

Part III
Philosophy, Knowledge and Mind

Russell's conception of philosophy

by John G. Slater

DURING HIS YEARS at Cambridge Russell adopted, largely on the authority of his teachers, what he understood to be the idealistic position in philosophy. He listened to lectures by G.F. Stout and J.McT.E. McTaggart and he read books, notably F.H. Bradley's *Principles of Logic* and *Appearance and Reality* and B. Bosanquet's *Logic*. In his early years he did not read Hegel himself, nor was he encouraged to do so by his teachers. He also made the acquaintance of G.E. Moore, whose experience of idealism was similar to his own. Moore became disillusioned with the answers idealists gave to philosophical questions, perhaps because his training in the classical languages led him to try to pin down the exact meaning of a passage. Whatever the reason for his questioning, he persisted in it with embarrassing consequences for the idealists.

Russell witnessed many of these exchanges. His own experience in mathematics had been similar to Moore's in philosophy. Each found their respective subjects as taught at Cambridge gravely deficient. Mathematics was a haven for fallacious arguments, and philosophy for outrageous theories. Because of his experience in the study of mathematics, Russell was prepared to appreciate, and then to emulate, Moore's attack on the fundamental positions of idealism.

The first public move in his break with idealism came on 11 December 1897. That evening he read a paper to the Apostles entitled "Seems, Madam? Nay, It Is". Russell caused this essay to be published for the first time in 1957 in *Why I am Not a Christian*, a collection of his writings on religion edited by Paul Edwards. In that book the revolutionary characteristics of the paper pass almost unnoticed, because it reads like so many of his later essays. In 1983 it was published in its proper chronological order in *Cambridge Essays, 1888-99*, the first volume of *The Collected Papers of Bertrand Russell*. Read in this new context the break it marks with his past work is plain.

In "Seems, Madam?" he attacked one of the principal contentions of idealistic philosophy as propounded by McTaggart, namely, that it gives "comfort and consolation", or, to put it differently, that it is an adequate substitute for religion. Since

he is not questioning the truth of the idealistic philosophy, but only its emotional value, he begins his attack by assuming as true the metaphysic which distinguishes appearance from a timeless and perfect Reality. He then notes that "the emotional value of a doctrine, as a comfort in adversity, appears to depend upon its prediction of the future" (1983, p. 106). But there is a future only in the world of appearances. Reality, being timeless, can have no connection with the future that it might not also have with the past. It may be that its most intimate connection with the experienced world occurred in the past. If so, it can offer no comfort in future adversity.

Idealism of this sort, he argues, embraces "a hopeless dualism" (1983, p. 107). Reality is a mere "empty abstraction" which has no relations with anything in the experienced world, where everything of interest to human beings has its place. Since there are no relations between the two realms, "we cannot find in philosophy the consolations of religion" (p. 109).

He does allow that philosophy has other consolations to offer. The first is that philosophizing is a pleasant way to spend time. The second is that philosophy itself can provide aesthetic satisfaction; reading philosophy, for instance the works of Spinoza, improves our attitude to life by putting us in a certain mood. This is due to the emotion aroused in us by what we are reading, not to our assent that what we are reading is true. "Our satisfaction, indeed, seems to be, in these moods, the exact opposite of the metaphysician's professions. It is the satisfaction of forgetting the real world and its evils, and persuading ourselves, for the moment, of the reality of a world we have ourselves created" (p. 109). He cites Bradley as one who appears to share this view. Aesthetic satisfaction, he notes, differs from religious consolation, because the latter, but not the former, requires belief in that which consoles.

To the objection that he has overlooked certain differences in our experience, that some experiences more closely approximate Reality than others, he replies that the gulf between our experience and a *timeless* Reality is unbridgeable. The only value such profound experience can have, therefore, is their emotional value: "they are at best the consolations of philosophizing, not of philosophy" (p. 110).

His general conclusion provides a hint of his future philosophical position. After pointing out that metaphysics can supply the place of religion only by "being bad metaphysics", he asks: "Why not admit that metaphysics, like science, is justified by intellectual curiosity, and ought to be guided by intellectual curiosity alone? The desire to find comfort in metaphysics has, we must all admit, produced a great deal of fallacious reasoning and intellectual dishonesty. From this, at any rate, the abandonment of religion would deliver us. And since intellectual curiosity exists in some people, it is probable that some attempts would still be made to understand the world, and it is possible that they would be freed from certain hitherto persistent fallacies" (p. 111).

Certain features of this early paper are worth our notice. In the first place, the attack he makes on one of idealism's central claims is a logical one. The belief in optimism is shown to have no ground in idealistic metaphysics. The way for showing this is prepared by some rather straightforward analysis of the meaning of such notions as "the future" and "emotional satisfaction". So far his method is not unlike that used by Bradley in the "Appearance" part of *Appearance and Reality*. Where he parts company with the idealists comes in this passage:

his optimism depends upon the denial of time, is compelled to represent the Absolute as a future state of things, as "a harmony which must some day become explicit". It would be unkind to urge this contradiction, as it is mainly McTaggart himself who has made me aware of it. But what I do wish to urge is, that any comfort which may be derived from the doctrine that Reality is timeless and eternally good, is derived only and exclusively by means of this contradiction. (P. 107)

For Russell the discovery of the contradiction is grounds for abandoning the position, whereas for McTaggart it is not. Russell's failure to search for a saving synthesis in this predicament shows he is not using "contradiction" in the Hegelian sense, as meaning its parts are partly compatible and partly incompatible, but rather in the mathematical sense, as meaning its parts are incompatible with one another without qualification. No trace of the Hegelian logic is to be found in this essay.

A second noteworthy feature of the paper concerns its style. Its language, and indeed its whole tone, differs markedly from those writings, passages from which are printed in *My Philosophical Development* (1959, pp. 40-1, 43-53), in which he is attempting to philosophize in the neo-Hegelian mode. With them the reader is left with the definite impression of someone writing in accordance with a set of rules. In "Seems, Madam?" no such impression is given. Rather the picture gathered is of someone exposing, in quite ordinary language, the mistakes he has found in idealistic metaphysics. The tone is confident, and there are even witty sallies of the sort which make his later writings a delight to read. Idealistic metaphysicians, he remarks at one point, should adopt as their slogan, "God's in his heaven, all's wrong with the world" (p. 106). In later years Russell wrote a good deal according to formulae, all of his fiction and his auto-obituary in the style of *The Times* are examples. All of it (and I include here his early idealist writings) with the single exception of the obituary is disappointing. Mimicking the writing styles of others was not something he could do gracefully.

A third feature of the paper is the role accorded science. Both early and late there are important references to science. At the beginning he allows to philosophy "the few exceptional questions" that science has, so far, not been able to answer. Since he has already mentioned that philosophy has had to abandon its claims to offer "comfort in adversity, explanation in intellectual difficulty, and guidance in moral perplexity" (p. 106), the remark about exceptional questions suggests he had already arrived at the view that philosophy is pre-scientific inquiry into questions not yet having definite answers. Philosophy, then, will continue to shrink as science grows. At the end of the essay he states explicitly what is only implied earlier, namely, that both philosophy and science are, or in philosophy's case ought to be, the products of intellectual curiosity. Later he made this point by saying that both aimed at discovering truth, or, more provocatively, that "science is what we know and philosophy is what we don't know" (1960, p. 11).

The last feature I want to mention is what he has to say, by implication, about the motivation of philosophers. To pursue the study of philosophy out of a desire to find a substitute for religion is bound to end in bad metaphysics. By contrast, a metaphysic which results from a pure desire to understand the world is almost certain to be better. In later writings he often returned to the questions of motivation in philosophy. Sometimes, as in "Mysticism and Logic", the rival motivations are

So much, indeed, is the future bound up with optimism, that McTaggart himself, while all

mysticism and science, with the very greatest philosophers being driven by both (1918, p. 1); at other times, as in "On Scientific Method in Philosophy", the rival impulses are religion and ethics on the one hand and science on the other, with a small group being moved by both impulses (1918, p. 97); at still other times, as in *A History of Western Philosophy*, the rival inspirations are mathematics and empirical science, with the philosophers of logical analysis, of whom he considers himself one, moved by both, which, to his way of thinking, is a definite mark in their favour (1945, pp. 828–9; 1946, p. 857).

In drawing attention to these features of "Seems, Madam?" I wish it understood that I am not claiming that the little essay represents Russell's mature conception of philosophy. But I do think I am right in suggesting that there is much in it that is important to his developed conception and nothing that is incompatible with it.

Three years after "Seems, Madam?" he published *A Critical Exposition of the Philosophy of Leibniz*, the second chapter of which opens with this pronouncement: "That all sound philosophy should begin with an analysis of propositions, is a truth too evident, perhaps, to demand a proof. That Leibniz's philosophy began with such an analysis, is less evident, but seems no less true" (1900, p. 8). At the time this passage was written the vast majority of working philosophers would not have agreed that the proposition was quite as evident as he claims it to be, but there were a few, Moore was one, who would have. What he means by "an analysis of propositions" is clarified by the subsequent discussion. Analysis has as its ultimate goal the sorting of propositions into classes: the class of existential propositions, the class of subject–predicate propositions, and so on. These classifications are determined by the various forms that the propositions exhibit, forms that are, in some cases, exposed only after typical examples have been analyzed. The forms to be exposed are the logical forms of the propositions, so all sound philosophy should begin with a study of the logical forms of propositions. The classification itself will raise questions about the logical relations holding between the various classes of propositions. In his book, to take an example, he discusses the question whether all propositions can be reduced to subject–predicate propositions. Further analysis is required by any attempt to answer such questions. The search for counter-examples demands considerable analytical skill.

Since analysis is undertaken in his book on Leibniz's philosophy for purposes of classification only, the ancient and vexatious charge that analysis leads to falsification does not arise. By 1903, when *The Principles of Mathematics* was published, he had expanded his use of analysis to include the replacement of a complex by its constituents as they are revealed by analysis, and the question of falsification does bother him. He offers an answer to it:

I have already touched on a very important logical doctrine, which the theory of whole and part brings into prominence—I mean the doctrine that analysis is falsification. Whatever can be analyzed is a whole, and we have already seen that analysis of wholes is in some measure falsification. But it is important to realize the very narrow limits of this doctrine. We cannot conclude that the parts of a whole are not really its parts, nor that the parts are not presupposed in the whole in a sense in which the whole is not presupposed in the parts, nor yet that the logically prior is not usually simpler than the subsequent. In short, though analysis gives us the truth, and nothing but the truth, yet it can never give us the whole

truth. This is the only sense in which the doctrine is to be accepted. In any wider sense, it becomes merely a cloak for laziness, by giving an excuse to those who dislike the labour of analysis. (1903, p. 141)

When he returns to the question later in the book he maintains that falsification is only a problem "when what is to be analyzed is a unity." "A proposition has a certain indefinable unity," he goes on, "in virtue of which it is an assertion; and this is so completely lost by analysis that no enumeration of constituents will restore it, even though itself be mentioned as a constituent" (1903, pp. 466–7). He allows that this difficulty is a grave one, but since only propositions and propositional complexes are unities, the problem does not arise with regard to existent things. In his later work he is not troubled by this problem because relations assume the role of unifiers in propositions.

As the passages cited from these two early books show, Russell was concerned about questions of philosophical method from the start of his career. There is a further piece of evidence for this assertion. On 7 February 1903 he wrote in his diary: "I am planning a work on the aim and scope of Philosophy" (1985, p. 19). It was not, however, until nearly a decade later that he formulated his method, and, when he did, it was in response to teaching pressures. In October 1910 he began teaching at Trinity College, Cambridge. His principal reason for accepting the lectureship was to stimulate interest in his work among the young; he wanted to motivate the best of them to carry on the work he had begun.

In a letter to Lady Ottoline Morrell, written during his second year of teaching and a few days after he had written "The Philosophy of Bergson" in March of 1912, he told her of a visit from his pupil, George Geach (the father, by the way, of Peter Geach):

I began to talk about how philosophy should be studied—how people ought to have more of the scientific impulse for collecting queer facts, less fear of spending their time on matters not dignified in themselves but important for their consequences, as the man of science does with his test-tubes; how the love of system, since new facts are the enemies of systems, has to be kept rigidly in check, in spite of being a thing every philosopher ought to have; how vital it is to avoid unction and edification and the wish to be literary. Some day I must write on how to study philosophy; I have a lot to say about it. There is so much to be found out by patience and a scientific spirit. Writing on Bergson has filled me with ideas about philosophy in general. Geach asked me if I thought him too much of a disciple, so I said I did, and that led me on to say how I didn't want to teach a doctrine, but a spirit, an attitude to philosophy. I do care *enormously* about that. (#373, n.d.)

By the end of the summer of 1912 he was under pressure from Wittgenstein to alter his lectures: "talking with Wittgenstein has made me feel that I must alter my lectures and put more into them. Hitherto they have contained chiefly what I have published but I must put in more general remarks on method, and on what is and what is not possible in philosophy" (#569, n.d.). Four days later he had half decided to write his presidential address for the Aristotelian Society on this topic rather than on causality.

I can't see how to do a paper on Cause, and I have half decided to do one instead "on the

scope and method of philosophy", which would be *very* controversial, a battle-cry, with *all* existing philosophers against me. If I can see clearly what to say I will do that—I should say that philosophy can't deal with any questions of human interest, but reduces itself to logic, but in that sphere *can* attain real knowledge; also that most of the great systems have depended on elementary blunders in logic. What I have to say on causation could come in as an illustration. (#574, n.d.)

As it turned out he did write his presidential address on causality, but two years later he developed all of these thoughts in "On Scientific Method in Philosophy", written for delivery to the Oxford philosophers during the first months of the war. Its tone, he confessed in a letter to Lucy Donnelly (14 Dec. 1914), was deliberately calculated to infuriate his audience. He singled out this passage as an instance: "Organic life, we are told, has developed gradually from the protozoon to the philosopher, and this development, we are assured, is indubitably an advance. Unfortunately it is the philosopher, not the protozoon, who gives us this assurance, and we can have no security that the impartial outsider would agree with the philosopher's self-complacent assumption" (1918, p. 106). This passage is followed immediately by the well-known one concerning the Grand Augur and the pigs.

Before turning to a discussion of "On Scientific Method" I want to return to the years during which *Principia Mathematica* was brought to a successful end, for I think there can be little doubt that his mature conception of philosophy derived in large part from his reflections on his work on "the big book", as he referred to it in his correspondence. Several features of it impressed him as important for any philosophical enterprise.

One such feature was the collaborative nature of the work. The principal effort had come from Whitehead and him, but many others had contributed bits and pieces which found places in the finished work. Intense collaborative work of the sort he and Whitehead had engaged in for a decade was unheard of in philosophy, but not uncommon in science. In this respect, then, *Principia* was a typically scientific, rather than a typically philosophical, enterprise. Still it was, at least in its early parts, undeniably philosophical. This suggested that what was common to both science and philosophy deserved more emphasis than had traditionally been accorded it.

A second feature of the work which impressed him was the piecemeal nature of much of it. It was true, of course, that the overall structure was pretty well known almost from the start of the collaboration, through the preliminary work Whitehead and he and others had done. But within the grand architectonic there were many problems of limited scope which required solution. A famous problem of this sort was the problem of how to incorporate propositions with definite descriptions as apparent constituents into the developing notation. His reflections on the way he had solved that problem led him to the conclusion that his method, which he had hit upon under pressure to discover a solution to this difficulty, did not differ in any significant way from the method a scientist used to discover an hypothesis to explain his puzzling data.

Russell began his work on descriptions by immersing himself in the problem. This took the form of collecting a set of examples, which, together, indicated the scope of the problem. These were the "queer facts" he told Geach about. At the

same time he examined the writings of others for clues as to the solution of the problem. The importance of this step is both positive and negative: positive, because some writer may have solved the problem or gone some way towards its solution; and negative, because one learns of blind alleys and wrong turnings one might be tempted to take oneself. A further reason for studying other authors is that it keeps the problem before the mind and thus facilitates thinking. At a certain point, however, he found that conscious thought had to cease. In his letters to Lady Ottoline he stated that this step was always necessary and that reading "endless silly novels" (#707, pmk. 23 Feb. 1913) was his favoured way of passing the time while his ideas were incubating. If and when a solution came, it came all at once. Then all he had to do was to write it down. At such times, he became, he told her, "just a very competent machine—the points that turn up as I write I see how to deal with, by a kind of easy instinct—it really means being tremendously strung up, but it feels oddly easy" (#781, pmk. 20 May 1913). This letter was written during the time he was producing ten manuscript pages a day of *Theory of Knowledge*, which was first published in its entirety in 1984 as Volume 7 of his *Collected Papers*.

A third feature of *Principia* which impressed him was the place of logic in it. The first half of the first volume develops purely logical principles, "generally statements which can be made concerning everything without mentioning any one thing or predicate or relation" (1918, p. 112). These purely formal principles hold regardless of the subject-matter to which they are applied. In *Principia* itself they serve as the foundation for mathematics, but they do not belong to mathematics. Every enterprise which aims at discovering what is true must use some selection of these principles. The material under study will provide content for the principles. Because of their utter basicness he was convinced they provided a starting-point for any philosophical inquiry.

A fourth feature of *Principia* which he thought important was the tiny number of undefined terms Whitehead and he had to introduce in their development of the system. In the propositional calculus, for instance, by taking the tilde (for "not") and the wedge (for inclusive "or") they had been able to define all the other connectives. Defined terms are introduced to keep axiom systems manageable. Without them formulae would tend to grow in length until they would defy comprehension. In his later work Russell introduced the name "minimum vocabularies" to refer to the set of undefined terms in a science. A minimum vocabulary is "one in which no word can be defined in terms of the others" (1944, p. 14). Given a minimum vocabulary for a science, all of the propositions of that science are expressible in terms of it and logical terminology. It should be noted that minimum vocabularies are selected with an intended interpretation in mind, but they are, of course, open to any other interpretation that makes all of the primitive propositions true simultaneously.

Lastly, he was greatly impressed by the precision of *Principia's* language. Precision is important as a cure for vagueness. Vague concepts are undesirable because they allow of borderline cases which may or may not fall under them. When language is vague, neither the speaker nor the hearer can be sure of what is being said. Russell thought most philosophers welcomed the vagueness of natural language because it yields what they believed were philosophical riches without demanding that much thinking go into their extraction. His contempt for such philosophers was sharp.

I have just read an American first book of metaphysics, by one of the six realists [A *First Book in Metaphysics* by Walter T. Marvin]—a sort of expansion of my shilling shocker [*The Problems of Philosophy*]—references to me on almost every other page—but it is a despairingly feeble book. Philosophical capacity is astonishingly rare—people are content with soft ideas, and don't exact sharp ideas that cut like diamonds. Soft ideas are disgusting to one's taste. It is a quite recent experience to me to see the same feebleness associated with my own views that I am accustomed to see associated with other people's—it is far from agreeable. But I don't wonder—it requires a great tension of mind and a constant stringing up of one's faculties to keep one's ideas sharpened. And in philosophy vague ideas are outwardly much more potent and fruitful than exact ones. I believe a certain sort of mathematicians have far more philosophical capacity than most of the people who take up philosophy. Hitherto the people attracted by philosophy have been mostly those who loved the big generalizations, which are all wrong, so that few people with exact minds have taken up the subject. (#663, 29 Dec. 1912)

On another occasion he had spent two hours arguing with another American realist, William Pepperell Montague, which led to this report: "All the philosophers I meet make me feel how very bad the education of philosophers is—they are mostly unaccustomed to things where one can be definitely right or wrong, and therefore precision is not part of their ideal. Making machines, like Wittgenstein, is a much better training—if the machine won't work, it is no use appealing to the reason against the understanding, or to the nobler parts of our nature, etc. etc." (#608, n.d.). The American realists had a way of focusing his attention on their lack of precision. They consciously tried to be precise, but, in his opinion, their writings failed "in the final edge of accuracy on which beauty depends in thinking". And he went on:

It is lonely work starting a new line of thought; mathematicians can't understand because they don't know philosophy, and philosophers because they don't know mathematics. The result is that, except in *Principia Mathematica* which they don't read, I always have to talk a kind of baby-talk, like explaining things to a conceited child that thinks it knows everything already. The *precise* phrases that occur to me would be unintelligible, and I have to get more or less vague phrases that come somewhere near what I mean. (#591, 28 Sept. 1912)

To rid philosophy of this blight he dreamed of founding at Cambridge a school of mathematical philosophers who would take up and carry on the work he and Whitehead had started in *Principia*, but the outbreak of war killed that dream.

Reflections of this sort went into "On Scientific Method in Philosophy". In it he explicitly adopts the method of analysis as the right method in philosophy. By applying it to problems of limited scope, hypotheses can be discovered which will, after further testing and refinement, provide the best available solutions to the problems. He lays it down that every such hypothesis must be both general and *à priori*. By "general" he means "applicable to everything that exists or may exist" (1918, p. 110); and by "*à priori*", he means "can be neither proved nor disproved by empirical evidence" (1918, p. 111). "Philosophy", he says, "is the science of the possible" and is "indistinguishable from logic as that word has now come to be used."

The tools which the analytical philosopher uses are those of mathematical logic. As already noticed many of these principles are purely logical, but many others are both logical and mathematical. Both sorts of principles are available for use on philosophical problems, since both sorts are general and *à priori*. But because of the nature of philosophical problems, the philosopher will find the purely logical principles to be of most use. A very large number of these principles are stated in *Principia*, but the working analytical philosopher will find that others are needed. In a report to Lady Ottoline on his own work he noted this fact:

... the sort of thing that interests me now is this: Some of our knowledge comes from sense, some comes otherwise; what comes otherwise is called "*à priori*". Most actual knowledge is a mixture of both. The analysis of a piece of actual knowledge into pure sense and pure *à priori* is often very difficult, but almost always very important: the pure *à priori*, like the pure metal, is infinitely more potent and beautiful than the ore from which it was extracted. As regards the *mathematical* element in science, *Principia Mathematica* does the extraction very elaborately. But there are a number of other more elusive *à priori* elements in knowledge—such problems as causality and matter involve them. It is these that I want to get hold of now—I am only quite at the beginning—it is a vast problem of analysis, wanting tools that one has to make oneself before getting to work. It is very hard, to begin with, to make out what science really asserts—for example, what the law of gravitation means. Neither science nor philosophy helps one here—mathematical logic is the only help. And when one thinks one has found out what it asserts, one can't state the result so as to be intelligible to anyone who doesn't know mathematical logic. So one's audience must be small! (#616, n.d.)

The message is clear: the analytical philosopher must be more than merely highly trained in mathematical logic, he or she must also have enough genius to develop logic itself when an extension of it is required to solve a philosophical problem. It is little wonder that of his pre-war pupils at Cambridge Wittgenstein was the only one who Russell thought showed any promise.

His conception of philosophy, he was the first to admit, rather drastically reduces the scope of the philosopher's concerns. It excludes all speculation about the universe, because "the universe" is not a legitimate subject of any proposition. His reason for saying this stems from his requirement that philosophical propositions be general. General propositions refer, distributively, to everything that exists or may exist. "Everything" here is to be taken literally and exhaustively; there is not, in addition, the collection of all things about which fresh predicates can be discovered. A consequence of excluding "the universe" as a subject of propositions is to banish from philosophical concern questions about pessimism and optimism, which were central both to neo-Hegelianism and evolutionary philosophy.

Ethics and politics, both considered branches of philosophy since ancient times, also fall outside the pale. He has two, related reasons for their exclusion. The first is that ethical and political notions are "essentially anthropocentric"; they attempt "to legislate for the universe on the basis of the present desires of men" (1918, p. 107). To allow them a place in philosophy is almost certainly to bias our collection of facts and thus taint the process of inquiry from the start. The second reason is that in ethics

... it is impossible to produce conclusive intellectual arguments. When two people differ about (say) the nature of matter, it should be possible to prove either that one is right and the other wrong, or that both are wrong, or that there are insufficient grounds to warrant an opinion. In a fundamental question of ethics I do not think a theoretical argument is possible. (1944, pp. 719–20)

This state of affairs obtains because “ultimate ethical valuations are subjective” (*ibid.*). Ethical (and political) judgments fail to be either general or *a priori*.

Excluded too are all philosophies, such as evolutionism, which are based on the results, rather than the method, of science. Science is a self-correcting enterprise, so all of its results, at whatever level of generality, are subject to revision as new evidence comes to light. Generalizations on scientific results must be made at some particular time; the results at that time are accepted by the philosopher and a vast superstructure erected upon them. The danger is that the results, which, because they are the most general are also the least probable, will soon require correction, leaving the philosopher's theories without a base. His favourite example of this sort of doomed enterprise is the evolutionary philosophy of Herbert Spencer.

With so much banished what remains for the philosopher to do? Obviously any and all logical questions are included in his conception of philosophy. Logic has two great tasks. The first is to discover as many purely formal principles as possible. This work has been greatly advanced by *Principia*, but, as we have seen, it does not list them all. The second concern of the logician is “with the analysis and enumeration of logical forms, i.e. with the kinds of propositions that may occur, with the various types of facts, and with the classification of the constituents of facts. In this way logic provides an inventory of possibilities, a repertory of abstractly tenable hypotheses” (1918, p. 112). This second task is reminiscent of the sort of analysis carried out in Russell's book on Leibniz. In both *Theory of Knowledge* and his 1918 lectures on “The Philosophy of Logical Atomism” he made valiant attempts to carry out his programme.

Nearly all of epistemology remains within philosophy. Even before he explicitly formulated his conception of philosophy he noticed one feature of the study of human knowledge which caused him some discomfort. On 13 December 1911, when *The Problems of Philosophy* was passing through the press, he wrote Lady Ottoline:

There is one great question: Can human beings *know* anything, and if so what and how? This question is really the most essentially philosophical of all questions. But ultimately one has to come down to a sheer assertion that one does know this or that, e.g. one's own existence—and then one can ask why one knows it, and whether anything else fulfills the same conditions. (#286)

For the rest of his life, whenever he undertook epistemological inquiries, he was to confront this problem of how to deal with subjectivity at the heart of epistemology. In his autobiography for the Schilpp volume he draws attention to his failure to find a way round it.

Theory of knowledge, with which I have been largely concerned, has a certain essential

subjectivity; it asks “how do I know what I know?” and starts inevitably from personal experience. Its data are egocentric, and so are the earlier stages of its argumentation. I have not, so far, got beyond the earlier stages, and have therefore seemed more subjective in outlook than in fact I am. (1944, p. 16)

In *Human Knowledge, Its Scope and Limits*, his last philosophical book, he returned to the question. “Can two persons experience the same ‘this’, and if so, in what circumstances? I do not think this question can be decided by logical considerations: *a priori* either answer would be possible. But taking the question empirically, it has an answer.” The empirical answer he proposes is that, by using scientific concepts, two people can arrive at interpretations of a given sentence which “are nearly certain to be both true or both false” (1948, p. 108; 1948a, p. 92). But regarding empirical knowledge we can never free ourselves entirely from our senses. The epistemology of empirical knowledge does not, therefore, quite measure up to his standard.

Finally, many metaphysical questions fall under his conception of philosophy. The nature of matter and mind occupied his attention, off and on, for nearly twenty years. His metaphysical work, like his epistemological and logical work, consisted in searches for minimum vocabularies which, with the aid of logical principles, could be used to develop logical constructions with all the essential properties of the inferred entities for which they are to be substituted. *Our Knowledge of the External World*, *The Analysis of Mind*, and *The Analysis of Matter* are all, in part, metaphysical works. The titles of the last two underscore his commitment to his philosophical method.

In “On Scientific Method in Philosophy” he does not offer an analysis of “analysis” itself, but in *Theory of Knowledge* he does. The question arises in the context of attempting “to find some not definitely refutable theory of what is meant by ‘understanding a proposition’” (1984, p. 119). He feels obliged to begin by commenting upon the emotional associations of both “analysis” and “synthesis”.

Most of us have been told in youth that analysis is easy and base, whereas synthesis is glorious and difficult. Some of us may have felt inclined to reverse that judgment; but however that may be, it is only by analysis that we can hope to discover what analysis and synthesis are, and therefore only the humble analyst can know in what the glories of synthesis consist. (P. 119)

He proceeds immediately to offer preliminary definitions of both terms, but I will report only that for “analysis”.

Analysis may be defined as the discovery of the constituents and the manner of combination of a given complex. The complex is to be one with which we are acquainted; the analysis is *complete* when we become acquainted with all the constituents and with their manner of combination, and know that there are no more constituents and that is their manner of combination. (P. 119)

The remainder of the chapter is devoted to a search for a tenable theory of analysis. His argument is complex and defies short summary, but the theory he comes to favour can be understood without its preliminaries.

It would seem, therefore, that, throughout the process of analysis, we are acquainted with the complex and with its constituents, and that what changes during the process is only the direction of our attention.... At first, our attention is directed to the complex; then it passes to the *terms* which are constituents; before the analysis is complete it must pass also to the *relation* which is a constituent. (P. 127)

Although this process is necessary for analysis, it is not sufficient, because we might attend to the terms and the relation of a complex with which we are acquainted without drawing the conclusion that "this complex consists of these terms so related" (p. 127). He is not sure what would make for sufficiency, but suggests that it requires a shifting of attention from constituents to complex and back again, such shifting of attention being continued until we can produce an expression of the form "*aRb*" in which the terms and the relation comprising the complex are named. Shifting attention from the complex to its constituents, he warns us, "is sometimes a very difficult feat, and in certain cases is one which seems never to have been performed by human beings" (p. 128). When we have succeeded in giving our complex the complex name "*aRb*" nearly all the work of analysis has been done, but certain difficult questions remain which he does not know how to answer (*ibid.*), so the theory is not complete.

Two principles often mentioned by him in his methodological writings should be briefly considered. The first is Occam's Razor, which he states as "entities are not to be multiplied without necessity" (1919, p. 184). He seems always to have interpreted this as applying to kinds of entities. Two requirements of his method, minimum vocabularies and the substitution of logical constructions for inferred entities, seem to achieve the sort of parsimony demanded by Occam's Razor. The other principle concerns the necessity of "a robust sense of reality" (p. 170) for good work in logic. "Logic, I should maintain," he said in a famous passage, "must no more admit a unicorn than zoology can; for logic is concerned with the real world just as truly as zoology, though with its more abstract and general features" (p. 169). This principle serves, in his method, to exclude certain proposed hypotheses from serious consideration. His insistence on it emphasizes once again the similarity he finds between science and philosophy.

It is time now to gather up some threads. The essential characteristics of his conception of philosophy are fixed by a commitment to method. This method resembles scientific method closely enough to warrant the same name. There is, first, the apprehension of a problem, which, when definite enough, leads to a search for data apparently relevant to its solution. These data are then given rigorous logical examination; the analyst will use all the tools at his command to discover a relative ranking of the data according to their resistance to doubt, and to begin the process of fashioning a minimum vocabulary adequate to the problem area. The analysis, when successful, will reveal the logical atoms out of which more complicated structures may be built.

At the start, the problem will be stated in terms of inferred entities. As analysis proceeds, more and more of these inferred entities will be replaced by logical constructions which have all the logically important properties of the inferred entities for which they are substitutions. Ideally, he would like to see all inferred entities replaced by logical constructions. His "supreme maxim of scientific philosophizing"

is: "Wherever possible logical constructions are to be substituted for inferred entities" (1918, p. 155). The greater the number of such substitutions the more precise the language becomes. The data may be vague, but constructions out of such data will be precise. "Since all data suffer from a lack of mathematical precision through being of a certain size and somewhat vague in outline, it is plain that if such a notion as that of a point is to find any application to empirical material, the point must be neither a datum nor a hypothetical addition to data, but a construction by means of data with their hypothetical additions" (pp. 116ff.). Whitehead, he goes on to report, has invented a definition of "point" out of sense-data which has all the properties mathematicians expect a point to have. Russell gives a more elaborate account of the logical construction of a point in both *Our Knowledge of the External World* (1914, pp. 114ff.) and *The Analysis of Matter* (1927, pp. 290ff.).

Logical constructions are introduced into a system by explicit definition. As I mentioned earlier, one important function they serve is to keep the system manageable. An example is the definition of the cardinal number one as the class of all unit classes (1910, p. 363). One could continue the system of *Principia* by always using the notation for "the class of all unit classes" in place of "1", but it would quickly prove unmanageable if *no* definitions were permitted. Since logical constructions are essentially notational devices, it is easily seen why Russell often called them logical fictions. Calling them fictions serves as a reminder that they are "not ultimate realities" (1925, p. 17).

The discovery of useful logical constructions employs a procedure akin to the testing of hypotheses in science. Various possibilities suggest themselves to the mind prepared by study to receive them. Most of them fail when tested against the data. The Bertrand Russell Archives contain many working papers in which Russell, thinking intensely about some problem, runs through one solution after another in quest of one that works. In matters as complex as these, it is understandable that someone else reviewing his work might find a more elegant solution to the problem than he did. The tests he considered most important, he tells us in *The Analysis of Matter*, are "the usual scientific grounds of economy and comprehensiveness of theoretical explanation" (1927, p. 10).

Viewed from our place in history it seems that his espousal of neutral monism follows almost inevitably from his philosophical method. To show why I think this is true I want to return to the matter of minimum vocabularies. By the time he completed his work on *Principia* he was committed to tackling philosophical problems by attempting to find a set of axioms which, when suitably interpreted, yields a theoretical explanation of the problem in question. He did not usually speak of axioms, preferring the language of minimum vocabularies. A minimum vocabulary, you will recall, has two defining features. It is a set of basic words for a science such that (1) no basic word can be defined in terms of the others, and (2) every other word in the science can be defined in terms of the basic words (1948, p. 259; 1948a, p. 243). In philosophy he preferred "minimum vocabulary" to "axiom system" because it allowed for cases where success was only partial. The model of complete success remained, of course, *Principia*. Once a minimum vocabulary was to hand all other words of the science in question received definitions ultimately stateable in terms of the minimum vocabulary and logical words alone, but, while inquiry was proceeding, there would inevitably be words which, for a time, resisted such definition. These words would have to be included in the vocabulary of the science,

but one could still speak of the minimum vocabulary at that time. Under these circumstances the minimum vocabulary for the science would include all initial or undefined words and all those words still requiring definitions. He provides many examples of this sort of minimum vocabulary in *Human Knowledge* (1948, pp. 259ff.; 1948a, p. 243ff.)

For every developed body of knowledge it is possible to discover a minimum vocabulary. Once a minimum vocabulary for a science is known, it may prove possible to define its basic vocabulary in terms of the minimum vocabulary of another science, thus showing that the one science is merely a branch of the other. Russell believed *Principia* had done this for arithmetic by defining "zero", "number" and "successor", the primitive terms of the Peano postulates, in purely logical terminology. One long-range aim of his method, then, is to effect reductions of one science to another.

Philosophy, for Russell, is the science of the à priori elements in our knowledge. We claim to have knowledge both of the world and of our own inner states. Both physics and psychology can be studied by the philosopher with the aim of discovering their minimum vocabularies. But though they are distinct sciences there is obviously an intimate connection between them. On the one hand, our evidence for the truth of physics comes through our senses; and, on the other hand, the workings of our senses are to be explained by their physiology, which for him is a branch of physics. William James had proposed that physical objects and mental states differed only in their organization of the same fundamental stuff. Given Russell's way of thinking in philosophy this doctrine of neutral monism, as it came to be called, had an immediate appeal for him. He was fascinated by it, because it promised to reduce the minimum vocabularies of both physics and psychology to one vocabulary, which, if it could be done, would gladden his reductionist heart. And the enterprise was appealing for another reason: it provided many agreeable problems of construction for the mathematical logician. "Everything interesting", he wrote in *What I Believe*, "is a matter of organization, not of primal substance" (1925, p. 18). It took him several years (many of them war years) to think through all of his objections to the theory of neutral monism. At last he found answers to them and consciously adopted a qualified form of neutral monism as his own position. The neutral entities he took to be events—very short segments of space-time—and in his work during the 1920s he proposed logical constructions out of events for some important inferred entities, both physical and mental. The fit of neutral monism with his philosophical method is so nice that one is reminded of an old saying, suitably changed for the occasion: if Russell had not found neutral monism, he would have been forced to invent it.

I want to conclude by drawing attention to a curious feature of his work in philosophy. As we have seen, passion and emotion have no place in his conception of philosophy. Yet passion does have a central role in his own philosophizing. We know this from the "Prologue" to his *Autobiography* and not from his published philosophical works themselves. The "Prologue", however, is a retrospective account, so it might easily misstate the strength of the passion which drove him to "search for knowledge" (1967, p. 13; 1967a, p. 3). For any who have doubts on this matter, I want now to lay them to rest.

His letters to Lady Ottoline, whose acceptance of him as her lover loosed a torrent

of words, show, if anything, that he understated the force of the passion which drove him. With reference to *Principia*, he wrote:

It is not an easy thing to move the world. I have put into the world a great body of abstract thought, which is moving those whom one might hope to move by it, and will ultimately, probably, move many people who will have never heard of philosophy. What makes it vital, what makes it fruitful, is the absolute unbridled Titanic passion that I have put into it. It is passion that has made my intellect clear, passion that has made me never stop to ask myself if the work was worth doing, passion that has made me not care if no human being ever read a word of it; it is passion that enables me to sit for years before a blank page, thinking the whole time about one probably trivial point which I could not get right. That same passion now has gone into my other writing. (#429, 30 April 1912)

Later in the same letter he refers again to the sort of passion which drove him to work on the foundations of mathematics. "In that mood, pure thought on things not connected with human life, seems to me the only thing worth while. I find then a kind of joy in clearness, in transparent lucidity, in godlike detachment. I like to see the clear stream issuing from the sandy soil. But this is mainly pride, which is one of my besetting sins." There is something fascinating about a passion so strong that it drives all before it, yet results in philosophical writings one of whose main characteristics is a complete absence of it. Take for instance Part IV of *Principia*. It deals with "relation-arithmetic", and we know from the way Russell discusses it in *My Philosophical Development* (1959, pp. 95–191) that a lot of his passion was poured into it, yet there is not a trace of it in the published work. Many of his readers, in the days before his *Autobiography* was published, were misled by this absence into believing that Russell was a man largely devoid of passion—at least when he was doing philosophy. But his letters to Lady Ottoline reveal him to be most gripped by strong passion at those times when he was thinking about "big things" (#429, pmk. 30 April 1912).

He sometimes experienced such passion when he had no large problem to think about. When this happened he felt great distress, but, as he explained to her, there was little he could do about it. "Of course in theory the solution is obvious: keep the passion, but turn it only onto big things. But that is treating oneself too much like a machine; or perhaps not enough like a machine. Passion is like the wind, it sweeps along wherever it finds a way open, but you cannot make it go down one way and not down another, you can only screen it off altogether" (*ibid.*). What seems fortunate for us is that his passions so often swept down fruitful paths.

Although he is very ready in private to acknowledge the extraordinary passions he felt, he did not want them to show in his work in any obvious way. He wanted his philosophical writings to shock his audience by their apparent lack of human concern.

My intellect is amazingly clear these days—it sees into the heart of things in a white flash. I expect my paper on matter will be a model of cold passionless analysis, setting forth the most painful conclusions with utter disregard of human feelings. I haven't had enough courage hitherto about matter, I haven't been sceptical enough. I want to write a paper which my enemies will call "the bankruptcy of realism". There is nothing to compare to passion

for giving one cold insight. Most of my best work has been done in the inspiration of remorse, but any passion will do if it is strong. Philosophy is a reluctant mistress—one can only reach her heart with the cold steel in the hand of passion. (#423, n.d.)

A revised version of his paper on matter survives, and it does show that he had achieved his aim of cold and passionless analysis. After he read the revised paper to an audience at Cambridge he reported that it had not been a success, which he blamed on the rawness of the ideas in it. Only Wittgenstein understood it, so there was not much discussion (#608, n.d.).

Passion figures in two ways in his accounts of it. There is first the enormous expenditure of passion in just getting the work done. And then there is the passion captured in the very structure of the work itself. Whether in architecture or logic this transfigured passion accounts for much of the beauty of the work. Philosophy, as opposed to philosophizing, is like a building after the architect and building crews have departed. The passion that remains forever fixed in the completed structure is only a fraction of the total amount expended during the construction, but it accounts for much of the total value. "What people really enjoy in writing or music", he wrote Lady Ottoline, "is just the last drop of anguish in the man's soul: they take the place of gladiatorial shows" (#429, pmk. 30 April 1912).

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