Space and Time (New York: Random House, 1970), claims to "abstract" from the Russell-Poincaré exchange material concerning the problem of an intrinsic time-metric; in fact Russell and Poincaré say almost nothing about that problem, since their quarrel concerned the empirical nature of the axioms of geometry, and, derivative from this, the physical investigation of the real nature of space. Furthermore, Russell's position in this exchange is identified by van Fraassen with that of Bosanquet (so that van Fraassen can trot out a catalogue of Bosanquet quotations), yet I have not come across the latter's name once in reading through the debate.
123. A. Grunbaum, Philosophical Problems of Space and Time (New York: Alfred A. Knopf, 1963), p.119.
124. A. Grunbaum, "Law and Convention in Physical Theory", in Current Issues in the Philosophy of Science, edited by H. Peigl and G. Maxwell (Holt, Rinehart and Winston: New York, 1961), p.142.
125. A. Grunbaum, Geometry and Chronometry in Philosophical Perspective, p.120.
126. A.S. Eddington, Space, Time, and Gravitation (Cambridge: Cambridge U.P., 1953), p.9.
127. A. Grunbaum, "Law and Convention in Physical Theory", p.147.
128. Hans Reichenbach, Axiomatization of the Theory of Relativity, translated and edited by Maria Reichenbach (Berkeley and Los Angeles: University of California Press, 1969), p.13.
129. A. Grunbaum, Geometry and Chronometry in Philosophical Perspective, p.106.

130. Rudolf Carnap, "Replies and Systematic Expositions", in The Philosophy of Rudolf Carnap, "The Library of Living Philosophers", edited by P.A. Schilpp (Illinois: Open Court Press, 1963), p.958.
131. H.Reichenbach, "The Philosophical Significance of the Theory of Relativity", p.297.
132. Ibid., p.297.
133. Ibid., p.293.
134. Ibid., p.294.
135. Ibid., p.298.
136. A.Grunbaum, Philosophical Problems of Space and Time, p.131; also found in his Geometry and Chronometry in Philosophical Perspective, p.122. Grunbaum is citing the work of L.Rougier, La Philosophie Géométrique de Henri Poincaré (Paris: P.Alcan, 1920), pp.200 to 201.
137. A.Einstein, "Geometry and Experience", in Readings in the Philosophy of Science, edited by M.Brodbeck and H.Peagi (New York: Appleton-Century-Crofts, 1953), p.191.
138. E.A.Wilne, Sir James Jeans (Cambridge: Cambridge U.P., 1952), p.82; italics mine.
139. H.Helmholtz, "On the Origin and Significance of Geometrical Axioms", in Men and Numbers, edited by James R. Newman (New York: Simon and Schuster, 1956), p.666.
140. A.Grunbaum, Geometry and Chronometry in Philosophical Perspective, p.113.
141. B.Russell, "The Logic of Geometry", Mind, 5(17), Jan. 1896. Van Fraassen would not have had to pull a rabbit from his hat (see note 122 above) if he had worked from this article for material concerning Russell's views on time-metrics!
142. B.Russell, "The A Priori in Geometry", Proceedings of the Aristotelean Society, 3(2), 1896.
143. Ibid., p.99.
144. Ibid., pp.109 to 110.

145. B.Russell, "Is Position in Time and Space Absolute or Relative?", Ibid., 10(39), July, 1901, p.20. Einstein's theory of relativity caused Bertrand Russell to return, in Human Knowledge: Its Scope and Limits, to the views of Bradley which he had despised with youthful enthusiasm during his "revolt into realism" with G.E. Moore.
146. B.Russell, "Geometry, Non-Euclidean", initial entry in The Encyclopaedia Britannica (Cambridge: Cambridge U.P., 1902), p.666.
147. B.Russell, "Geometry, Section VI: Non-Euclidean Geometry", initial entry (with A.N.Whitehead) in The Encyclopaedia Britannica, Vol.XI (Cambridge: Cambridge U.P., 1910), p.728.
148. B.Russell, "Geometry, Non-Euclidean", p.671.
149. Ibid., p.674.
150. Ibid., p.674.
151. Ibid., p.674.
152. B.Russell, "Geometry, Section VI: Non-Euclidean Geometry", p.729.
153. B.Russell, "Geometry, Non-Euclidean", p.673.
154. Russell wrote to Lady Ottoline Morrell (dated January 20, 1914), "At last I have finished the Poincaré preface... it cost me a frightful lot of time and thought for such a short thing. It was a delicate matter, as the book contains a fierce attack on me, which I thought ignorant and unfair, but which nearly destroyed my reputation in France." Russell is referring to his preface to Poincaré's Dernières Pensées.
155. Bertrand Russell's signed preface to Geometry and Introduction by Jean Nicod (Paris: 1924), p.xiii.
156. Ibid., p.xiii.
157. Ibid., p.xiv.
158. E.Cassirer, Einstein's Theory of Relativity, p.438.
159. Ibid., p.439.

160. H.P. Robertson, op.cit., p.322.
161. See A.Calinon, "Les espaces géométriques", Revue Philosophique, v.27, 1889.
162. H.J. Paton, Kant's Metaphysics of Experience (London: George Allen and Unwin Ltd., 1936), p.162.
163. Ibid., p.166.
164. H.Reichenbach, The Rise of Scientific Philosophy (Berkeley and Los Angeles: University of California Press, 1951), p.138.
165. H.Reichenbach, "The Philosophical Significance of the Theory of Relativity", pp.298 to 299.
166. H.Reichenbach, The Rise of Scientific Philosophy, p.140; see also his Philosophy of Space and Time, section 11: "Visualization of Non-Euclidean Geometry".
167. A.Grunbaum, Philosophical Problems of Space and Time, p.334.
168. H.Reichenbach, The Rise of Scientific Philosophy, p.133.
169. A.Einstein, "Geometry and Experience", p.189.
170. Ibid., p.190.
171. H.Preudenthal, "The Main Trends in the Foundations of Geometry in the 19th Century", in Logic, Methodology and the Philosophy of Science, edited by Nagel, Suppes, and Tarski (Stanford:Stanford U.P., 1962), p.617.
172. J.J.A.Mooij, "La Philosophie Géométrique de Henri Poincaré", Synthese, vol.16, 1966, p.59.
173. Ibid., p.59.
174. Ibid., pp.59 to 60; Mooij claims, that "His (Poincaré's) notion of 'convention' is not characteristic of abstract axiomatic theory."
175. B.Russell, Essay on the Foundations of Geometry, p.118.
176. A.Calinon, "Les espaces géométriques", in Revue Philosophique, v.27, 1889, pp.588 to 595; translated by H.P. Robertson and cited in the latter's "Geometry as a Branch of Physics", p.322.

177. Morris Kline's "Foreword" to Essay on the Foundations of Geometry. p.v.
178. B.Russell. Human Knowledge: Its Scope and Limits (New York: Simon and Schuster, 1948), p.310.
179. Morris Kline's "Foreword" to Essay on the Foundations of Geometry. p.iv.
180. Karl Menger. "The Theory of Relativity and Geometry". p.464.
181. Ibid., p.465.
182. Morris Kline. "Projective Geometry", in Men and Numbers, edited by J.R.Newman (New York: Simon and Schuster, 1956), p.638.
183. R.Courant and H.Robbins. What Is Mathematics? (New York: Oxford U.P., 1941), p.235.
184. Ibid., p.241.

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