Introduction

by Ian Winchester

THE ESSAYS IN this collection on Russell's early technical philosophy cover mainly the years between 1896 and 1914 and range over a wide variety of technical topics and problems which interested Russell in those years, including: his early plan for a comprehensive Hegelian dialectic of the sciences, his early studies of dynamics and of logical paradoxes, his work on the foundations of geometry, his wrestling with Meinong, his attempts to produce a satisfactory theory of types, the propositional logic he actually produced, the question of whether or not classes can be eliminated in *Principia Mathematica*, his conception of philosophy as it varies and firms in the early period, his early realism, and his struggles with neutral monism.

There is room for debate as to just what Russell's early technical work includes and what dates are spanned by such a notion. This collection, therefore, is one answer to such questions. But admittedly there is a certain arbitrariness to any choice. The First World War marks a break in Russell's life which is like no other, since it is this event which separates Russell from his university (he lost his lectureship at Trinity due to his pacifism), from his only promising pupil, Wittgenstein, and from his major collaborator Whitehead. Wittgenstein joined the Austrian army, and Whitehead, whose son was killed in action, supported the war. So far as Russell's technical work was concerned, he never devoted full time to philosophy after 1914 in the way that he had prior to it, except for brief bursts.

Russell certainly changed his mind after that date on many important points, particularly with respect to mind, language and knowledge. But the beginning of the war marked the end of his developing picture of what kind of activity philosophy was. Thus his view that philosophy is essentially an analytical activity, that analysis doesn't falsify, that logic and language are intimately related but that language as we find it does not have the final say in logical matters, that whatever we can understand must be composed of elements with which we are acquainted—all these were present before the beginning of the war and often much earlier. (See John Slater's article in this collection.) On the other hand, one might argue that 1905 would be the natural date for marking a break with Russell's early technical work since after

"On Denoting", published in *Mind* that year, Russell's views undergo a radical change due to the technical possibilities in logic, theory of knowledge and metaphysics which the technical results in that paper appear to promise. He now has a technical way to avoid a number of inferential fallacies in logic and to make other arguments he thinks valid to go through. He now has a new, technical way to display the fallacy in the ontological argument. He now has a means to drive a clean wedge between knowledge arising out of acquaintance and that from description, the private kind and the public kind. And this will affect both his theory of mind and his picture of science, leading him to attempts to reconstruct the world of science and the everyday world out of the elements of acquaintance. He now has technical grounds for finding solipsism irrefutable, if unbelievable. And his philosophy takes a sceptical turn from which he struggled to escape for the rest of his career.

But if 1905 has, like 1914, strong grounds for being taken as the year of the great watershed, dividing Russell's early technical work from his later, so might 1901 make a plausible date for this division. In 1901 he discovered the class of all classes paradox which goes by his name, Russell's Paradox. And this discovery affected all his subsequent work on the foundations of mathematics and led to his work on, among other things, type theory and the theory of descriptions.

Thus, perhaps, one should date the transition from Russell's early technical philosophy to his later at the time of his discovery of the paradox. Certainly prior to the discovery of the paradox Russell was a philosophical enthusiast in many directions. He was a full realist, believing even in the reality of everything which the Hegelians had denied. He had no doubts about the possibility of developing the whole of mathematics from a few logical principles. Like Kant he saw room for the à priori in not only mathematics, but physics as well. But after his struggle with the paradox, he justifies premisses not on grounds of their aprioricity, but because they lead to undeniable consequences. Perhaps this is the point at which we should mark Russell off as being thereafter in the British Empiricist tradition.

On the other hand, Russell is hard to date clearly because there are always present elements from his earlier phases living simultaneously with foreshadowings of his later thought, so that clean breaks are not really characteristic of him, even when he thinks he has made one himself. The fifteen papers and the panel discussion which are collected below illustrate this complexity in Russell, as well as mark a variety of phases in his early thought. Thus the papers in Part I, Residual Hegelianism, by Griffin, Winchester and Moore relate to Russell's earliest technical phase, roughly from 1895 to 1901 or so. In this phase Russell was largely concerned with what it was he wanted to accomplish in philosophy initially as a Hegelian but, under the impact of Moore, as a realist. Thus, although Russell looked with rejected-Hegelianism at logic, mathematics and physics by 1900 or so, he dragged a lot of his Hegelian baggage with him. Certainly his urge to comprehensiveness was partly due to his own unusual powers. But it is arguably also due to his earlier Hegelianism, of which the Tiergarten programme, discussed by Nicholas Griffin, is perhaps the most obvious early manifestation.

Not surprisingly, Gregory Moore also finds Russell's interest in paradoxes, something which is never evident in Frege, for example, to have its roots in the earlier Hegelian phase. This shows itself not only in Russell's actually having found the one which bears his name, but also in his manner of writing throughout his life, certainly in his early technical phase. Thus as late as in "On Denoting" we find Russell presenting his case as the solution to a connected set of "paradoxes and puzzles" which he develops as strongly as he can. It is true that, unlike Hegel, he doesn't see these as necessarily engendered and ultimately unresoluable. But that he sees them as the starting-point is probably connected with this earlier indoctrination in and fascination by Hegelian thought.

Also, the earliest technical debates in which he engages are connected with the claims of Hegelian or neo-Hegelian logicians like Bradley. Thus whether or not external or internal relations are primary is central to Russell, and arises out of his rejection of Hegelian logic. Of course Leibniz plays a role here, too, and Russell's interest in Leibniz is partly connected with the similarity between Leibniz's development intellectually and Russell's own, as well as with a variety of characteristic Leibnizian logical doctrines which bear relations to that of the Hegelians, particularly their idealism. Though he argues in print with Bradley about internal relations, it is really with Leibniz that he actually argues, both in his *Leibniz* and in *The Principles of Mathematics*, for Leibniz's logic was ultimately intentional. The strength of Russell's attachment to Wittgenstein's early logical notions was, I suspect, connected with Wittgenstein's offering what looked like a plausible case for a totally extensional logic and hence an ultimately anti-Hegelian one.

This struggle to free himself from his Hegelian predispositions is also strikingly evident in his treatment of Newtonian mechanics in the *Principles*. In Winchester's paper on "The Antinomy of Dynamical Causation" the residual concern to detect antinomies and to worry about them is clearly present. At much the same time as Russell was struggling with the antinomies of Newtonian mechanics Einstein, Russell's somewhat younger contemporary, was similarly struggling especially with those at the intersect between Newtonian and Maxwellian physics. Einstein, unlike Russell, was concerned to reframe mechanics so that the antinomies disappeared. Thus Russell, with unparalleled mathematical and logical equipment, did not see deeply into mechanics, perhaps because he still enjoyed antinomies more than he cared to admit. While Einstein, with no Hegelian presuppositions to struggle with, and following a similar path to Russell, made a difference to mechanics and to our picture of the world which Russell was unable to glimpse, until he later read Einstein. Russell's two resulting works due to the impact of Einstein's thought, namely, *The ABC of Relativity* and *The Analysis of Matter*, come in what might be called Russell's middle technical thought, by which time Russell was now strongly of a Humean cast of mind modified and strengthened by his logical work with Whitehead.

The large, central group of papers in Part II deal with Russell's mathematical and logical work, especially with respect to his very early technical work. The very earliest is probably his publication on the foundations of geometry which derived from his fellowship dissertation. Joan Richards doesn't discuss Russell's work directly. What she does do in her "The *Essay on the Foundations of Geometry* and the Cambridge Mathematical Tradition" is place Russell's work in context. The tension between the Hegelian and Kantian elements in mathematical thought in pre-1890's Cambridge and the actual mathematical work going on there is crucial to Russell's reading of the problems of "founding" geometry. Russell came soon to think the essay itself worthless and merely "rot" and not "*ce petit chef d'oeuvre de la dialectique subtile*" as a French commentator would have it. But its interest in foundations and Russell's discovery that Leibniz's philosophy could be developed from a very few

logical premisses surely lay behind his conception of "founding" all of mathematics on logical principles. Thus the context developed by Richards is exactly to the point.

It is rather hard to find an easy path from Russell's very early work in mathematics and the foundations of mathematics, including his fellowship dissertation and his book on Leibniz, through to his *Principia Mathematica* phase. One way would be to look at his early logical manuscripts closely. Ivor Grattan-Guinness has surveyed these with a view to their orderly publication, and with some attempt to separate the wheat from the chaff. Thus his "Russell's Logical Manuscripts", which is published elsewhere and therefore only summarized in this collection, is very useful in orienting the reader towards the multitude of manuscripts which remain for scholarly contemplation. Grattan-Guinness is concerned primarily with problems of publication and a survey of the extent of the manuscripts. One rather interesting conclusion which Grattan-Guinness draws from his own survey of the various logical manuscripts is that Russell is and remains unclear throughout these manuscripts as to just what mathematics is, and to what the scope of "mathematics" is. Perhaps, when these various manuscripts are published and thus more readily available to scholars than now, other illuminating insights will be presented. But at the moment there is only a rather limited number of logical notions which arise in Russell's early technical work, including his theory of descriptions, type theory, classes and their elimination, propositional and predicate logic and the like, about which critical discussion circles endlessly.

From the point of view of such discussion, O'Leary, Urquhart, Couture and Seymour have fresh and illuminating approaches to old topics. Thus O'Leary, with the help of a computer programme and the rewriting of Russell and Whitehead's notation into Polish parenthesis-free notation, has attempted to reconstruct all of Russell's proofs in the early part of *Principia Mathematica*. In the process of doing this, O'Leary finds and makes explicit much of the mechanics of the early part of *Principia* which is masked by Russell's conciseness, including Russell's use of a host of unmentioned inference rules. Interestingly enough, O'Leary finds some of Russell's proofs as yet impossible to work out. He may be the only one, including Whitehead, who ever tried.

We know, of course, why Russell became interested in type theory. After the shock of his class of all classes paradox he came to think that some kinds of self-reference were vicious and needed a systematic way through the minefield which he increasingly discovered to be involved. This involved, for someone who wanted utter logical simplicity to lie behind all of mathematics, a distressing complexity. Thus it is not surprising, as Urquhart points out, how reluctant Russell was to adopt any type theory at all if it could be avoided, and if not avoided, then the simplest, most efficient one. Urquhart argues plausibly that Russell's dream of a type-free universal logic still has much to be said for it as an ideal and that for certain parts of mathematics, algorithm theory, group theory and their ilk, self-reference without type distinction is central to the kind of mathematical games therein played. However, unless something like Wittgenstein's later views about the nature of, and harmlessness of, paradoxes in logic are right, then we might agree with Urquhart's conclusion that "the idea of a type-free universal logic for the time being at least remains for us, as it was for Russell, a beautiful but impossible dream."

One of the ways in which Russell had conceived of ridding his logic of the class of all classes paradox was to eliminate the notion of classes. If classes as a fundamental notion were unnecessary, then the paradox wouldn't arise (or at any rate might not), and hence the beautiful dream might be maintained. Russell himself did not succeed in this, and classes remained. But some writers have suggested that a substitutional interpretation of quantifiers allows the elimination of classes in *Principia*. Jocelyne Couture argues that this attempt to claim that classes are mere linguistic expressions fails, since classes as defined in *Principia* fail to have the necessary formal characteristics which would enable them to be eliminated. Whether or not this proves to be the final word on the topic, Couture lays out clearly, and in a short compass, the issues surrounding the question. And her conclusion that "to maintain the substitutional interpretation for classes would be the same as saying that substitutional instances of a given statement can be formed with expressions belonging to different grammatical categories" is not easy to see a way around.

Perhaps no topic in Russell's early technical work has received so much discussion as the theory of descriptions first put forward in "On Denoting". Although in that article Russell used as his examples sentences drawn from everyday language, including "The present King of France is bald", he had in mind mathematical phrases which are difficult to interpret like "The class of all classes is a member of itself." Superficially it looks as if all phrases of the form "the so-and-so" should be treated like proper names, and in Aristotelian logic they are. But then the assumption is naturally made that a proper name, at least in logic, should be something which has a referent, or at least which is to be assumed to have one. And if a denoting phrase is a self-contradictory one, then it is hard to see either how it might denote anything or even be properly assumed for sake of argument to do so. Russell thus treats such phrases, whether self-contradictory or not, as being of a different logical kind from proper names. In effect, he treats them as never, properly speaking, having referential uses and as always having meaning only in the context of a complete sentence. They are thus, in his terminology, incomplete symbols in a way in which proper names are not. If one supposes that what logic is doing is to analyze a fragment of natural language and to display the argument patterns of natural language (when they are correct), then one can argue, as Strawson and others have, that Russell's analysis doesn't account for the referential uses of definite descriptions when, in ordinary speech, they actually have such uses. Russell's reply to Strawson, reprinted in his philosophical autobiography, *My Philosophical Development*, takes the form of denying that he had any special interest in analyzing natural language as such, and that he was, rather, concerned to replace the confused thoughts which most people at most times have in their heads with a less confused and better analyzed one. This is, of course, a question of considerable depth, namely, what exactly is logic up to? Michel Seymour enters this discussion with his paper on the referential use of definite descriptions by trying to steer a course between the two pictures of logic without having to commit himself to the one or the other. Thus he argues that we can accept Russell's view that definite descriptions are incomplete symbols while allowing at the level of pragmatics that they have different kinds of referential uses.

It is a well-known gap in the writings of Whitehead and Russell that the projected volume on geometry never appeared. This volume was supposed to be Whitehead's responsibility, but the contingencies and exigencies of life during and after the First World War appear to have been too much for Whitehead's being able to get on with

it; and the falling-out between Russell and the Whiteheads over the war meant that further collaboration was out of the question. Martha Harrell, in her paper on the extension of *Principia* and related systems to geometry, suggests that the real reason might have been that Whitehead was unhappy with the need to use the equivalent of a Kantian move in order to reduce geometry to the logic of *Principia*. Kant, you will recall, thought that geometry required diagrams, and Harrell suggests that Whitehead thought that *Principia* required classes in intention and that this was tantamount to Kant's view with respect to geometry. It is not clear why, however, if Harrell is right, Whitehead didn't object to the entire project of *Principia* and not merely to geometry. Be that as it may, Harrell argues that there is enough in the *Principia* as we have it to indicate the lines on which such a geometrical extension might have been made. Among the more interesting discoveries which Harrell has made is an explanation of why *Principia* contains no development of complex numbers. The reason given by Whitehead and Russell is, she tells us, because such numbers do not form a one-dimensional series; thus they belong to geometry.

Another suggestion as to why Whitehead didn't continue, which certainly strikes one as plausible, is that the advent of relativity, especially the general theory, upset Whitehead's picture of space. Indeed, it is well known in physics circles that Whitehead later produced his own version of gravitational theory in which he retained Euclidian space, a theory which was until a few years ago considered to be the only serious contender along with that of Einstein as regards experimental adequacy. Certainly if geometry was an ambiguous notion, or at any rate in flux, then a treatment of it from a logicist point of view would be difficult to produce.

During the 1920's both Russell and Whitehead turned to questions related to physics and to its geometrical underpinnings. The correspondence which Harrell has analyzed between Whitehead and Russell, detailing Whitehead's work for the fourth volume of *Principia*, sheds considerable light on the thought of both men during the period of the rapid development of physics in the 'twenties and 'thirties. In his earlier *Our Knowledge of the External World* (1914) Russell had used Whitehead's method of constructing space–time points with smooth properties out of entities which were not smooth. This had been part of Whitehead's work towards the fourth volume, and Whitehead considered Russell's use of it in print, even though Russell had acknowledged Whitehead as its author, as wrong. From the correspondence cited by Harrell between April 1905 and October 1913 it would appear that, at least as regards the construction of such space–time points, Whitehead had communicated his results to Russell before 1914 and perhaps as early as 1913. On this particular point, as on many others, the development of a widely accepted theory in geometry, as Harrell points, out is not really to be found. Thus one may hope, with her, that reconsideration of the ideas in *Principia* on geometry may prove fruitful yet.

The remaining articles comprise Part III. Although they relate to Russell's early technical philosophy, they are on the more general philosophical questions of what philosophy is, of what the relation between science and scientific theories and the world is, and of what the relation of mind and the world may be. The Part includes two historical studies, on the philosophical relations of Russell and Meinong, and the logical relations of Russell and Quine. Russell's conception of philosophy, while forming in the 1890's, is pretty well fixed by the time of the publication of *Principia*. His scientific realism is a result of his having abandoned idealism and monism and

is a feature of his thought even after 1897 or so. But his attachment to neutral monism is a slowly developing process involving dialogue with, among others, William James over a long period of time, and it is not until 1922 that he embraces it fully.

John Slater teases out for us in his "Russell's Conception of Philosophy" Russell's changing picture of philosophy, both as a body of doctrine and as an activity, from his early years at Cambridge, through his first break with the prevalent idealism in December 1897, to his early view that philosophy is where problems not yet amenable to scientific treatment are kept hostage, and finally to his later, more provocative putting of the same thing, that science is what we know and philosophy is what we don't. He also shows us how Russell's picture of method developed, from his early claims in *A Critical Exposition of the Philosophy of Leibniz* that all sound philosophy should begin with an analysis of propositions; to his view in *The Principles of Mathematics* that analysis, while necessary in philosophy, nonetheless in some sense leads in part to falsification; to his view from 1905 and subsequently that analysis can lead to a right understanding (as in his theory of descriptions) and thus need not lead to falsification at all. Although Slater doesn't say so, the view in the *Principles* has a residual Hegelian flavour which, to readers of the earlier papers in this collection, need come as no surprise.

Slater also argues, convincingly to my mind, that Russell's long collaboration with Whitehead was also central to his picture of scientific method both in that it was collaborative as well as in that it was piecemeal. Philosophy, at least since Aristotle, had tended not to be either of these, and for Russell it was something of a revelation that it could be.

Russell took Meinong deadly seriously. The referential puzzles dealt with by Meinong were added to Russell's stock of problems. For years he worked on them, first with his denoting theory, then with a grand elaboration of his theory of acquaintance in the suppressed manuscript on *Theory of Knowledge* (1913). Janet Farrell Smith reveals many parallels between the theories of Meinong and Russell, even as late as *The Analysis of Mind* (1921).

Both Russell's scientific realism, discussed by Michael Bradie, and his neutral monism, which Robert Tully tackles, are connected by Slater with his picture of scientific method in philosophy. The philosopher, like the scientist, Russell supposes, aims at producing true accounts of his subject-matter and does this by means of an analysis. This analysis begins from a problem stated in terms of everyday entities or inferred entities which, as the analysis proceeds, is replaced successively by logical constructions out of more primitive materials. Both his *Analysis of Matter* (1927) and his earlier *Analysis of Mind* (1921) involve this method explicitly. Slater makes the unusual but illuminating claim that Russell's espousal of monism follows almost inevitably from his philosophical method. Essentially Slater argues that Russell's notion that analysis arrives at a minimum vocabulary for any science relating to a subject-matter under scrutiny leads inevitably to his considering physics and psychology as involving, ultimately, a common minimum vocabulary. And hence neutral monism. This is because, as Russell sees it, the evidence for physics comes through our senses, that is to say, via the psychological; while on the other hand, the workings of our senses are to be understood by means of physiology, a branch of physics for Russell. A common minimum vocabulary for these two intimately

connected scientific realms thus was a natural direction for his thought to take ... and neutral monism is tailor-made for this task.

Michael Bradie's paper on Russell's early scientific realism treats in detail one aspect of Russell's philosophical method which Slater has introduced, namely, the construction of common-sense objects and the theoretical objects of science out of sense-data. One puzzle which Bradie treats that is of general interest is the question of whether Russell can be both a logical constructivist and a scientific realist at the same time. If a realist is one who believes that there are really objective things outside of the data of consciousness, then a scientific realist is, presumably, one who believes that the theoretical entities in scientific theories, if such theories are true, really do have objective counterparts in the world outside of human experience. And moreover, he must also believe or presuppose that scientific theories can be true.

According to Bradie, it is hard to interpret Russell's account as permitting him to admit objective counterparts for his logical constructions. Sometimes it seems that Russell thinks only the aspects of a thing (its sense-data or sensibilia) are the more basic, and sometimes the logical construction itself. But perhaps, suggests Bradie, the difference is between what is fundamental according to Russell from the point of view of our *knowledge* of the external world, as opposed to what is metaphysically fundamental? That is, from our epistemological predicament, sense-data are fundamental. Whereas, ontologically speaking, material objects and theoretical entities are fundamental but are never directly apprehended by us. Rather, they are to be conceived of as logical constructions by us out of our perspectival, experiential data. In the early realist period which Bradie discusses, Russell comes very near to neutral monism, as Bradie shows. However, Russell doesn't quite embrace it since he still conceives of the mind or ego on Leibnizian lines as an ultimate particular. Thus his theory of perception is still relational: the mind, thought of as one particular, stands in relation to (in awareness of) a sensibilium, and so there comes to be sense-data. Such sense-data, however, are not yet conceived by Russell as mental since they fail to possess whatever the intrinsic mental characteristic is which thoughts and desires possess. At this point Russell does not yet conceive of the difference between the mental and the physical as merely a matter or arrangement of some more primitive stuff, but rather that the mental possesses an intrinsic quality which material stuff does not.

Russell abandons this view sometime after 1918 when he essentially conceives of the ego, too, as a logical construction. So long as he thought that sense-data failed to possess the intrinsic quality of the mental, he could never embrace neutral monism. Another barrier for Russell was probably logical. He had distinguished as early as 1905 between logically proper names, which pick out objects with which we are directly acquainted, and definite descriptions, which sometimes look as if they are functioning as proper names but which must be analyzed differently. But this logical doctrine seems to require an epistemological one, namely, that acquaintance be a two-term relation between a knower and a known, an ego and some object of perception.

Robert Tully in his article on Russell's neutral monism not only shows the steps by which Russell came to abandon this earlier position, but also, contrary to the established commentary in this field, argues (convincingly, to my mind) that Russell's enthusiasm for neutral monism was not a momentary enthusiasm which issued in *The Analysis of Mind*. Rather, it was something he maintained and worked on for the rest of his philosophical career. Tully is surely right in, for example, assimilating Russell's analysis of demonstratives in terms of descriptions of compresent qualities to the residual problems which egocentric particulars posed for neutral monism which he pointed out in his final lecture on logical atomism in 1918. Similarly, his later treatment of belief from the vantage point of the methods of behaviourism is also a later attempt to handle the residual problems neutral monism faces with respect to belief of the kind he had considered in 1914. He then thought that belief, like thought and desire, possessed special intrinsically mental qualities. And, under the impact of his discussions with Wittgenstein before the war, he had also added (as he said in the 1918 lectures) another kind of logical form to his logical zoo, namely, the form "x believes that p", "x thinks that p", "x desires that p", in other words the propositional attitudes. These judgments have great difficulty being construed as extensional in a propositional calculus, since they are compound judgments which do not appear to be a truth-function of their internal propositions.

It is possible that Wittgenstein's analysis of them in the *Tractatus* had something to do with Russell's being able to embrace neutral monism with a clear conscience shortly after the war. But he continued to struggle with their analysis in *An Inquiry into Meaning and Truth*, nearly twenty years later.

Professor Quine's piece on his personal, logical correspondence with Russell is not trying to make a contribution either to Russell criticism or to philosophy, but rather to the sense of Russell's engagement, both in personal and philosophical, as well as global, matters over a very long life. It is perhaps not remarkable that some of the Russell–Quine correspondence is on topics such as getting the Polish Jew, Tarski, a job in the U.S. after 1939 or Russell's embarrassment at having been misquoted as to Tarski's status as a logician. But it is rather remarkable that so much of Russell's correspondence with Quine between 1935 and 1967, when Russell was in his mid-sixties to mid-nineties, was on logical topics directly, or failing that, on philosophical ones.

One often wishes, in philosophical discussion, that one had a tape-recording of what people actually said in discussion. What one would not give for a single actual encounter between Socrates and Parmenides and other contemporary Athenians, for example. As it happens, each of these papers was delivered over a three-day period in Toronto at Trinity College under the kind auspices of the Provost of Trinity and with the financial assistance of the Canada Council (SSHRC), the Ontario Institute for Studies in Education and the Bertrand Russell Editorial Project at McMaster University. There are, in fact, tape-recordings of the discussions following each of these papers. But the final panel, moderated by Sir Alfred Ayer, took the form of vigorous discussion among Ayer, Robert Tully, W.V.O. Quine, Ivor Grattan-Guinness, Nicholas Griffin, John Passmore, Harry Ruja and other members of a largish international audience, on the various topics which had taken up the previous days. The panel discussion ranges over the implications of Russell's early technical work and its tenability in the light of subsequent criticism and developments. Perhaps we cannot have an encounter between Russell and some of his contemporaries. But what we do have is one between Ayer and Quine over Russell's phenomenalism due to the elimination of singular terms, between Grattan-Guinness and Quine over the necessity of a theory of types, between Yvon Gauthier and Nicholas Griffin and John Passmore on paradoxes in mathematics, Ayer and Tully

on Russell's avoidance of the issue of solipsism, and a variety of interjections from the audience generally. But the details of these delights are best discovered by the reader and not described by me.

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