Part I

Residual Hegelianism

The Tiergarten programme

by Nicholas Griffin

I remember a cold, bright day in early spring [1895] when I walked by myself in the Tiergarten, and made projects of future work. I thought that I would write one series of books on the philosophy of the sciences from pure mathematics to physiology, and another series of books on social questions: I hoped that the two series might ultimately meet in a synthesis at once scientific and practical. My scheme was largely inspired by Hegelian ideas. Nevertheless, I have to some extent followed it in later years, as much at any rate as could have been expected.[1]

ALTHOUGH, AS RUSSELL says, much of his subsequent output can be located within this plan for two series of books, in this paper I shall be concerned only with the very earliest years of the Tiergarten programme, when the inspiration of Hegel was strongest. In these years Russell did indeed plan a comprehensively Hegelian dialectic of the sciences, complete with dialectical supersessions and culminating in a metaphysical science of the Absolute Spirit. In this paper I shall be very largely concerned with describing Russell's dialectic, indicating the results he found (or hoped to find, or hoped he had found) at the various levels of the system, and the points on which the dialectical supersessions between the levels turned.[2]

Although Russell credits Hegel with the inspiration for the Tiergarten programme, he makes little direct use of Hegel, even in the period before 1897 when Hegel's influence was at its height. In fact, the most direct philosophical influences on Russell in the period 1895–1897 were Kant (whom I shall discuss later), the

[1] *The Autobiography of Bertrand Russell*, Vol. I: *1872–1914* (London: Allen and Unwin, 1967), p. 125. See also a similar account in Russell's "My Mental Development" in P.A. Schilpp, ed., *The Philosophy of Bertrand Russell* (Evanston and Chicago: Northwestern University, 1944), p. 11.

[2] An evaluation of the system on its own terms would be a very much longer undertaking, for which see Griffin, *Russell's Idealist Apprenticeship* (Oxford U. P., forthcoming). Russell himself provided an unfair, though typically concise, evaluation from the standpoint of his later philosophy: he called his attempts at Hegelian dialectic "unmitigated rubbish" and "complete nonsense", but added that he thought they were not more misguided than Hegel's own writings (*My Philosophical Development* [London: Allen and Unwin, 1959], pp. 41, 43; henceforth cited as *MPD*).

British neo-Hegelians, Bradley and McTaggart, and Russell's teacher, James Ward. McTaggart was more explicitly Hegelian than the others, but his interpretation of Hegel was nonetheless idiosyncratic for that. For our present purposes McTaggart's allegiance to Hegel shows up most importantly in his belief that a fully developed theory of the Absolute was possible, a task to which he devoted his entire philosophical career.

By contrast, Bradley, while he thought the existence of the Absolute could be established, held that a theory of the Absolute was impossible, since any attempt to articulate such a theory would result in distortion and falsification. Central to Bradley's position was the thesis that reality was a single, relationless whole consisting entirely of spirit. This he purported to establish primarily by a series of reductio arguments designed to show that matter and relations, and all else that depended upon them, were inherently contradictory and, thus, that, whatever else the Absolute might be, it must be non-material and non-relational. While such arguments might show what the Absolute was not, they could reveal very little of its positive nature. As a result, some (probably including Russell in his neo-Hegelian days) see Bradley as a sceptic, while others see him as a mystic.

Russell, as a neo-Hegelian, was concerned, like McTaggart, with the further determination of the nature of the Absolute. Unlike McTaggart, he did not propose to tackle the Absolute head-on, starting with metaphysics and moving on, when metaphysical issues were settled, to establish the basic postulates of the various sciences in conformity with metaphysical principle. Nor did he begin where empiricists begin, with a survey of supposedly hard empirical data. Instead, he began in the middle, as it were, with particular scientific theories. These were closer to the empirical data than any grand metaphysical theory could be, but at the same time they offered, he believed, a better articulated, more fully developed and more consistent account of the world than might be suggested by a philosophical analysis of the empirical data. As a neo-Hegelian, as throughout his career, Russell thought that science was more likely to be right about the world than either common sense or philosophy. To start from metaphysical first principles would likely yield a system, like Hegel's, embarrassingly at variance with the known facts. To start with nothing more than hard data would result in the sterile scepticism which had engulfed empiricism. In choosing his starting place Russell was probably influenced by his teacher, James Ward, who chose the same place to begin and whose lectures revealed his familiarity with a wide range of scientific work.

Nonetheless, Russell did not believe that the sciences, on their own or taken collectively, would provide a comprehensive and consistent view of the world. Each science formed its own subject-matter by abstraction from the full richness of experience, but there was no guarantee that the resulting sciences could be combined consistently to provide a full account of experience. To provide such a unified account of reality required a philosophical synthesis, and it was this that Russell hoped to provide. This, in turn, required a philosophical analysis of each science. The investigation of the individual sciences would, if pressed far enough, reveal the nature of the Absolute and thereby determine (at least in part) the principles of metaphysics, which could be regarded as the general, or universal, science.

Russell, of course, did not start in complete ignorance of the sort of metaphysical conclusions he expected to arrive at. He was an idealist (*MPD*, p. 42), and expected that the dialectic would bear him out, although very little is said about this in the surviving notes from the period.[3] On this much, of course, all neo-Hegelians agreed. What divided them was the question of monism versus pluralism. The division was not quite so radical as it might sound, for the style of pluralism canvassed among neo-Hegelians, e.g. by McTaggart, envisaged the Absolute as composed of individual spirits related by internal relations. The resulting organization of spirits itself constituted a single, organically related spirit. Bradley's monism, by contrast, did away with relations altogether. Even internal relations, it was claimed, involved some degree of falsification.

On this question Russell did not follow either Bradley or McTaggart. Like Bradley, he rejected relations, maintaining that putatively relational propositions could be shown to be equivalent to propositions which asserted intrinsic properties[4] of the terms of the original proposition or of the whole which those terms composed (*Papers*, 2: 224)[5]. It is usually assumed that, since Russell rejected relations until the very end of 1898, when he was abandoning neo-Hegelianism, he must have been a monist as a neo-Hegelian. Russell, himself, in many later writings made popular the view that the issue of monism *vs.* pluralism hangs on whether relations are rejected or accepted.[6] The argument is simple: if pluralism is true, there must be a plurality of diverse items. And diversity is a relation, so pluralism requires relations.

Russell's own neo-Hegelian position, however, was rather more complicated. In *An Essay on the Foundations of Geometry*[7] he argues, on the basis of a misunderstanding of Bradley's theory of judgment, that the existence of a multiplicity of diverse things is a necessary condition for the possibility of knowledge. All knowledge, he argues, involves a recognition of identity in difference, or of diverse things in relation. Thus the alternative to pluralism is total scepticism, and Russell rejected the latter. There is no contradiction here with Russell's reductive theory of relations, for the relation is supplied by the mind, in Leibniz's phrase "a mere ideal thing, the consideration of which is nevertheless useful."[8] Moreover, the diverse things which have, as it were, to be mentally related before knowledge is possible do not

[3] Occasional remarks can be found in *The Collected Papers of Bertrand Russell*, Vol. 2: *Philosophical Papers, 1896–99*, ed. Nicholas Griffin and Albert C. Lewis (London and Boston: Unwin Hyman, forthcoming), e.g. pp. 12, 16, 18, 19, 34. Henceforth cited as "*Papers*, 2".

[4] Russell, like many philosophers of the day, used the word "adjective" to refer to what would nowadays be called properties. Before he wrote "Why Do We Regard Time, but not Space, as Necessarily a Plenum?" (*Papers*, 2: 92–7), he used "adjective" as a contrast with "subject" (thus including relations among adjectives) in that paper, and afterwards, he distinguished adjectives from both subjects and relations.

[5] Russell's well-known distinction between the grammatical and the logical form of a proposition is already implicit in this reductive account of relations. So too is the problem of how to recognize in general when we have the logical rather than the grammatical form. In the absence of a general answer the existence of paraphrase procedures will not tell us what has been reduced and what has not. In adopting the distinction Russell was clearly influenced by Bradley's *Logic* (Oxford: Clarendon Press, 1883).

[6] *Cf. The Principles of Mathematics*, 2nd ed. (London: Allen and Unwin, 1937; 1st ed.: 1903), p. 226; "The Monistic Theory of Truth", in *Philosophical Essays* (New York: Simon and Schuster, 1966; 1st ed.: 1910), pp. 144–6; *History of Western Philosophy*, new ed. (London: Allen and Unwin, 1965; 1st ed.: 1945), pp. 703–4.

[7] (New York: Dover, 1956; 1st ed.: 1897), pp. 184–6. Henceforth cited as *EFG*.

[8] Fifth letter to Clarke, §47, in L.E. Loemker, ed., *Leibniz: Philosophical Papers and Letters* (Dordrecht: Reidel, 1976), p. 704; a passage often quoted by Russell after he had rejected the position. *Cf. A Critical Exposition of the Philosophy of Leibniz*, 2nd ed. (London: Allen and Unwin, 1937; 1st ed.: 1900), pp. 12–13; *The Principles of Mathematics*, p. 222.

entail the reality of relations. For Russell as a neo-Hegelian held that diversity itself is not a relation, for any genuine relation involves a unity in diversity. Unity and diversity themselves give only half of what is required and thus cannot be genuine relations (*EFG*, p. 198). We may call them "proto-relations" since they are presupposed by all relations. Russell's neo-Hegelian reductive theory of relations would thus eliminate relations in favour of adjectives and proto-relations. Thus Russell's neo-Hegelian position is a relationless pluralism. In this also he seems to have been influenced by Ward's monadism, rather than by McTaggart's personal idealism.[9]

This explains, in a preliminary way, the aims and some of the metaphysical consequences of Russell's dialectic of the sciences. They were derived primarily from the philosophy of Bradley and Ward. The methods to be used, however, were Kantian. Russell sought to establish the à priori parts of each of the sciences by means of a two-part argument. The general approach is explained by Russell as follows:

> We may start from the existence of our science as a fact, and analyse the reasoning employed with a view to discovering the fundamental postulate on which its logical possibility depends: in this case, the postulate, and all which follows from it alone, will be à priori. *Or* we may accept the existence of the subject-matter of our science as our basis of fact, and deduce dogmatically whatever principles we can from the essential nature of this subject-matter. In this latter case, however, it is not the whole empirical nature of the subject-matter, as revealed by the subsequent researches of our science, which forms our ground; for if it were, the whole science would, of course, be à priori. Rather it is that element, in the subject-matter, which makes possible the branch of experience dealt with by the science in question.[10]

Russell applies the two methods together, so that the whole treatment is supposedly self-correcting. This is perhaps best indicated by the example of metrical geometry, where Russell's use of the technique is clearest. In the first move, various metrical geometries are analyzed to determine the basic postulates common to all of them. These, on Russell's account, were the axiom of free mobility (or congruence), the axiom of dimensions and the axiom of the straight line. These three axioms give what he calls general metrical geometry and constitute the à priori component of all metrical geometries. This result is then confirmed by the second stage of the argument, a transcendental deduction which starts from the (experiential) subject-matter of metrical geometry, namely the form of externality in so far as it

[9] McTaggart, at least in his mature philosophy, rejected any attempt to reduce relations: *cf. The Nature of Existence* (Cambridge: Cambridge U. P., 1921, 1927), Vol. 1: §83. For two brief periods Russell seems to have been tempted by Bradleian monism. In the first, late in 1894, he considered monism as a possible solution to what he calls "the fundamental difficulty of Ethics", namely that of showing how the conflicting desires of individuals can be harmonized (*The Collected Papers of Bertrand Russell*, Vol. 1: *Cambridge Essays, 1888–99*, ed. K. Blackwell *et al.* [London: Allen and Unwin, 1983], pp. 97–8). In the second, in the middle of 1897, he considers it as a possible consequence of his recent adoption of a Maxwellian, plenal theory of matter (*Papers*, 2: 21–2). On neither occasion, however, did he go so far as to adopt monism.

[10] *Papers*, 1: 291–2. The passage was incorporated into *EFG*, §7. Russell attributes both methods to Kant: the former to the *Prolegomena*, and the latter to the *Critique of Pure Reason*. The former is known as the "analytic" or "regressive" method and the latter the "synthetic" or "progressive" method. See Kemp Smith, *A Commentary on Kant's 'Critique of Pure Reason'* (London: Macmillan, 1918), pp. 44–50, for further discussion.

admits of measurement.[11] Since, Russell subsequently argues, all measurement involves a form of externality, general metrical geometry is that science which is necessary if measurement is to be possible. Obviously, both parts of the investigation, the analytic investigation from geometry to its axioms, and the synthetic investigation from the form of externality to the postulates which make it possible, are supposed to end in the same place, namely with the three axioms of general metrical geometry. Russell, in *An Essay on the Foundations of Geometry*, makes brave efforts to maintain that this is so.

The defects of Russell's approach seem to me to be those inherent in transcendental deductions of all kinds, but especially damaging to those transcendental deductions designed, as Russell's in part was, to counter scepticism. As far as the analytic part is concerned, it would seem possible in principle to establish that the axioms chosen were *sufficient* for the science in question.[12] but not that they were *necessary*. For the possibility of alternative sufficient axiomatizations cannot be ruled out. The problem is a real one for Russell, for, as is well known, his three axioms of general metrical geometry are not necessary for every metrical geometry; in particular, metrical geometries for spaces of variable curvature are possible.[13] Similar problems occur in the synthetic part of the programme, compounded there by the difficulty of knowing when the basic postulates are sufficient because of the inherently greater vagueness of the subject-matter. At least for the analytic deduction one has an articulated theory to deal with, rather than just a subject-matter.

None of what's so far been said, of course, explains why Russell felt he needed a *dialectic* of the sciences. The transcendental deductions as so far described might simply be applied to each science in isolation. It is clear from a paper of 1893 that Russell sought a system of the sciences, and that he conceived the task of producing one as the prime task of epistemology (*Papers*, 1: 121). The need for the dialectic of the sciences is twofold. First, each science is incomplete as a description of the world, leaving out, of course, all the features that are treated by other sciences. "Every Science," Russell writes, "deals necessarily with abstractions: its results must therefore be partial and one-sided expressions of the truth" (*ibid.*). The language is Bradley's, but the basic point seems undeniable. The second reason which

[11] The form of externality irrespective of measurement is the subject-matter of projective geometry—which makes the synthetic side of Russell's argument somewhat simpler for projective geometry than for metrical. Such expository advantages as this yields, however, are more than lost by difficulties in the analytic side of the deduction, in identifying the axioms of projective geometry. In metrical geometry, Russell had the help of Helmholtz's earlier analytical work. In projective geometry, however, he laments that the axioms "have, as yet, found no Riemann or Helmholtz to formulate them philosophically" (*EFG*, p. 118). Russell's own efforts along these lines in *EFG* leave much to be desired, as Poincaré, for one, complained (*cf.* H. Poincaré, "De Fondements de la Géométrie. A propos d'un livre de M. Russell", *Revue de métaphysique et de morale*, 7 [1899]: 251–79). Poincaré's criticism stirred Russell to greater efforts which brought him much closer to contemporary standards of rigour, not to mention a surprisingly formalist account of geometry (*cf. Papers*, 2: 403–8, *passim*).

[12] Although even this is suspect in the case of an empirical science for which not all the empirical data was in. An axiom set sufficient for chemistry before the discovery of radioactivity could not be regarded as sufficient afterwards. But it should, at least in principle, be possible to establish the sufficiency of an axiom set for a science at any given stage of its development, or for a particular scientific theory.

[13] Russell later admitted that the theory of relativity "swept away everything at all resembling" the point of view of *EFG*. He claims that he had "never heard of the theory of tensors" until Einstein used it (*MPD*, p. 40). But this last is an error, for one of his notebooks from the 1890's contains extensive notes on Bianchi's *Vorlesung über Differentialgeometrie* (see RA 210.006549–F1).

makes the dialectic necessary is more disputable, for Russell goes on, in typically Bradleian fashion, to claim that the incompleteness of each science involves it in contradiction. Bradley and many other of the neo-Hegelians did not distinguish clearly between contradiction and incompleteness, claiming that anything less than a fully comprehensive description of the Absolute involved contradictions. In a later piece, "Note on the Logic of the Sciences" (*Papers*, 2: 5), Russell does distinguish incompleteness and inconsistency, but he still refers to both as contradictions, and claims that both require a dialectical transition for their resolution.

A dialectical transition, of either type, was a transition to a new science, which would repair the defects of the old. It was only when metaphysics was reached that this process of successive replacement of sciences would stop. Metaphysics alone constituted "independent and self-subsistence knowledge" (*Papers*, 2: 5). The distinction between the two types of transition is not so clear in application as it seems in outline. The purpose of the analysis of each individual science, according to Russell, was to reduce to an absolute minimum the number of contradictions it contained. Having uncovered its basic postulates and concepts with this aim in view, the task was "to supply, to these postulates or ideas, such supplement as will abolish the special contradictions of the science in question, and thus pass outside to a new science, which may then be similarly treated" (*ibid.*).

It is well known that Russell embarked on the Tiergarten programme with geometry. There is not much, I think, that is to be read into this fact. The *logical* order of the dialectic was to be arithmetic first, then geometry, then physics and finally psychology and metaphysics. Moreover, Russell had already chosen the epistemology of non-Euclidean geometry for the topic of his fellowship dissertation before his walk in the Tiergarten.[14] For this paper, however, I shall follow the order in which Russell actually tackled the sciences: geometry, physics, and arithmetic (with only occasional hints towards psychology and metaphysics).

The central postulate unearthed by the analysis of geometry—projective and metrical—was the homogeneity of the space, the thesis that every part of space is intrinsically like every other. Russell took this to be equivalent to the principle of the relativity of position (i.e., the principle that the spatial position of a geometrical figure is not an intrinsic feature of the figure itself, but depends entirely on relations), and to a third principle, the passivity of space, that space itself has no causal effects. Let us take Russell at his word and identify all three principles as the *principle of homogeneity*—the identification is not assumptionless, but the assumptions involved are not those I want to consider. According to Russell the principle of homogeneity is central to both projective and general metrical geometry, while, at the same time, it is a necessary condition on every form of externality. Exactly why Russell thought this is a complicated matter which need not concern us here.[15] Nor need we consider the other principles—that space be infinitely divisible and have an integral, finite number of dimensions—to which Russell accords a similar status. In this paper I am concerned chiefly to describe the overall structure of Russell's dialectic, rather than to examine the details of its components, and from this point of view the principle of homogeneity is the best starting-point, since it generates the most conspicuous of the antinomies from which his dialectic develops. My treatment here will necessarily be illustrative rather than comprehensive.

The simplest of the antinomies which result from the principle of homogeneity seem hardly worth taking seriously, and it is only by taking into account some of the metaphysical beliefs that Russell had inherited from the neo-Hegelians that they can be seen to be genuine problems at all. For the most part, it is worth noting, these background metaphysical beliefs were left unstated by Russell until after he had abandoned neo-Hegelianism. Isolating them as the sources of the antinomies was perhaps the most important step in Russell's coming to reject neo-Hegelianism. Consider, for example, the following argument: Space is the subject-matter of geometry. But geometry reveals that space is relational. Relations, however, are unreal, and they are, in any case, not subjects. Thus geometry can only exist by falsely hypostatizing space as an object of study. What this shows, according to Russell, is that geometry cannot be a self-contained science. It exists by falsely treating space as if it were real, and it can only be legimitized by an appeal to some other science which doesn't depend upon false hypostatization.

How introducing another science might help is made clearer by the following argument, also from Russell's *Essay on the Foundations of Geometry*: Geometry treats of spaces. The principle of homogeneity shows that all things spatial are relational. But all relations need terms between which they hold. Ultimately, if an infinite regress is to be avoided, such terms must be non-relational and, thus, non-spatial. To supply such terms without being involved in antinomies one has to go outside geometry, but in the attempt to supply them itself "Geometry is compelled to hypostatize space" and spatial points (*EFG*, p. 189). Very similar antinomies arise from infinite divisibility: for measurement, spatial magnitudes or extensions must be regarded as infinitely divisible. But what is divisible must be a substantial item, whereas extensions are mere relations, and thus neither substantial nor divisible.

There is nothing very unfamiliar in all this—except perhaps the language. For the modern relationist view of space seeks to reduce talk of space to talk of relations between pieces of non-geometrical matter. It is hardly any surprise to find that a relational theory of space requires some non-spatial items between which spatial relations hold: this is part of what it means to have a relationist theory. It is a reductive theory of space and must, therefore, have something to reduce space to. But it is important to notice a less familiar neo-Hegelian sub-text in Russell's argument. In the first place, Russell's underlying view is that only the Absolute can be a truly self-subsistent item. But, secondly and more importantly, Russell's reductive theory of relations requires the elimination of spatial relations. Consequently, there is a further reason which would have led Russell to expect that a relational theory of space issued in contradictions, for the relations in themselves were falsifying and contradictory.

It is tempting to conclude, at this point, that Russell had got himself into an untenable position and had best abandon either his metaphysics or his geometry. On the one hand, his metaphysics denied relations; on the other, his analysis of geometry required them. But to argue this way is to neglect the charms of the dialectic. The contradictions which Russell claimed to have uncovered in geometry were not used by him to show that geometry was mistaken and to be replaced, but merely to show that it was not finally and completely true. Geometry did not have to be

[14] It is true that the earliest surviving document from Russell's research for his fellowship is dated "Berlin. March 1895" (see *Papers*, 1: 256), but he had been working on the topic for some months before (*Papers*, 1: 247).

[15] I give a fuller discussion of Russell's reasons in *Russell's Idealist Apprenticeship*, Ch. 4.

redone as a result of Russell's analysis, it had to be transcended in order to arrive at a fuller and more correct view of reality. At one level, geometry was perfectly all right, despite its contradictions. The contradictions arose from trying to make it do more than it conceivably could. What was needed was a dialectical transition which would resolve the contradictions and bring discussion to a new level. It is only on this second level that geometry could be considered to be in error.

The direction of travel out of geometry should be sufficiently obvious. Consider again the following antinomy induced by the axiom of free mobility. The axiom states that figures can be moved arbitrarily in space without deformation, yet since figures are individuated by their relations to points and to other figures, it makes little sense to talk of *the same figure* being moved without deformation from one place to another. So what is it that is moved? It won't do to suggest that it's an ordinary physical object, for a physical object, when moved, could well be deformed, not through the action of space, but through the action of physical forces. What seems to be called for is some notion of abstract or "geometrical" matter (*EFG*, §§71–3) which could be moved without deformation. Such matter has to be more than mere extension—for that would take us back in a circle, since extension is relational—and yet it must be less than ordinary physical matter—for that is subject to the action of forces and might violate the axiom of free mobility. Not surprisingly, therefore, Russell moves to kinematics, where questions involving forces can be avoided.

Russell defines kinematic matter in such a way as to deal directly with the geometrical antinomies. In kinematics, "Matter is that of which spatial relations are adjectives" (*Papers*, 2: 14). Kinematics does not "introduce any property of matter except that of being susceptible of varying spatial adjectives without loss of identity" (*ibid.*). Constituents of this kinematical matter "must contain no space, but be localized, by their spatial relations, as points" (*ibid.*, pp. 14–15). But if this is true, one wonders how kinematic matter can help. If the trouble with geometric points was that they can only be localized by spatial relations to other points, matters seem not have been improved by introducing points of kinematic matter which can only be localized in the same way.

In a later note Russell seems implicitly to recognize as much. There he minimizes the distinction between *metrical* geometry and kinematics. The reason is not difficult to see. General metrical geometry depends upon the axiom of free mobility, which requires that there be something movable (i.e. something beyond mere spatial points and figures). What is movable is provided by kinematical matter—"that of which spatial relations are adjectives". Thus kinematical matter is a necessary precondition of metrical geometry, and Russell admits that kinematics "introduces no new ideas" over those in metrical geometry. For

> We already have matter and motion in metrical geometry: the only difference is, that in Geometry we study only the initial and final state of moving matter, not the actual process of motion, which occupies us in Kinematics. (*Papers*, 2: 21)

The difference seems to be that kinematics requires that matter have some non-spatial quality. Thus kinematics is concerned on the one hand with describing and measuring motion, and on the other with the non-spatial qualities of matter. But, at the same time, kinematics does not itself provide the required non-spatial qualities. The need for the latter leads to the transition from kinematics to dynamics. For, since we have started in kinematics with matter and motion, the non-spatial qualities of matter must be concerned with the change of motion of other matter, i.e. with force, which is the new concept required for dynamics. In dynamics

> Matter is not only the movable, but the mover: two pieces of matter are capable of causally affecting one another in such a way as to change their spatial relations. (*Papers*, 2: 15)

The transition from kinematics to dynamics is required, Russell explains, by the law of causation (presumably an à priori principle of science). The reciprocal causal relation between two pieces of matter which tends to change their spatial relations is force.

But dynamics does not eliminate the problematic circularity either. Faced with the traditional alternatives of defining matter in terms of extension (as Descartes had done) or force (as in Leibniz), Russell felt compelled to follow Leibniz: a definition in terms of extension would have taken him back to geometry. Russell's conclusion was characteristically radical: matter was not only not extensive, it was not even extended. Thus he came to regard atoms, as Boscovich had, as unextended centres of force. The problem now is that force is itself defined in terms of the production of motion in other matter. Thus we arrive at what Russell admits to be "a mainly relative conception of matter" (*Papers*, 2: 11). This ensures that matter cannot be regarded as substantial, for substances, on Russell's view, must be entirely self-subsistent, while philosophical definition is understood to reveal the real nature or essence of what is defined (*Papers*, 2: 410). Accordingly, matter cannot be a substance if it can be defined only by means of relations. This, of course, gives no cause for alarm to an idealist like Russell, even though it leads to contradiction. Indeed, he says a relative conception of matter is "desirable" (*Papers*, 2: 11). Matter, for Russell, cannot be regarded ultimately as a substance and thus cannot be given a completely non-relational characterization. The contradictions inherent in matter result from supposing it a substance.

The introduction of force generates another of the antinomies which Russell finds crucial to the early stages of his dialectic, the antinomy of absolute motion. The antinomy of absolute motion was not an antinomy which arose within geometry. It arose because of a prima facie conflict between the relativity of space and the evidence of physical science about the nature of actual space, particularly such phenomena as Newton's bucket experiment, Foucault's pendulum, and the like,[16] which were taken to show that space was absolute and not relative. On this problem Russell writes:

> The only way of defining a position, and hence a motion, is by reference to axes, which

[16] The physical evidence is discussed most fully by Russell in "Four Notes on Dynamics" (*Papers*, 2: 30–4). The verificationist appearance of many of his arguments there is misleading. Various possibilities are ruled out not because they are unverifiable, but because they would require conceptual resources which are not available given the type of reconstruction of science which Russell is undertaking.

axes, to be perceptible,[17] and to be capable of supplying relata for spatial relations, must be material, or rather, must be generated by relations to material points. Motion can therefore only be defined by relation to matter. But it is essential to the laws of motion that this matter should have no *dynamical* (i.e. causal) relation to the matter whose motion is considered or, indeed, to *any* matter. If it *has* such a relation, the laws of motion become inapplicable, and our equations become untrue. But the laws of motion lead to Gravitation, and if this be universal, there is *no* matter without any dynamical relation to any given matter. Hence arises an antinomy: for dynamics, it is *geometrically* necessary that our axes should be material, and *dynamically* necessary that they should be immaterial. (*Papers*, 2: 16)

Russell's initial reaction to the antinomy of absolute motion was a transition from dynamics to psychology. In the first note on the problem he made two tentative suggestions for dialectical transitions. In the first he introduces the subject as a source of absolute position:

Perhaps there may be hope in restoring the preeminence of the *here*, as a source of absolute position. (*Papers*, 2: 16)

The second suggestion is even more radical:

perhaps we may replace force by conation, and pass on into psychology. (*Ibid.*)

On this supposition, material points would be replaced by Leibnizian monads, whose psychic activities provide "the other adjectives than space and force" which Russell had decided were necessary to break out of the contradictions of dynamics.

These psychological remedies were not pursued further because Russell by 1897 was coming to abandon the Boscovichian theory of point-atoms which he had previously held. This change, he explains (*MPD*, pp. 42–3), was due to the influence of Whitehead, and indirectly of Faraday and Maxwell.[18] Among its intellectual advantages, it avoided the need for action at a distance which Russell had found difficult to accept (as, indeed, had Newton). In place of the point–atom theory, Russell came to accept a plenal theory of matter. On this change he writes:

When I adopted the more modern view, I gave it a Hegelian dress, and represented it as a dialectical transition from Leibniz to Spinoza, thus permitting myself to allow what I considered the logical order to prevail over that of chronology. (*MPD*, p. 43)

The results can be seen in two notes in "Various Notes on Mathematical Philosophy" (*Papers*, 2: 21–3). The plenal theory, he hoped, would enable him to overcome the antinomy of absolute motion.

[17] Here again his argument takes on a verificationist tinge, but an unnecessary one, as the next clause reveals. R. Torretti (*Philosophy of Geometry from Riemann to Poincaré* [Dordrecht: Reidel, 1978], p. 316) finds other grounds for accusing Russell of verificationism. But the charge is unjust, Russell was not a verificitionist as a neo-Hegelian, except perhaps in the rather trivial sense in which all idealists are verificationists.

[18] Russell read the latter's *Electricity and Magnetism*, which had been the topic of Whitehead's fellowship dissertation, in July 1897. He had also read Maxwell's influential article on Atoms in the *Encyclopaedia Britannica* and his *Matter and Motion* in April 1896.

The chief difficulty with the plenal theory was that of differentiating plenal matter without individuating it into elements—a problem endemic in Bradley's philosophy where the distinction between the two was commonly smudged. What Russell wanted was a non-spatial adjective of matter, distributed heterogeneously throughout the material plenum, and invariant under motion. A tall order, as Russell conceded, and his first consideration of the theory was rather sceptical: "This view is difficult, and I doubt if it will work, but it is important to make it work if possible. It would, I think, almost necessarily imply Spinozism in Metaphysics, i.e. a denial of substantial diversity in the Absolute" (*Papers*, 2: 22).

In part, the difficulty was one of explaining what was meant by "motion" on the plenal view, for matter, being everywhere, could no longer be said to move from one place to another. Clearly an account would have to be given in terms of change with time of the spatial distribution of the heterogeneous, non-spatial adjective of matter. He tackled this problem in another 1897 note, "Motion in a Plenum", in which he tried to work out some of the physics of his new view, even though the nature of the heterogeneous adjective remained undetermined. Labelling the unknown non-spatial adjective "A", he characterizes motion as a change in the relations of this adjective to spatial adjectives. On this view, it is assumed that the degree or intensity of A is a function of spatial position. Then, if the value of this function for a given position changes with time, there is motion. "The science of motion in a plenum would be complete if we could specify the adjective A, the laws of its distribution, and the laws of its change" (*Papers*, 2: 89). After desultory efforts at dealing with density, volume, conservation laws and the equation of the continuity, Russell confesses: "I don't see how to go further without discovering what a [the intensity or degree of A] is" (*ibid.*). Indeed, he goes on to say that it is "very difficult ... to see how A can be susceptible of quantity" (*ibid.*, p. 90), the reason being, presumably, that the measurement of quantity presupposes spatial extension, yet A is supposed to be not only non-spatial, but presupposed by geometry.

He is less pessimistic, but on the physical side vaguer, in a third note on the plenal theory. In this note he proposes to consider atoms "as mere adjectives of one single substance" (*Papers*, 2: 22) and later suggests that they might be regarded "as centres of condensation, as in the world of spirit" (*ibid.*), since it is necessary to preserve in some way "a distinction between ether and gross matter" (p. 23). His main occasion for optimism is the hope that such a conception of matter will solve the problem of absolute motion "for there is now no matter except the one whole, and this is eternally[19] under no forces. But matter under no forces was precisely what we required to solve the antinomy" (*ibid.*).

Having got this far, Russell reviews his entire dialectic. Its principle, he says, "appears to lie in making the Whole gradually more explicit. Our separate particles turn out, first to be related to other particles, and then to be necessarily related to all other particles, and finally to err in being separate particles at all" (*ibid.*, p. 23). But whereas, on the punctual theory, Russell made a number of admittedly vague suggestions for passing from dynamics to psychology; on the plenal theory he admits

[19] Although "eternally" is clearly written in the manuscript, one wonders if Russell didn't intend to write "externally".

that he doesn't know how to proceed beyond dynamics (*ibid.*, p. 23). In fact, this side of his dialectic never proceeded any further.

The one topic that remains to be tackled is that of continuity, which the plenal theory of matter had brought to the fore. This was a fundamental problem for Russell, and one on which, as a neo-Hegelian, he never came to any settled views. The problem of continuity had arisen in a variety of places: in geometry, in the paradoxes of the point and infinite divisibility, in the calculus and in physics through the plenum. He approached these antinomies most often, at least in the early years of his dialectic, as forms of an antinomy between number (which was discrete) and quantity (which was continuous). Cantor's transfinite arithmetic, e.g., was seen as a doomed attempt to match number and quantity by augmenting the range of available numbers in inadmissible ways (*Papers*, 2: 37, 50–2). In this general approach he was much influenced by reading Arthur Hannequin's book on the atomic hypothesis in contemporary science early in 1896.[20] In his review of Hannequin, Russell cites approvingly Hannequin's objections to Cantor, and adds some of his own. He is also sympathetic, as can be seen also from some later notes, to Hannequin's view that the calculus treats continua by covertly supposing them discrete. "Thus," Russell concludes, "the method of [finite] indivisibles ... remains the basis of all mathematical operations with continua" (*Papers*, 2: 37).

At about the time he reviewed Hannequin Russell wrote a short note on the relation between number and (continuous) quantity (*Papers*, 2: 13). There number is said to be derived by abstraction from instances of a concept. The applications of number thus conceived are straightforward so long as they are to discrete things, where we have both a unit and a completed whole. But Russell notes, "the chief uses of number are in its application to continua", as occurs in the measurement of continuous quantity, and there we have neither unit nor completed whole.

In Russell's treatment of arithmetic at this time number, in counting, expresses a ratio between a single instance of the concept and the collection of instances being counted. Prima facie, in applying number to continua, a similar approach is suggested, except that in this case a unit is arbitrarily chosen and the number (which this time measures the continuous quantity) expresses the ratio between the unit and the quantity. The trouble is that if the unit is of finite size there is no guarantee that it will provide an exact measurement of the continuous quantity. To obtain an exact measurement, we must choose an infinitesimal unit:

> Hence arises necessity for zero and infinity, former denial of units, latter of whole in which they are collected. Neither is a number, i.e. neither contains a whole of unities. Both contradictory, but necessary results of application of number to continua. Hence atomism, to escape from continuity. Differential calculus really atomistic. (*Papers*, 2: 13)

In June 1896 Russell embarked on his first full-scale treatment of the problems of continuity in a paper, "On Some Difficulties of Continuous Quantity" (*Papers*, 2: 46–58), which he originally intended for publication in *Mind*. Despite major subsequent revisions, the paper never came to a satisfactory conclusion and Russell abandoned it, hoping that a study of Hegel might help. (It didn't.) Here Russell pursues a transcendental deduction on the nature of the continuum: what must be the nature of the continuum if number is to be applied to it? The first property it must have is homogeneity (p. 48), for otherwise there could be no units to form the basis for numeration. For if numeration is to be possible, units are required, and each unit must be qualitatively identical (in relevant respects) to each of the others. At the same time, the continuum cannot be comprised of "a series of similar objects" (p. 48), for such a series, though homogeneous, would not be continuous. The conclusion Russell comes to is not unlike that which he adopted for the plenal theory of matter, that the continuum, though homogeneous, must be capable of differentiation.

> These differentiations must serve as marks for the quantum of the continuum, which quantum itself is regarded as homogeneous throughout. The differentiations may be given, or may be intellectually constructed; but in either case they must be regarded as irrelevant to the continuum, and as neglected by the numeration which they render possible. (P. 48)

A continuous quantity can now be measured by comparison with the unit. But, since the unit is finite and the continuum infinitely divisible, it is unlikely that an arbitrarily chosen part of the continuum can be measured exactly by the unit (p. 49). Russell next considers three attempts to overcome this difficulty: the methods of indivisibles, the calculus and Cantor's set theory. The method of indivisibles he regards, not so much an account of the application of number to continuous quantity, as a rejection of continuity. In the end he regarded it as the only philosophically correct treatment. The calculus is presented as differing from the method of indivisibles only in its use of limits. This he originally rejected on the grounds that if the increments "in the last stage" were finite we were still with the method of indivisibles, whereas if the increment was zero "the limit becomes unmeaning ... the ratio of two absolute zeroes" (p. 50). He subsequently retracted this on reading de Morgan's calculus text.[21]

Russell's treatment of Cantor was neither so soon, nor so easily, put to rights. When he wrote "Some Difficulties of Continuous Quantity" his knowledge of Cantor came from Hannequin's critical account and the French translations published in a special issue of *Acta Mathematica* in 1883 of which he made a voluminous abstract (see *Papers*, 2: Appendix III.2). He was not impressed. He objected, for example, to Cantor's second number class, for reasons analogous to those he directed at the calculus. He rejected Cantor's first transfinite ordinal, ω, effectively on the grounds that admitting the existence of such a number is to assign a number larger than any assignable number. Consequently there is no foundation for Cantor's second number class, and no possible basis for transfinite arithmetic:

> For the fact that no natural number is the largest of its kind, is itself deduced from the fact

[20] See A. Hannequin, *Essai critique sur l'hypothèse des atomes dans la science contemporaine* (Paris: Masson, 1895). Russell told Philip Jourdain in a letter of 9 September 1917 that he learnt of Cantor's work for the first time from reading Hannequin. See I. Grattan-Guinness, *Dear Russell—Dear Jourdain* (London: Duckworth, 1977), pp. 143–4.

[21] A. de Morgan, *The Differential and Integral Calculus* (London: Baldwin and Craddock, 1842). Those aware of the nature of the Cambridge Mathematical Tripos in the late nineteenth century will not be surprised to learn that Russell emerged from it without knowing what a limit was. See A.C. Lewis and N. Griffin, "Russell's Mathematical Education" (forthcoming).

that the natural numbers go on forever; how, then, in the very next breath, demand a number which shall be larger than any of this endless series? When a series has no upper limit, even the mathematician will hesitate to speak of anything larger than its upper limit. (P. 52)

Russell concluded "All that Cantor has really proved ... is that legitimate numbers, once for all, remain discrete, and can never suffice to compare any two casually given parts of a continuum" (p. 52). From all this Russell concludes first, that the continuum cannot be understood mathematically, and second that it cannot be understood at all. Ultimately, he comes to a time-honoured, indeed rather hackneyed, conclusion:

The continuum as an object of thought is self-contradictory; whatever we treat as a continuum must really, if it is to be intelligible, be discrete. To regard it otherwise is to admit the truth of hopeless and irresolvable contradictions. (P. 53)

There are, however, other more fundamental sources of contradiction in the continuum than those so far mentioned. An important one (in "Some Difficulties" and a roughly contemporaneous note in "Various Notes") is the following. If a continuous quantity is a thing then it must be capable of being regarded as a whole. But it is neither a whole nor does it have parts. Attempts to find parts result in the concept of mathematical zero and attempts to find the whole in infinity. Both notions are contradictory: "the one as the quantum which contains no quantity and the other as a synthesized whole whose synthesis can never be completed" (*Papers*, 2: 57). These contradictions, he concludes, are part of the very nature of the continuum and do not arise merely from efforts to apply number to it. Since the problems stem from treating the continuum as a thing, they force one to regard it as purely adjectival or relational: "Nothing ... can be regarded as a continuous quantity, except a hypostatized relation or adjective. Now this is precisely what space is" (*Papers*, 2: 20). In fact, this is why space, alone of continuous quantities, is directly measurable (a fact also noted in "Some Difficulties"): the contradictions of continuous quantity are the same as those of hypostatized space.

"On Some Difficulties of Continuous Quantity" was, to some extent, a forerunner of "On the Relations of Number and Quantity", which was published in *Mind* in 1897. Despite the fact that Russell keeps the classification of numbers in the earlier paper, the criticism of the calculus and Cantor is removed in the later paper. It might be thought that Russell's reading of Dedekind's *Continuity and Irrational Numbers* in December 1896 and of Couturat's *De l'Infini mathématique* (of which he wrote a respectfully critical review in *Mind*) in August 1896, were responsible for this change. But it is clear from his review that reading Couturat did not change his position on continuity and infinity immediately. And the fact is that though all his voluminous writing on mathematics over the next two years, there is very little appreciation of contemporary work in analysis. Irrational numbers are still left out of his account of the number continuum in "On Quantity" of 1898, and "An Analysis of Mathematical Reasoning", written shortly afterwards, continues his tradition of muddle about the differential and infinity (*Papers*, 2: 234–8), as does the 1899–1900 draft of *The Principles of Mathematics*. In analysis, at all events, clarity did not come easily.

Many philosophers before Russell had maintained that continuous quantities (even those of finite extent) could not be composed of elements, since to ensure continuity the elements must be of infinitesimal extent and no finite number of them could be combined to constitute a finite whole. Moreover, to regard a continuous quantity as composed of infinitely many infinitesimal elements was equally illegitimate. For the combination of infinitely many elements was a process for which there could be no end. Russell's initial arguments were very much along these lines. But in work during the next couple of years, he came to lay more emphasis on a rather different antinomy of continuous quantity. This antinomy had little to do with continuity per se.[22] It is in fact a special case of what might be called a part–whole paradox. Part–whole paradoxes arise for Russell because every whole involves its parts in a relation. In the case of continuous quantity no such relation is possible. This is not because the parts are infinitely small or because there are infinitely many of them, but because they are homogeneous. The problem here stems from Russell's doctrine of internal relations, a view which he was not to subject to scrutiny until 1898. The doctrine required that every relation is grounded upon the intrinsic properties of its relata, different relata having different intrinsic properties. But, by definition, where the parts are homogeneous (as they must be in continuous quantity, or so Russell has argued) they cannot have the intrinsic properties required for grounding any relation; considered as parts of the continuum they are all intrinsically the same. But we can see now that the case of continuous quantity is merely one example of a much more general problem. No collection of elements, even where the elements are finite in size and number, can form a whole if the elements are homogeneous.[23]

In "On the Relations of Number and Quantity" Russell argues that quantity is a category of comparison (*Papers*, 2: 70). This, he thinks, will avoid the dependence of quantity on number and turn quantity into a form of comparative measure (as to greater or less) entirely independent of number. But this, in its turn, brings further contradictions of a different type to the part–whole antinomies. Every judgment of quantity is a comparative judgment, a comparison of the quantity to be measured with the unit by which it is measured. Since the unit must be homogeneous with the quantity measured, every judgment of quantity involves a comparison of two things which differ quantitatively, but differ in none of the concepts applicable to them. Yet quantity itself is conceptual (since it is not given in sense). Thus we have, as he puts it, a conception of difference without a difference of conception. That is, in a quantitative judgment of comparison between A and B, A and B do not differ in the concepts applied to either, they are conceptually identical; but at the same time we have a quantitative difference between the two, and quantity is a concept, so we do have a conception of their difference.

This seems to constitute a contradiction: between two things which are in all points con-

[22] This becomes a little clearer, though Russell still does not make it explicit, in "On the Relations of Number and Quantity" (*Papers*, 2: 70–82). Though Russell doesn't advertise the fact, the contradictions that he finds in the concept of continuous quantity in this paper stem from the nature of quantity itself. They have nothing to do with its continuity and would hold equally of discrete quantity.

[23] The argument is not original to Russell. It can be found in Bradley's *Logic*, and more fully in Bosanquet's *Logic, or the Morphology of Knowledge*, but it does not seem to have been widely recognized.

ceptually alike, there ought to be no difference, but complete and entire similarity. (*Papers*, 2: 81)

It is by no means obvious to modern eyes that there really is a contradiction here. For there would normally be taken to be no contradiction in saying of two things that had all their intrinsic properties in common, that they differed in their relations to other things. The problem for Russell, here as with the part–whole paradoxes, comes from his doctrine of internal relations. For if every relation must be grounded upon intrinsic properties of its terms, we do indeed have a genuine contradiction. If $A>B$ then the relation which A has to B is different from the relation B has to A. Accordingly there must be an intrinsic property of A upon which the first relation is grounded and a different intrinsic property of B upon which the second is grounded. But if A and B are homogeneous, as they must be if they are to be quantitatively compared, they both have exactly the same intrinisic properties.

The discovery of the antinomy of quantity was an important step in the logical development of Russell's dialectic. More important still, however, was his generalization of it the following year into what he called "the contradiction of relativity". The contradiction of relativity arises wherever there is a "difference between two terms, without a difference in the conceptions applicable to them" (*Papers*, 2: 166). This formulation will encompass the various contradictions Russell thought he had uncovered in geometry (e.g., the antinomy of the point, and the antinomy between points and lines in projective geometry), the antinomy of absolute motion in dynamics, as well as the antinomy of quantity and a whole range of similar antinomies which he uncovered in mathematics in the course of his work in 1898. In fact, he thought the contradiction of relativity so pervasive in mathematics as partially to define its subject-matter (*Papers*, 2: 166). It is easy now to see the false assumption upon which it, and Russell's entire dialectic, was based. Without the doctrine of internal relations the contradictions simply disappear. Yet this was a doctrine Russell only came to abandon at the end of 1898, after a prodigious amount of work had been done working on the assumption that the contradiction of relativity pervaded mathematics. It was only through this work, however, that Russell came to realize the mathematical importance of asymmetrical relations, which constituted the hardest case for the doctrine of internal relations to deal with. It is not surprising, therefore, that Russell should have regarded his rejection of this doctrine as the decisive reason for his break with neo-Hegelianism.[24]

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