

# RUSSELL, CLIFFORD, WHITEHEAD AND DIFFERENTIAL GEOMETRY

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When Russell was fifteen, he was given a copy of W. K. Clifford's *The Common Sense of the Exact Sciences* (1886). Russell later recalled reading it immediately "with passionate interest and with an intoxicating delight in intellectual clarification". Why then, when Russell wrote *An Essay on the Foundations of Geometry* (1897), did he choose to defend spaces of homogeneous curvature as a priori? Why was he almost completely silent thereafter on the subject of Clifford, and his writings on geometry and space? We suggest that Russell may have avoided Clifford's hypothesis that space had heterogeneous curvature because it seemed impossible to reconcile a coherent theory of measurement with a space of variable curvature. Whitehead objected to Einstein's general theory of relativity on this basis, formulating an alternate theory that preserved the constant curvature of space and, therefore, a familiar sense of measurement. After Einstein's general theory, Russell chose to distance himself from the position he argued in the *Essay*.

**W**illiam Kingdon Clifford (1845–1879), the gifted Victorian mathematician and public champion of scientific thought, was a man in whom Russell saw a reflection of himself. They had, indeed, a lot in common: both were Cambridge-trained mathematicians with wide intellectual interests and a gift for writing about them in a popular manner, both were members of the Cambridge Apostles, both were freethinkers, and both shared a sort of swift, intransigent intellec-

tual style which scorned established wisdom. In his essay “The Ethics of Belief” (1877) Clifford famously maintained “it is wrong, everywhere, and for any one, to believe anything upon insufficient evidence.”<sup>1</sup> This was an inspiring doctrine for a young iconoclast, though Russell in maturity held it in a more moderate (and more defensible) form: “it is undesirable to believe a proposition when there is no ground whatever for supposing it true” (*SE*, p. 11).

Russell discovered Clifford early. When he was fifteen, one of his tutors, John Stuart, gave him a copy of *The Common Sense of the Exact Sciences* (1886).<sup>2</sup> Russell read it straightaway “with passionate interest and with an intoxicating delight in intellectual clarification”.<sup>3</sup> Fifty-seven years later, Russell was called upon to write a preface for a reprint of the book: his enthusiasm for it had evidently not diminished, and he describes the book in ecstatic terms. Yet, he acknowledges that, until he came to write the preface, he had not even looked at the book since reading it at fifteen. Indeed, it has to be admitted that, for all Russell’s enthusiasm for Clifford and their similarity of outlook, Clifford does not bulk large in Russell’s corpus. There are only a handful of references to him in Russell’s *Collected Papers* (most of them occurring in the article he wrote on non-Euclidean geometry for the *Encyclopaedia Britannica*; see *Papers* 3: 489–90, 501, 504), and a few in his books; but none, for example, in *A History of Western Philosophy*. Yet it was not that Russell lost his enthusiasm for Clifford and had to be reminded of it when he

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<sup>1</sup> W. K. Clifford, “The Ethics of Belief”, *Contemporary Review* (1877), reprinted in his *Lectures and Essays*, ed. L. Stephen and F. Pollock (London: Macmillan; 2nd edn., 1886), vol. 2, pp. 177–211, at 186. This was the remark which provoked William James to write an even more famous essay, “The Will to Believe” (1896), which purported to set out the circumstances in which it was permissible to believe on no evidence at all.

<sup>2</sup> Clifford, *The Common Sense of the Exact Sciences*, 2nd edn. (London: Kegan Paul, Trench, & Co., 1886). The copy is in Russell’s library (RL no. 2788). The flyleaf is inscribed “Bertrand Russell from his tutor J. Stuart. New Years Day 1888.” Apart from a capital “L” pencilled into the left-hand margin of page 1, it is not annotated. Nothing is known about Stuart, one of a long series of tutors who taught Russell at home up to the age of sixteen.

<sup>3</sup> See Russell’s preface to Clifford, *The Common Sense of the Exact Sciences*, ed. K. Pearson and J. R. Newman (New York: Knopf, 1946), as reprinted in *Papers* 11: 317; see also N. Griffin and A. Lewis, “Bertrand Russell’s Mathematical Education”, *Notes and Records of the Royal Society of London* 44 (1990): 51–71 (at 53). All subsequent references to *The Common Sense of the Exact Sciences* are to the Knopf edition as reprinted by Dover in 1955.

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wrote the preface. In 1912 he told Ottoline Morrell that Clifford was “an absolutely first-rate mathematician, [who] cared immensely about philosophy.... All his writing has the clearness and force that comes of white-hot intellectual passion.”<sup>4</sup> So one problem in dealing with Russell’s encounter with Clifford is to explain this comparative silence. In this paper, we hope to explain one part of it: the fact that Russell, in his early work on the philosophy of geometry, did not take more seriously Clifford’s ideas about spaces of variable curvature. We suggest Russell had good reason to neglect such models of space. A coherent theory of measurement for spaces of non-constant curvature had not been produced. This may explain why Russell chose to defend *a priori* spaces of constant curvature in his 1897 book, *An Essay on the Foundations of Geometry*. Clifford did not clarify how measurement was operational in spaces of heterogeneous curvature; neither did Einstein’s theory of relativity offer a solution to this stumbling block. Whitehead objected to Einstein’s theory on these grounds. In its place Whitehead proposed an alternate theory that preserved the constant curvature of space and, therefore, a familiar sense of measurement as well.

### I. RUSSELL, CLIFFORD, AND CLIFFORD’S PHILOSOPHY OF MATHEMATICS

It is clear from Russell’s preface to *The Common Sense of the Exact Sciences* that one of the causes of his youthful enthusiasm for Clifford was what he took to be Clifford’s belief that the exact sciences, and in particular mathematics, could become agents for social and material progress. In a short article written a few years after the preface, Russell describes his early belief that the study of mathematics could help humanity achieve a better society and a higher state of wellbeing:

I had thoughts of mathematics, as the Russians still do, as primarily a help in making machines, and in day-dreams I have seen myself inventing some wonderful labour-saving device ... I began to hope that human motives could be treated like forces in mechanics, and to imagine a quasi-mathematical psychology which would have been something like that in the third book of Spinoza’s ethics.<sup>5</sup>

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<sup>4</sup> Russell to O. Morrell, no. 375, 10 March 1912.

<sup>5</sup> Russell, “A Turning-Point in My Life”, *The Saturday Book* 8 (1948): 143–4.

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Russell identified these thoughts as arising from his reading of Clifford, from whom he adopted the idea that mathematics was the exemplar of reason, and that reason was the foundation of all sound belief.<sup>6</sup> The search for certainty of belief through sound reasoning was connected, for Russell, with social and material progress. Through the acquisition of sound and certain knowledge, Russell believed that the human race might become “more humane, more tolerant, and more enlightened, with the consequence that war and disease and poverty, and the other major evils of our existence, would continually diminish” (*Papers* 11: 320). As such, mathematics, the most important and potentially precise kind of reasoning, became the focus of Russell’s larger quest to find certainty in knowledge (*PfM*, p. 10). “In this beneficent process”, Russell wrote in his preface,

rational knowledge was to be the chief agent, and mathematics, as the most completely rational kind of knowledge, was to be in the van. This faith was Clifford’s, and it was mine when I first read his book; in turning over its pages again, the ghosts of the old hopes rise up to mock me. (*Papers* 11: 320)

It is odd that Russell identifies his “old hopes” as having arisen from *The Common Sense of the Exact Sciences*, since this book does not cast certainty of belief or sound reasoning as agents of social progress. Nor are these sentiments strongly evidenced in Clifford’s collected *Lectures and Essays*, though Russell may have read some of his own enthusiasm into “On the Aims and Instruments of Scientific Thought” (1872).<sup>7</sup> Though a great deal of that lecture is taken up with emphasizing the gap between the exactness of the mathematical sciences and the approximations of the experimental ones, Clifford does argue that scientific thought is not thought about scientific matters but thought of a certain exact and evidence-guided kind about any matter at all. He looks forward, for example, to the development of psychology as an exact science (p. 142), and he scorns any idea, whether derived from Kant and the German idealists or from Herbert Spencer, of the ultimate unknowability of things. Clifford gets perhaps as close as he ever gets to stating Russell’s

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<sup>6</sup> A. R. Garciadiego, *Bertrand Russell and the Origins of the Set-theoretic “Paradoxes”* (Boston: Birkhäuser Verlag, 1992), p. 48; Monk, 1: 27.

<sup>7</sup> Clifford, *Lectures and Essays*, ed. L. Stephen and F. Pollock, 2 vols. (London: Macmillan, 1879): 124–57.

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hopes at the end of the lecture:

By saying that the order of events is reasonable we do not mean that everything has a purpose, or that everything can be explained, or that everything has a cause; for neither of these is true. But we mean that to every reasonable question there is an intelligible answer, which either we or posterity may know *by the exercise of scientific thought*. . . . Remember, then, that it is the guide of action; that the truth which it arrives at is not that which we can ideally contemplate without error, but that which we may act upon without fear; and you cannot fail to see that scientific thought is not an accompaniment or condition of human progress, but human progress itself. (Pp. 156–7)

The youthful Russell dreamed that the growth of rational knowledge—mathematics being the most rational knowledge of all—could ameliorate mankind. While Russell identified this as Clifford’s thought, it may not be the case that Clifford held this view himself.

Despite Russell’s enthusiasm, Clifford’s philosophy of mathematics, expressed most fully in “The Philosophy of the Pure Sciences”,<sup>8</sup> was less than fully coherent, at least partly because Clifford’s early death had cut short its development. *The Common Sense of the Exact Sciences* itself was left incomplete at his death and parts of the text were posthumously edited, revised and even written by Karl Pearson. Howard Smokler, in one of the few studies of Clifford’s philosophy of mathematics, concluded that his account of arithmetic was “too obscure to be properly evaluated”.<sup>9</sup> It was an amalgam of empiricist and rationalist elements. On the rationalist side, there were principles of organization, including principles by which sensory experience was arranged to yield distinct objects, a principle of the uniformity of nature, and principles for the analysis of concepts to give definitions. On the empirical side there was the sensory input itself and the process of counting. Starting from the concept of “distinct object”, the principle of the uniformity of nature was used to ensure that objects remained distinct throughout space and time and under different arrangements. Through counting, numerals could be assigned to an ordered sequence of sets of distinct objects, each

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<sup>8</sup> In *Lectures and Essays*, 1: 254–340. Russell made a summary of parts of this work in 1896 (RA1 210.006550–F4), though it is difficult to see that it had any impact on his own thinking at this time.

<sup>9</sup> Howard Smokler, “Scientific Concepts and Philosophical Theory: an Essay in the Philosophy of W. K. Clifford” (unpublished PH.D. thesis, Columbia U., 1959), p. 109.

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set containing one more element than its predecessor. Finally, via the principles governing definition, arithmetical operations could be defined in terms of counting. The approach had serious limitations, offering no obvious way to extend the process to signed integers or real numbers. It is difficult to discern what his attitude was to the real numbers, because he held that the science of number and the science of continuous quantity were two fundamentally different things, the one founded on the “hypothesis of the distinctness of things” and the other on the “totally different hypothesis of continuity”. Nonetheless, he goes on to note the “close and extensive” relations between the two sciences; so close, indeed, as to leave one perplexed as to how two such similar sciences could be based upon apparently contradictory hypotheses.<sup>10</sup> Perhaps the best that can be said of Clifford’s account is that, in spite of itself, it makes clear why mid-nineteenth-century mathematicians were looking for an arithmetical theory of continuous quantity, though Clifford himself thought such attempts were “logically false and educationally mischievous” (p. 337). Of these views, inasmuch as they appear in *The Common Sense of the Exact Sciences*, Russell says diplomatically in his preface:

The opening chapter, on Number, although it says admirably what, in the seventies, seemed best worth saying, cannot tell the reader what is now known to be most important, since in this subject the great advances made by Dedekind, Cantor, and Frege came in the decade immediately following Clifford’s death. He was, moreover, a geometer rather than an analyst, and it was in geometry that his mathematical intuition appeared at its best. (*Papers* II: 318)

Clifford’s approach, for all its weaknesses to modern eyes, was not unsophisticated by the standards of its day. Reading it in 1888 probably inoculated Russell against Mill’s much cruder account of arithmetic truths as inductive generalizations from empirical experience in *A System of Logic*,<sup>11</sup> which he read two years later. Evidently he had not accepted Clifford’s account of how we come by knowledge of arithmetic, but he must have recognized that Clifford’s approach offered rather better hopes of an explanation than Mill’s.

The main positive lesson that Russell took from Clifford’s book con-

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<sup>10</sup> “The Philosophy of the Pure Sciences”, in *Lectures and Essays*, I: 337.

<sup>11</sup> J. S. Mill, *A System of Logic* (1843), III, xxiv, 5; in Vol. 7 of *The Collected Works of John Stuart Mill*, ed. J. M. Robson (Toronto: U. of Toronto P., 1973).

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cerned geometry. When he first read the book, Russell had only recently learnt of non-Euclidean geometry and, like many people in that position, was considerably perplexed by it. It must have seemed, at the very least, a dire warning of human fallibility. Clifford did not ignore that aspect of it, emphasizing that what had previously been taken to be certain and exact knowledge of even those parts of the universe that were inaccessible to human observation, was no longer certain, could not be established as exact, and could not be assumed to hold everywhere.<sup>12</sup> But unlike many philosophers who thought that non-Euclidean geometry must be resisted lest thought refute itself, Clifford maintained that this was a great step forward, as important as that taken by Copernicus, and an intellectual adventure as well, a “relief from the dreary infinities of homaloidal space” (*ibid.*, p. 323). From a philosophical point of view, it is hard to overstate the elegance and clarity with which Clifford presents the new geometries in the third of the lectures on the philosophy of the pure sciences; the presentation in *The Common Sense of the Exact Sciences* is much briefer and more purely didactic. Nonetheless, reading the latter book laid to rest, at least to a limited extent, Russell’s feeling of geometrical doubt that had been occasioned by non-Euclidean geometry (*MPD*, p. 36), no doubt, as Garciadiego suggests,<sup>13</sup> by helping him understand that non-Euclidean geometries do not contradict the Euclidean one. “[W]hat I read in this book”, he said, “did much to diminish the bewilderment I had been feeling. In spite of all the work that has since been done, hardly anything that Clifford (or Karl Pearson<sup>14</sup>) says on this subject could be bettered by a writer in the present day” (*Papers II*: 318).

It is surprising, then, considering how emphatically Russell cites Clifford’s book as an early influence on his thought, to find that when Russell came to work on the philosophy of non-Euclidean geometry for his first philosophical book, *An Essay on the Foundations of Geometry* (1897), based on his fellowship dissertation of 1895, he not only largely ignores Clifford’s work, but dismisses as impossible kinds of geometry on which Clifford had set much store. Russell makes only two brief references to Clifford in his *Essay*. Both cite Clifford as being among a number of scientists who hold “a naïve realism as regards absolute space” and

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<sup>12</sup> Cf. “The Philosophy of the Pure Sciences”, pp. 295–300.

<sup>13</sup> Garciadiego, pp. 47–8. See also *Papers II*: 318.

<sup>14</sup> Who was responsible for writing the whole of the key chapter on position (Chap. 4).

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who investigate physical space “in the spirit of a chemist discussing whether hydrogen is a metal, or an astronomer discussing the nebula hypothesis”.<sup>15</sup> In his first reference to Clifford, Russell says that Clifford “seems to have thought that we actually see the images of things on the retina”, which Russell uses to illustrate his point that Clifford conceives geometry as he does physics, as “an experimental science” (*EFG*, pp. 93, 94). Clifford does see the science of space as entirely empirical and subject to the limits of physical perception. He explains in “The Philosophy of the Pure Sciences” that each experience of space is “filled in” by our thought, not at random but by certain rules. He gives as an example the fact that “Parallelism is impossible on the curved pictures of my retina; so [the condition that we call parallel] is part of the filling in [of our thought]” (p. 261). In Russell’s second reference, he notes that Clifford’s empirical view of space leads him to find current evidence wholly insufficient to decide the nature of space on scales of the infinite and the infinitesimal (*EFG*, p. 97). Russell refers to a passage where Clifford states that “Even apart, then, from our knowledge of the way nerves act in carrying messages, it appears that we have no means of knowing anything more about an aggregate than that it is too fine grained for us to perceive its discontinuity, if it has any.”<sup>16</sup> Russell argues that more than one model of space could be acceptable to our a priori need to intuit space, and so he agreed with Clifford that the philosophical possibility of non-Euclidean geometry as the structure of space demoted the science of space to the level of empirical uncertainty existent in chemistry or astronomy. As Russell admits, the science of space is now “left to the mercy of approximate observations of stellar triangles—a meagre support, indeed, for the cherished religion of our childhood [i.e. Euclidean geometry]” (*EFG*, p. 97). Russell acknowledges that non-Euclidean geometry has opened up the possibility that space is non-Euclidean (a possibility that he takes very seriously). But he does not accept Clifford’s more radical suggestion that space may have a variable curvature, let alone his conjecture that non-constant curvature may ex-

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<sup>15</sup> Russell, *An Essay on the Foundations of Geometry* (New York: Dover, 1956), pp. 93, 97. Further references to Russell’s *Essay* (abbreviated as *EFG*) refer to this edition, which is a reprint of the 1897 original published by Cambridge University Press. Routledge reissued the *Essay* in 1996 with a typesetting that changed the page numbers from the original and Dover editions.

<sup>16</sup> “The Philosophy of the Pure Sciences”, *Lectures and Essays*, 1: 320.

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plain the effects of various physical forces.

It is this last point which makes Clifford's position so radical. In the most famous of Clifford's speculations on the topic, "On the Space-Theory of Matter" (1870), a paper which exists only as an abstract<sup>17</sup> and which is sometimes cited as an anticipation of Einstein's general theory of relativity, he suggests that curvature "is continually being passed from one portion of space to another after the manner of a wave", that the resulting variation in curvature is what "we call the *motion of matter*", and that "in the physical world nothing else takes place but this variation" (p. 22). He reports that he has been attempting to explain "in a general way" the laws of double refraction on this hypothesis, "but have not yet arrived at any results sufficiently decisive to be communicated" (*ibid.*).<sup>18</sup> The idea that the curvature of space may have physical causes is one that hardly figures at all in *Lectures and Essays*. But it does reappear stated quite strongly in *The Common Sense of the Exact Sciences*, albeit in passages actually written by Karl Pearson.<sup>19</sup> In particular, the chapter on position, which was entirely written by Pearson, concludes with the following paragraph:

These postulates [of geometry] are *not*, as is too often assumed, necessary and universal truths; they are merely axioms based on our experience of a certain limited region.... The danger of asserting dogmatically that an axiom based on the experience of a limited region holds universally will now be to some extent apparent to the reader. It may lead us to entirely overlook, or when suggested at once reject, a possible explanation of phenomena. The hypotheses that space is not homaloidal, and again, that its geometrical character may change with time, may or may not be destined to play a great part in the physics of the fu-

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<sup>17</sup> Clifford, *Mathematical Papers*, ed. Robert Tucker (New York: Chelsea, 1968; 1st edn., 1882), pp. 21–2. The abstract was published, originally in *Proceedings of the Cambridge Philosophical Society*, six years after the paper was given.

<sup>18</sup> Nothing seems to have survived of his efforts on double refraction. It is worth mentioning that Clifford's were not the first speculations along similar lines. Kant, as early as 1746, had speculated that the properties of space might depend upon the disposition of forces (*Gedanken von der wahren Schätzung der lebendigen Kräfte*, in *Gesammelte Schriften* [Berlin: Preussischen Akademie der Wissenschaften, 1902], 1: 1–181), and Lobachevski in 1829 had made much more sophisticated proposals for the geometrical treatment of forces (*Zwei Geometrische Abhandlungen* [Leipzig: Teubner, 1898–99], 1: 1–66).

<sup>19</sup> For further evidence that Clifford took the view seriously, see M. Chisholm, *Such Silver Currents: the Story of William and Lucy Clifford 1845–1929* (Cambridge: Lutterworth P., 2002), pp. 161, 164–5.

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ture; yet we cannot refuse to consider them as possible explanations of physical phenomena, because they may be opposed to popular dogmatic belief in the universality of certain geometrical axioms—a belief which has arisen from centuries of indiscriminating worship of the genius of Euclid. (Pp. 203–4)

Just before this, Pearson added the following, more definite note of his own:

The most notable physical quantities which vary with position and time are heat, light, and electro-magnetism. It is these which we ought peculiarly to consider when seeking for any physical changes, which may be due to changes in the curvature of space.... [I]f we *assume* as an axiom that space resists curvature with a resistance proportional to the change, we find that waves of “space-displacement” are precisely similar to those of the elastic medium [the ether] which we suppose to propagate light and heat. We also find that “space-twist” is a quantity exactly corresponding to magnetic induction, and satisfying relations similar to those which hold for the magnetic field. It is a question whether physicists might not find it simpler to assume that space is capable of a varying curvature, and of a resistance to that variation, than to suppose the existence of a subtle medium pervading an invariable homaloidal space. (*Ibid.*, p. 203n.)

It was perhaps sheer misfortune that Pearson did not include gravitation, along with heat, light and electromagnetism, as a physical phenomenon that might be susceptible of a geometrical explanation, for it was gravitation that proved susceptible to such an explanation in Einstein’s general theory of relativity.

Although Russell makes no mention of Clifford’s radical theory in his *Essay*, he was certainly cognizant of it. In an 1893 paper he wrote for James Ward’s course on metaphysics, Russell attempts to defend Kant’s view of geometry from the spectre of non-Euclidean descriptions of space. In this paper he writes, “W. K. Clifford even hints, in his wild enthusiastic way, that changes of shape such as we ascribe to changes of temperature, etc., might possibly be explicable as due to changes in the measure of curvature of space; this suggestion is of course rather preposterous ...” (*Papers* 1: 127). Russell admitted non-Euclidean spaces of constant curvature as a priori in his *Essay*, a qualified version of his complete defence of Kant in this 1893 paper. That was as far as he was willing to admit, in 1897, that new geometries had import for the science of space. We should note, however, that in holding this position, Russell

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was more radical than some of his colleagues.<sup>20</sup>

In his preface to the 1946 edition of Clifford's *Common Sense*, Russell described Clifford's insight into the relation of physics to geometry as "prophetic". He commented: "All that is said [by Clifford] on the relation of geometry to physics is entirely in harmony with Einstein's theory of gravitation, which was published thirty-six years after Clifford's death" (*Papers* II: 317). Nonetheless, the view of geometry that Clifford advocated and that underlay general relativity was one that Russell, in the *Essay*, declared to be impossible on a priori grounds. Later in life Russell acknowledged that "Einstein's revolution swept away everything at all resembling [the] point of view" of the *Essay* (*MPD*, p. 40).

## II. RUSSELL, WHITEHEAD, AND THE THEORY OF MEASUREMENT IN GEOMETRIES OF VARIABLE CURVATURE

Russell's purpose in the *Essay* was to determine, in the light of non-Euclidean geometry, which geometrical principles were a priori and which a posteriori. In formulating the principles Russell was much influenced by Helmholtz,<sup>21</sup> in particular in adopting the principle that space is homogeneous, that is, it has everywhere the same curvature. Russell argued in the *Essay* that the homogeneity was an a priori requirement of the concept of space; curvature could be positive, negative or zero (that, for Russell, was an empirical matter), but it must of necessity be everywhere the same (*EFG*, p. 149). Clifford strongly disagreed with the idea of raising the homogeneous curvature of space to the level of a philosophical postulate.<sup>22</sup> Clifford was astutely aware of the limits that confined speculation regarding the actual nature of physical space. The most apt geometrical description of space was to Clifford a strictly empirical question, to which he could not provide a certain answer, since

<sup>20</sup> In his review of the *Essay* in *Science*, Halstead praises Russell for admitting the empirical and unknown nature of the actual curvature of space, noting that other philosophers continue to subordinate non-Euclidean spaces in favour of Euclidean ones. On the other hand, Halstead was scathingly critical of certain historical inaccuracies Russell made in Chapter I. See G. B. Halstead, "The Foundations of Geometry", *Science* 6 (1897): 487–91.

<sup>21</sup> Hermann von Helmholtz, "Ueber die thatsächlichen Grundlagen der Geometrie" (1866) and "Ueber die thatsächlichen, die der Geometrie zum Grunde liegen" (1868), in his *Wissenschaftlichen Abhandlungen* (Leipzig: Barth, 1882–95), 2: 610–39.

<sup>22</sup> *The Common Sense of the Exact Sciences*, pp. 200–11.

the experience of space on a human level reveals nothing of the true character of space on scales ranging from the infinitely small to the infinitely large.<sup>23</sup> Russell, in contrast, defended the Kantian idea that certain a priori axioms of space are necessary for human experience. Such axioms are achieved through logical analysis and philosophical argumentation; once analyzed such axioms are held to be beyond empirical testability, as human experience simply could not have them any other way. It was Clifford's opinion that assertions concerning the absolute, a priori truth of a certain geometry stemmed from a kind of dogmatic thinking "rather characteristic of the mediaeval theologian than of the modern scientist".<sup>24</sup>

There may be something in the view that Russell's attachment to the a priori in geometry derived from his early, quasi-religious desire for certainty. As an adolescent, Russell began to look towards the discipline of mathematics as the area within which he might find certain knowledge: "I wanted certainty in the kind of way in which people want religious faith. I thought that certainty is more likely to be found in mathematics than elsewhere" (*PfM*, p. 53). When Russell was eighteen he rejected Christianity and became agnostic. Scholars who have studied Russell's life have noted the connection between Russell's loss of religious faith and his search for certainty in mathematics. There was indeed considerable Victorian anguish on the very point which concerns us.<sup>25</sup> Russell's biographers have documented the role his "mathematical mysticism" played in the unfolding of his early life's work (e.g. Monk); other Russell scholars have made a study of his "personal religion".<sup>26</sup> It took a long time for Russell to become comfortable in his agnosticism. Something of this can be seen in his remark, not to be taken too seriously, that Clifford's approach to geometry left "meagre support" for the "religion of our childhood" (quoted above). It can be seen also, without tongue in cheek, in the remark that immediately follows, that "the possibility of an inaccuracy so slight, that our finest instruments and our most distant

<sup>23</sup> *Ibid.*, pp. 201, 203; "Philosophy of the Pure Sciences", pp. 320–3.

<sup>24</sup> *The Common Sense of the Exact Sciences*, p. 201.

<sup>25</sup> See Imré Toth, "Gott und Geometrie: Eine viktorianische Kontroverse", in D. Henrich, ed., *Evolutionstheorie und ihre Evolution* (Schriftenreihe der Universität Regensburg, bd. 7, 1982), pp. 141–204. The issue is even mentioned by Ivan Karamazov in Dostoevsky's novel.

<sup>26</sup> S. Andersson, *In Quest of Certainty: Bertrand Russell's Search for Certainty in Religion and Mathematics up to The Principles of Mathematics (1903)* (Stockholm: Almqvist & Wiksell International, 1994).

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parallaxes show no trace of it” which Clifford’s account of geometry would leave open, “would trouble men’s minds no more than the analogous chance of inaccuracy on the law of gravitation, were it not for the philosophical import of even the slenderest possibility in this sphere” (*EFG*, p. 97). The a priori in geometry was not to be given up lightly.

Clifford, by contrast, rejoiced rather than mourned on the occasion of the discovery of non-Euclidean geometry. Clifford did not wish to have the kind of certain transcendental knowledge about the universe in all its immensity and eternity that Euclidean geometry had seemed to offer. To Clifford, such universally applicable knowledge seemed simply unattainable. Joan Richards links this aspect of Clifford’s philosophy of science with his personal agnosticism, a view that for him came very close to atheism.<sup>27</sup> Clifford, like Russell, gave up religious faith in his youth, under the influence of the debate surrounding the significance of Darwin’s *Origin of Species*. While Clifford abandoned his religious faith without regret, Russell, after giving up religion at the age of eighteen, transferred his lingering desire for religious faith into his search for certainty in mathematics.

But such social-constructivist explanations, though interesting (and sometimes plausible), can hardly give a full account of the matter. In the nature of things, they leave the *explanandum* radically underdetermined, and in an area where it might be expected to be rather closely determined. Russell, after all, had, or at least thought he had, good reasons for holding that the constant curvature of space was an a priori necessity. The most important of his reasons is the following transcendental argument from the possibility of measurement.<sup>28</sup> Russell argues, first, that all measurement depends upon the measurement of space. It is easy to confirm this in particular cases: time is measured by the apparent motion of the sun, weight by the stretching of a spring, temperature by the height of mercury in a thermometer, etc.<sup>29</sup> The claim, of course, cannot be conclusively demonstrated by citing examples alone, but it seems plausible

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<sup>27</sup> J. Richards, *Mathematical Visions: the Pursuit of Geometry in Victorian England* (San Diego: Academic P., 1988), pp. 112–13.

<sup>28</sup> All the arguments are discussed in detail in N. Griffin, *Russell’s Idealist Apprenticeship* (Oxford: Clarendon P., 1991), pp. 154–63.

<sup>29</sup> Russell cites particular instances at many places in his writings between 1893 and 1897; e.g. *Papers* 2: 124, 273–4, 295–6; *EFG*, p. 157. The general case is argued at *Papers* 2: 55–6, 78–9.

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nonetheless and Clifford seems inclined to agree with it.<sup>30</sup> Russell's second point is that in any space of constant curvature spatial measurement is always assured by means of congruence relations between spatial figures which are preserved no matter how the figures are moved through space. This, however, he maintains would not be possible in a space of variable curvature. In such a geometry, he writes, suppose that "the length of an infinitesimal arc in some standard position were  $ds$ ; then in any other position  $p$  its length would be  $ds \cdot f(p)$ , where the form of the function  $f(p)$  must be supposed known. But how are we to determine the position  $p$ ?" (*EFG*, p. 152). To do so, we require  $p$ 's coordinates, i.e., "some measurement of distance from the origin", but this will require knowing what the function  $f(p)$  is. Russell continues:

For suppose the origin to be  $O$ , and  $Op$  to be a straight line whose length is required. If we have a measuring rod with which we travel along the line and measure successive infinitesimal arcs, the measuring rod will change its size as we move, so that an arc which appears by the measure to be  $ds$  will really be  $f(s) \cdot ds$ , where  $s$  is the previously traversed distance. If, on the other hand, we move our line  $Op$  slowly through the origin, and measure each piece as it passes through, our measure, it is true, will not alter, but now we have no means of discovering the law by which any element has changed its length in coming to the origin. Hence, until we assume our function  $f(p)$ , we have no means of determining  $p$ . . . . It follows that experience can neither prove nor disprove the constancy of shapes throughout motion, since, if shapes were not constant, we should have to *assume* a law of their variation before measurement became possible, and therefore measurement could not itself reveal that variation to us. (*EFG*, pp. 152–3)

Thus Russell concludes that unless space is of constant curvature, space-measurement is impossible, and if space-measurement is impossible, no form of measurement is possible. Thus the constant curvature of space is a necessary a priori condition for the possibility of measurement.<sup>31</sup>

Now this, on the face of it, seems to us a rather strong argument and one which thus bodes ill for general relativity unless it can in some way be rebutted. And so, indeed, it seemed to Whitehead, to the point that,

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<sup>30</sup> Cf. *The Common Sense of the Exact Sciences*, pp. 90–1.

<sup>31</sup> Russell repeats the argument that distance cannot be measured in spaces of variable curvature in his 1902 article on non-Euclidean geometry for the tenth edition of the *Encyclopaedia Britannica* (cf. *Papers* 3: 492–3).

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when general relativity was proposed, he put forward an alternative theory which preserved the constant curvature of space.<sup>32</sup> Whitehead presents his theory in a Euclidean space which is pervaded by two fields representing mass impetus and electromagnetic impetus, thus preserving “the old division between physics and geometry”.<sup>33</sup> But he uses Euclidean geometry only because he thinks it provides “the simplest exposition of the facts of nature”; any geometry of constant curvature would serve his purpose equally well: “[i]t is this uniformity which is essential to my outlook” against what he refers to as the “casual heterogeneity of ... Einstein’s later theory”.<sup>34</sup>

It was the “casual heterogeneity” of space on Einstein’s theory which, Whitehead thought, would render the concept of distance meaningless:

I cannot understand what meaning can be assigned to the distance of the sun from Sirius if the very nature of space depends upon casual intervening objects which we know nothing about. Unless we start with some knowledge of a systematically related structure of space-time we are dependent upon the contingent relations of bodies which we have not examined and cannot prejudge.

(*Ibid.*, pp. 58–9)

The basis of his complaint was that distance had to be defined in terms of congruence and congruence could only be defined in spaces of constant curvature. This was essentially the same objection as Russell had brought against geometries of variable curvature twenty-five years previously.<sup>35</sup> The actual arguments which Whitehead gave in his books for

<sup>32</sup> Whitehead’s theory is presented most thoroughly in *The Principle of Relativity* (Cambridge: Cambridge U. P., 1922), where, between lectures on the philosophy of nature and mathematical notes on the theory of tensors, the physical aspects of the theory are presented with Einsteinian brevity. But the philosophical basis for it can also be found in *An Enquiry concerning the Principles of Natural Knowledge* (Cambridge: Cambridge U. P., 1925; 1st edn. 1919) and *The Concept of Nature* (Cambridge: Cambridge U. P., 1955; 1st edn. 1920), which has a user-friendly summary in Chapters 8 and 9. The basic idea is also stated very briefly at the end of *Process and Reality: an Essay in Cosmology*, corrected edn., ed. D. R. Griffin and D. W. Sherburne (New York: Free P., 1978), pp. 332–3. Russell, in his own book on relativity, *The ABC of Relativity* (London: Kegan Paul, Trench, Trubner, 1925), makes no mention of Whitehead’s dissent, nor of the underlying issue.

<sup>33</sup> *The Principle of Relativity*, p. v.

<sup>34</sup> *Ibid.* “Casual heterogeneity” was a frequent term of abuse: cf. also pp. 25, 65.

<sup>35</sup> Whitehead never refers to Russell’s parallel argument in the *Essay*. In part this was probably because Whitehead was well aware that Russell had repudiated the entire

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this conclusion are long and obscure and pervaded by the sort of terminological innovation for which he is notorious and which preclude a brief summary.<sup>36</sup> But in this case, mercifully, Whitehead provided his own summary in a newspaper article:

Now the spatial and temporal relations of event-particles to each other are expressed by the existence in space (in whatever sense that term is used) of points, straight lines, and planes. The qualitative properties and relations of these spatial elements furnish the set conditions which are a necessary prerequisite of measurement. For it must be remembered that measurement is essentially the comparison of operations which are performed under the same set [of] assigned conditions. If there is no possibility of assigned conditions applicable to different circumstances, there can be no measurement. We cannot, therefore, begin to measure in space until we have determined a non-metrical geometry and have utilized it to assign the conditions of congruence agreeing with our sensible experiences.... For this reason I doubt the possibility of measurement in space which is heterogeneous as to its properties in different parts. I do not understand how the fixed conditions for measurement are to be obtained. In other words, I do not see how there can be definite rules of congruence applicable under all circumstances. This objection does not touch the possibility of physical spaces of any uniform type, non-Euclidean or Euclidean.

philosophical position that underlay the *Essay*. But it may also have been because Russell may have got the basic idea of the dependence of space measurement on constancy of curvature from Whitehead himself when he was Whitehead's student. There seems no way to confirm this because there is no evidence to suggest when Whitehead came by the idea; it seems, however, very unlikely that it was in reaction to studying the general theory of relativity.

<sup>36</sup> Cf. *The Principle of Relativity*, Chap. 3, and *The Concept of Nature*, Chap. 6. C. D. Broad, in his obituary of Whitehead (*Mind* 57 [1948]: 143–4), complains vehemently of the extent to which Whitehead's work on relativity was ignored by the scientific community, but notes that Whitehead himself was partly to blame on account of the obscurity of his writings. Einstein himself did not ignore it, but confessed in conversation: "I simply do not understand Whitehead"—which gives some idea of the problem (F. S. C. Northrop, "Whitehead's Philosophy of Science", in P. A. Schilpp, ed., *The Philosophy of Alfred North Whitehead* [New York: Tudor, 1951; 1st edn., 1941], p. 204). By far the best account of the physics of the theory is J. L. Synge's three lectures on the theory given at the University of Maryland in 1951. To the best of our knowledge these never appeared in print, although a mimeographed typescript was released: J. L. Synge, *The Relativity Theory of A. N. Whitehead* (University of Maryland Institute for Fluid Dynamics and Applied Mathematics, 1951). Synge himself, though initially sympathetic to Whitehead's theory, abandoned it in the face of empirical evidence presented by Clark (see n. 48 below). The lectures have been published on the web by A. J. Coleman, another Whitehead defender, [http://arxiv.org/PS\\_cache/physics/pdf/0505/0505027v2.pdf](http://arxiv.org/PS_cache/physics/pdf/0505/0505027v2.pdf) (accessed 18 Sept. 2008).

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But Einstein's interpretation of his procedure postulates measurement in heterogeneous physical space, and I am very sceptical as to whether any real meaning can be attached to such a concept.<sup>37</sup>

If all this is correct, then the general theory of relativity must be conceptually confused and Russell was certainly right to ignore Clifford's geometry of curvature when he wrote the *Essay*.

Now it was a signal merit of Whitehead's theory that it had empirical consequences. Where Einstein's theory disagreed with Newton's, Whitehead's agreed with Einstein's. This covered the three main early experimental tests of general relativity: the motion of the perihelion of Mercury, the solar eclipse observations, and the gravitational red shift. But there were at least two effects on which Whitehead's theory differed from Einstein's: one concerned solar spectral lines, where Whitehead's theory predicted a specific interference between gravitational and electromagnetic effects, and the other arose from the notoriously difficult two-body problem in general relativity, where Whitehead's theory predicted a secular acceleration of the centre of mass of two bodies. Both effects were too small to permit confirmation of the theory in Whitehead's day. Interest seems to have focused on the second effect, perhaps because the lack of general (or indeed, in those days, of any) solutions to the two-body problem in general relativity was seen as a problem for the theory. Whitehead calculated the effect for the motion of the moon and tried to confirm it using astronomical tables, but without success. In 1937 Levi-Civita<sup>38</sup> published the surprising result that the same effect was predicted from general relativity and might be observed before too long in the motion of binary stars. Levi-Civita's results, however, were quickly thrown into question in papers by Robertson and Eddington and Clark, and Levi-Civita retracted his report.<sup>39</sup> The general consensus now is that

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<sup>37</sup> Whitehead, "Einstein's Theory; An Alternative Suggestion", *The Times Educational Supplement*, 12 Feb. 1920, p. 83; reprinted in Whitehead, *Essays in Science and Philosophy* (New York: Philosophical Library, 1948), pp. 241–8 (at 246–7).

<sup>38</sup> Tullio Levi-Civita, "Astronomical Consequences of the Relativistic Two-Body Problem", *American Journal of Mathematics* 59 (1937): 225–34.

<sup>39</sup> H. P. Robertson, "The Two-Body Problem in General Relativity", *Annals of Mathematics* 39 (1938): 101–4, and A. S. Eddington and G. L. Clark, "The Problem of  $n$  Bodies in General Relativity Theory", *Proceedings of the Royal Society of London*, series A, 166 (1938): 465–75. See Clark, "The Problem of Two Bodies in Whitehead's Theory", *Proceedings of the Royal Society of Edinburgh*, A, 64 (1954): 49–56. It was Clark's later paper that persuaded Synge, Whitehead's only serious defender, to abandon the theory. Cf.

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Whitehead's solution of the two-body problem is refuted by experience, a result confirmed most decisively by extremely accurate measurements of the gravitational effects of the tides.<sup>40</sup> There remain those who are unconvinced,<sup>41</sup> but it would seem reasonable to conclude that Whitehead's theory has been experimentally refuted.

This, however, by no means ends our concerns. Whitehead's alternative theory of gravitation may well be wrong, but his critique of Einstein's theory may well be correct—and, if it is, Russell's early dismissal of Clifford would be vindicated. The crucial issue for this paper is not whether Whitehead's theory is correct, but whether Einstein's theory can give a logically coherent and physically meaningful account of measurement. This is an exceedingly complicated problem, and no new steps towards its solution will be taken here. The key to the solution is the role that the velocity of light plays in Einstein's theory, a role which was explicitly denied by Whitehead. While this much is agreed on all sides, many detailed proposals end up giving circular definitions. Two that, so far as we can see, do not are given by Basri<sup>42</sup> and Graves<sup>43</sup>; both are exceptionally complex. Basri gives an ingenious operational definition of spatial interval and shows how to construct coordinate systems in arbitrary fields. This, on its own, is sufficient to reply to Whitehead's objection. Graves, more generally, shows how spatio-temporal intervals, the metric tensor, and the Riemann curvature tensor, can all be measured without circularity using only the resources of general relativity. Assum-

Synge's letter of 1961 to Whitehead's biographer, Victor Lowe, quoted in Lowe, *Alfred North Whitehead: the Man and His Work* (Baltimore: Johns Hopkins U. P., 1990), 2: 127.

<sup>40</sup> Cf. C. M. Will, "Relativistic Gravity in the Solar System II. Anisotropy in the Newtonian Gravitational Constant", *Astrophysical Journal* 169 (1971): 141–55. See also, more generally, Will, *Theory and Experiment in Gravitational Physics* (Cambridge: Cambridge U. P., 1993; 1st ed., 1981).

<sup>41</sup> D. R. Fowler, "Disconfirmation of Whitehead's Relativity Theory—A Critical Reply", *Process Studies* 4 (1974): 288–90, argues that, by using different initial conditions, the value predicted by Whitehead's theory can be reduced by a factor of 100. Even so, if we have understood the mathematics correctly (cf. Will, *Theory and Experiment*, p. 199), this still leaves the predicted value greater by a factor of 10 than the upper limit of the observed value.

<sup>42</sup> Saul A. Basri, "Operational Foundations of Einstein's General Theory of Relativity", *Reviews of Modern Physics* 37 (1965): 288–315.

<sup>43</sup> John C. Graves, *The Conceptual Foundations of Contemporary Relativity Theory* (Cambridge, Mass.: MIT P., 1971), Chap. 10. Graves' discussion is particularly useful for its criticism of earlier attempts, and (by Basri's standards) is relatively non-technical.

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ing these results to be correct, Russell was right in claiming that Einstein's revolution swept away the philosophical position he had defended in *An Essay on the Foundations of Geometry*. But whether, in 1959 in *My Philosophical Development*, he had adequate grounds for being quite so confident remains doubtful.

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