Wittgenstein's Notes on Logic

MICHAEL POTTER

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One often makes a remark and only sees later *how* true it is.

Wittgenstein, 10 October 1914

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Preface

I wrote the first draft of this book while I was a Senior Research Fellow in the Department of Philosophy at Stirling University, funded by the AHRC. The friendly welcome I received there, in a department which I came to see as an oasis of academic seriousness and respect, contributed very markedly to my well being and hence to the successful completion of the project. Such conducive research environments are rare; I suspect that university administrators who recognize them when they arise, and cherish them as they should, are even rarer.

A happy feature of the AHRC project was a series of workshops and a concluding conference on the *Tractatus* and its history, at various of which I tried out some of the ideas expressed here. I have pleasant memories of the constructive atmosphere at these meetings, and am grateful for all the feedback I received, even when it was no more articulate than a raised eyebrow. Something similar goes for a series of seminars on the first draft of the book which I conducted in Cambridge in the Lent Term, 2006. I am sure the final version is better as a result.

In the archival research for the book I have been assisted by librarians at the Houghton Library, Harvard University; the Russell Archive, McMaster University; and the Manuscripts Room, Cambridge University Library. I am grateful to Blackwell Publishing for permission to reprint the *Notes on Logic* as an appendix. In compiling the version printed here I was grateful for earlier editorial work by Michael Biggs. Will Crouch compiled the index for me. Nicholas Griffin, Stephen Read, David Cardwell, and Brian McGuinness have responded helpfully to requests for information. I have also benefited from detailed comments on the first draft by some unusually generous colleagues. I am very grateful to Ian Proops, Peter Sullivan, and two anonymous readers for Oxford University Press, not least because each of them has saved me from a number of embarrassing displays of my own ignorance and stupidity.

Wittgenstein scholars who read this book will, of course, look first for their own names in the index. Most will, I fear, come away disappointed. If I had footnoted everyone I have read, and explained in full what I thought of their

views, the book would have been twice as long and twice as late, but not, I suspect, twice as useful. My gratitude for what I have gained from their work is no less sincere for remaining unitemized. To Brian McGuinness, however, I owe a particular debt which it would be churlish not to single out: his editorial work, most notably on Wittgenstein's letters, has saved me an enormous amount of time; and his writings on Wittgenstein display a combination of historical accuracy and philosophical acuity which I can only dream of emulating.

It was Peter Sullivan who arranged the AHRC project in the first place and hence created the circumstances in which it was possible for me to write this book. I must have discussed almost every idea in it with him at some point, and many were no doubt originally his. Since neither of us now knows which these are, and some of those which are certainly his he now prefers to deny, this preface is the only feasible place to record that debt.

MDP

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Introduction

In 1911 Wittgenstein arrived in Cambridge to study philosophy with Russell. In 1913, just before he left to spend a year communing with his soul in Norway, he produced the *Notes on Logic*, a sort of summary of the conclusions he had reached during his time in Cambridge, and his first philosophical work. My intention in what follows is to engage in a study of that period and this work.

Most of Wittgenstein's surviving pre-Tractarian writings were published, in stages, some time ago (in 1957, 1961, and 1979). Since then it has been a familiar method of all serious exegetes of the *Tractatus* to mine these writings for remarks to support their interpretations. One reason they do this is simply because they can: although Wittgenstein had his prewar Cambridge notebooks destroyed, and two of his later notebooks are probably missing, what remains (the *Notes on Logic* already mentioned, the *Notes dictated to Moore*, three surviving wartime notebooks, and a handful of letters) is a body of evidence of a scale nicely poised to intrigue but not intimidate the diligent scholar. But it is also because they must: some of the remarks in the *Tractatus* are so obscure that only by relying on the earlier notebooks for support do we have any hope of divining their meaning.

Although I hope in this book to contribute to the same project of Tractarian exegesis, I aim to do so by a somewhat different method. Instead of studying the *Tractatus*, and drawing on Wittgenstein's earlier writings only when they contribute to understanding it, I shall here be focusing on the 1913 *Notes on Logic*, treating them if not quite as a terminus in Wittgenstein's work then at least as worthy of study in their own right.

There are several benefits to be derived from this approach. One is that it gives us a better chance of understanding Wittgenstein's own reasons for some of the views he held. This is probably a good reason to study the early works of almost any major philosopher, but it is especially so with Wittgenstein, whose own arguments for his views are so often too compressed to be comprehensible without understanding the context in which he formulated them. An obvious example is that commentators have presented a remarkably large number of Wittgenstein's logical claims as consequences of his picture theory. But not all

of that theory is present in the *Notes*. In particular, the analogy with pictures which gives the theory its name came to him almost a year after he compiled them. Understanding the parts of the theory which were already in place by then puts us in a much better place to understand what further work he thought there was for the picturing analogy to contribute.

Another benefit has to do with Wittgenstein's method of working, by which remarks were first written down, then compiled and rearranged (almost endlessly in the case of some of his later work). These rearrangements sometimes gave the remarks, in the resulting juxtapositions, resonances which they did not originally have. However allusive these resonances may be, however possible it is that Wittgenstein may sometimes have been struck by them and used his final arrangement of the text to invite them, the fact remains that it is likely to be worthwhile to study the remarks in their original setting.

But I also hope here to recommend, by exemplification, an approach to the study of the *Tractatus* that, if not actually denying the boundary between biography and philosophy, at least regards the interaction between them as potentially fruitful. I do not apologize for introducing biographical observations into what is primarily a philosophical study; and the reason is that I have found it often helps, in judging which interpretation of Wittgenstein's remarks is plausible, to have a sense of how he thought and how he worked. If we are to gain the maximum insight from his work, we need to understand, certainly, what motivated him to address the problem he did in the way that he did. But the exegetical task of understanding him aright can at times seem harder with Wittgenstein than with some other philosophers. (Russell is an obvious example.) One thing that makes it easier, though, is the striking unity, if not in Wittgenstein's thought then in his method of thought. Almost all of his ideas are, in a certain sense, simple. Once we have grasped the sort of simplicity that is in question, it can then become a useful measure by which to assess our interpretations in the future. And it is here that an understanding of the man is relevant.

Wittgenstein wrote the *Tractatus* during the First World War, of course, but it had its birth in the two years he spent working in Cambridge with Russell between 1911 and 1913. He compiled the *Notes on Logic* at the very end of that period, as a summary for Russell (and perhaps, to an extent we shall discuss later, for himself) of the work he had accomplished. The destruction of his notebooks, mentioned earlier, makes the *Notes* almost our only guide to the work he had been doing in Cambridge. By studying them, therefore, we can hope to discover which of his ideas Wittgenstein owed to this period and which to the very different circumstances in which he worked later, first in Norway and then on active service during the war.

We can also hope to lay bare some of the influences which helped to form his views. The acknowledgment Wittgenstein made in the preface to the *Trac-*

tatus to 'the great works of Frege and the writings of my friend Bertrand Russell' is endlessly quoted. So, too, is a diary entry from 1931 in which he listed Frege and Russell (along with others) as people whose ideas he had used in what he called his 'work of clarification'.[1] But there is some gap between acknowledging an influence and determining what that influence was; and in any case we are by no means compelled to assume that Wittgenstein was conscious of, and chose to acknowledge, all the influences that shaped his thinking. Once again, though, the benefits will not just be biographical: knowing where he got his ideas from is often a useful tool for understanding what those ideas really were.

Wittgenstein's writings have been worked over so thoroughly in the half century since his death that the lack until now of any book-length study of the *Notes on Logic* is rather remarkable in itself. This is especially so when one pays attention to the significance of their timing, just highlighted. One reason for this neglect may be that the *Notes* are, even by Wittgenstein's gnomic standards, hard to understand on a first reading. If they were, to some extent at least, compiled only for Russell's benefit (or for Wittgenstein's own), then that is of course part of the explanation. But another part of the explanation lies in the rather complicated circumstances of their composition, which have furnished us with a text (or rather a series of texts) whose convoluted and repetitive structure has compounded the difficulties in comprehension. An important aspect of this book, therefore, will be to disentangle these texts in order to leave the way to philosophical understanding of Wittgenstein's intentions much clearer.

That historical detective work will be the subject of Appendix A, and the *Notes on Logic* themselves are reprinted with a critical apparatus as Appendix B. I use B*n* and C*n* to mean the *n*th paragraphs of the Birmingham and Cambridge versions of the *Notes* respectively. (Decimal numbers unqualified are, of course, references to the *Tractatus*.) The bulk of the book itself is taken up with exegesis—not, certainly, of every sentence of the *Notes*, but at least of what I take to be their central claims. The aim will be to show that, once the problematic structure of the surviving text is disentangled, the *Notes* are a much more coherent and substantial work than has hitherto been recognized.

The reader will no doubt notice, however, that the *Notes on Logic* are not, to begin with, mentioned very often. The reason for this lies in the fact that I have attempted here to describe not only the contents of these *Notes*, narrowly conceived, but the whole of Wittgenstein's period working with Russell in Cambridge. Just what part of that period Wittgenstein saw the *Notes* as summarizing is now hard to determine—I shall discuss this question further in §11.3—but even if he viewed them as a report on all of his discoveries up to that point which he thought worth preserving, it would be natural for many

[1] *CV*, 19.

more of those discoveries to have occurred to him in the second year of study than in the first: that first year was, after all, when he came to Cambridge as, in effect, a self-taught philosophical novice. In relation to his first year of study, therefore, our evidence concerning what Wittgenstein was thinking is much more conjectural: there is little hard evidence apart from a couple of letters to Russell. The aim of the first half dozen chapters, therefore, will be to make those conjectures that seem possible concerning the work Wittgenstein did *before* what is in the *Notes on Logic*. In practice, this will involve us to a considerable extent at first in examining Russell's work during this period. Such an examination would be appropriate in any case, since it constitutes the context in which Wittgenstein was working. But the closeness and complexity of the working relationship between Wittgenstein and Russell makes the latter's work during this period of more than usual importance. It is clear that for much of this period they saw themselves as collaborators in a common project. So Russell's writings can give us significant clues to what Wittgenstein's own views were.

In my discussions of Wittgenstein's and Russell's work I shall generally adopt their logical notation. In particular, the reader needs to be familiar with the following:

$\sim p$	not-p
$p \vee q$	p or q
$p . q$	p and q
$p \supset q$	If p then q (in Russell's dubious idiolect: 'p implies q')
$(x).\phi(x)$	For all x, $\phi(x)$ (Russell: '$\phi(x)$ is always true')
$(\exists x).\phi(x)$	For some x, $\phi(x)$ (Russell: '$\phi(x)$ is sometimes true')

Russell usually used dots rather than brackets to indicate scope, but readers not at home with this convention can probably let the sense carry them through. It is also worth emphasizing at the outset that Wittgenstein did not show much sign of wanting Quine's distinction between use and mention. One of the emerging themes of the book will be what Wittgenstein owed to Frege and what to Russell, but one thing which a casual inspection of the *Notes on Logic* tells us is that he was not inclined while he was compiling them towards Frege's pedantry in the use of quotation marks: for Wittgenstein they are sometimes a naming device, but sometimes no more than a form of parenthesis. And Wittgenstein's propositional letters 'p', 'q', etc. are sometimes schematic, sometimes not. For instance, he plainly intended

'p' is true if and only if p

as a schema to stand for

'Snow is white' is true if and only if snow is white

and similar propositions. I shall take that as a general licence, in expounding Wittgenstein's thought, to be no prissier than he was about the use–mention distinction, except occasionally when it seems to matter.

Chapter 1

Finding a problem

What led Wittgenstein to study philosophy with Russell in Cambridge? The narrative of Wittgenstein's life before 1911 is well summarized in the available biographies,[1] so I shall confine myself in this chapter to picking out a few points that deserve emphasis in the current context.

1.1 Early life

Wittgenstein's father, a steel magnate, was one of the richest men in Austria. He was not an aristocrat, but Wittgenstein evidently acquired in his youth many of the attitudes of the rich. (One example, perhaps, is the tendency he had in the early part of his life to dabble in various fields, a tendency which displays a sort of enthusiastic amateurism sometimes to be observed in the independently wealthy.) Between the ages of 14 and 17 Wittgenstein was educated at the *Oberrealschule* in Linz. This was, notoriously, the same school as Hitler, but they overlapped for only one year (1903–4), during which Hitler was in class IIIA and Wittgenstein in class V, and there is no reason to think that Hitler influenced Wittgenstein's philosophy any more than that Wittgenstein influenced Hitler's anti-Semitism. Wittgenstein's Jewishness is in any case not a prominent theme in accounts of his early life: all four of his grandparents were baptized and, although he was no doubt aware of his Jewish roots, I know of no reason to think that he became at all self-conscious about them until much later. (Some of the remarks his acquaintances made about him when he came to Cambridge display casual amusement at the oddity of foreigners, but I have come across none that allude to his Jewishness.)[2]

In 1906, when he was seventeen, Wittgenstein went to Germany to spend three semesters studying engineering at the *Technische Hochschule* in Charlottenburg (a suburb of Berlin). Stories from his later life attest to his fascination with how things work, his capacity for spatial reasoning, and his ability to mend quite complicated pieces of machinery. Although this may have been to some extent a natural talent, the training at the *Technische Hochschule*, which emphasized the practical over the abstract, was specifically designed to foster

[1] McGuinness, *Young Ludwig*; Monk, *The Duty of Genius*. [2] Cf. McGuinness, 'Wittgenstein and the idea of Jewishness'.

it. On the other hand, the mathematical component of the training was rather limited: higher mathematics (i.e. differential and integral calculus and analytic geometry) in the first year of the course, and mechanics in both years. In addition there were what the course timetable[3] describes as descriptive geometry and graphical statics, but these will have been practical courses, more technical drawing than anything that we would nowadays regard as mathematics. This point bears emphasis, if only because some writers on Wittgenstein's philosophy have overestimated his mathematical knowledge—assuming, perhaps, that his training as an engineer included a substantial mathematical education.

Evidence that Wittgenstein made some sort of effort to extend his mathematical knowledge has survived in his copy of the German translation of Lamb's *Hydrodynamics*, which he presumably bought in Berlin around this time. What is curious about the volume is the markings Wittgenstein made in the margins—not so much the fact that they occur only in the first four of the twelve chapters of the book (although that does perhaps suggest that his interest was that of a *dilettante* rather than a serious student), but the nature of the comments themselves. Apart from a few corrections of obvious misprints in the text, Wittgenstein's marginalia are almost all rewordings that he seems to have regarded as stylistic improvements.[4] One might, of course, see this as an early sign of Wittgenstein's later deliberate interest in language and preciseness of expression, but there is something else about them too: the impression one has is almost that Wittgenstein was not really interested in the mathematics at all; or, if he was, one would like to have been able to explain to him that this was not the right way to go about studying a mathematical text.

1.2 Manchester

In 1908, after a short period constructing kites for meteorological research, Wittgenstein became a research student at Manchester. The university he was joining was one of the leading scientific research institutions in the world. It seems that he originally hoped to work with Rutherford, the professor of physics, who had just been awarded the Nobel prize; Chadwick (later to win the Nobel prize for discovering the neutron) was Rutherford's assistant; Geiger and Marsden were performing their famous experiments on the scattering of alpha particles; De Hevesy, who joined the department two years later, would receive the Nobel prize for his work on the use of isotopes as tracers. The chemistry department, unquestionably the finest in Britain, contained organic chemists such as Perkin, Haworth (who first synthesized vitamin C) and Robinson. In the event, though, Wittgenstein ended up working

[3]See Hamilton, 'Wittgenstein and the mind's eye'. [4]Spielt and McGuinness, 'Marginalia in Wittgenstein's copy of Lamb's *Hydrodynamics*'.

not with Rutherford but at the engineering laboratory, whose head, newly appointed in 1908, was Petavel, inventor of a device for measuring variations in pressure caused by exploding gases and later director of the National Physical Laboratory.

Manchester's strength was not confined entirely to the experimental sciences. In the mathematics department were Littlewood, one of the best mathematical analysts in the world, and Lamb. The presence in Manchester of Lamb, whose book Wittgenstein had been so idiosyncratically studying, may well have been one of the things that attracted Wittgenstein there. When he arrived, Wittgenstein lost no time in approaching Lamb with questions about some equations he had devised. As he related it to his sister, Lamb

> will try to solve the equations that I came up with and which I showed him. He said he didn't know for certain whether they are altogether solvable with today's methods and so I am eagerly awaiting the outcome of his attempts.[5]

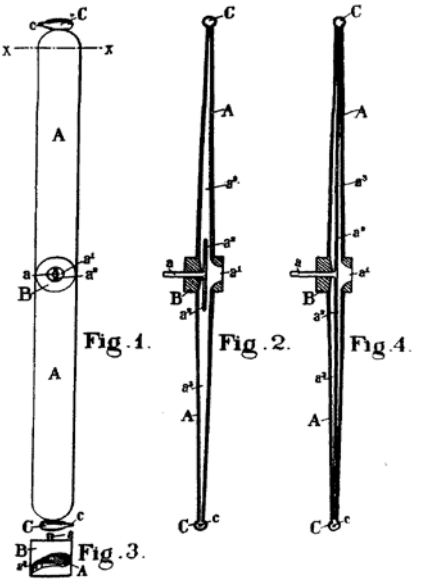
Perhaps this seemed (or was intended to seem) impressive to Wittgenstein's sister: he had come up with some equations which one of the foremost applied mathematicians of the day did not know how to solve! But of course what every applied mathematician knows is that devising equations one cannot solve is easy. The hard part is to model a system in such a way that the resulting equations *are* soluble.

His meeting with Lamb evidently mattered greatly to Wittgenstein at the time: his letter to his sister makes plain his extreme state of nervous tension throughout the day on which he made it. As things turned out, however, Lamb did not play the central role in his life that Wittgenstein seems to have hoped for; and it is hard to avoid the suspicion that what Wittgenstein was reporting was really a polite brush-off from a professor confronted with a somewhat eccentric student and some rather curious equations.

In his first year at Manchester Wittgenstein at least began to attend Littlewood's lectures on mathematical analysis, but we do not know how long he carried on. What we do know is that early in his time at Manchester Wittgenstein became interested in the philosophy of mathematics and, after three years at Manchester, decided to go to Cambridge to study with Russell. Quite how this interest in the philosophy of mathematics arose is something of a mystery, however. The account offered by McGuinness[6] would be hard to improve on, both as a summary of what is known and as a caution against speculation that goes beyond it.

Some commentators have presented Wittgenstein's interest in the philosophy of mathematics as flowing naturally from the mathematics he was engaged in, but I am sceptical about this since, as we have just seen, he had not

[5]To Hermione Wittgenstein, Oct. 1908. [6]*Young Ludwig*, ch. 3.



Wittgenstein's patented jet rotor

really done very much mathematics by this stage in his life. It has been common, perhaps in an attempt to make this account more plausible, to present Wittgenstein's research in Manchester as having a strong mathematical component; and it is true that the reminiscences of Eccles, Wittgenstein's friend during his time in Manchester and himself an engineer, refer to a theoretical aspect to Wittgenstein's work, but the only evidence we have of his engineering work, a patent he filed in 1910, leaves little trace of it.

The patent involved the idea of mounting a jet nozzle on each end of the rotor blade of a propeller. It is a curious mixture. There are certainly elements in the design that are original and farsighted: powered flight was still new in 1910, and the idea of using any sort of jet engine to power an aeroplane, although not itself original, had yet to reach the engineering mainstream. And the idea of placing sources of propulsion at the tips of a propellor, although again not original, was eventually used successfully thirty years later by another Austrian, Friedrich von Doblhoff, to construct a helicopter with no need of a tail rotor. On the other hand, Wittgenstein's implementation of the idea does not address the practical difficulties involved in turning it into a workable engine. One set of difficulties is created by Wittgenstein's idea of mounting combustion chambers on the end of rotating propeller shafts. The propeller blades would have to be very strong in order to withstand the stress generated by the very high moments of the rotating combustion chambers. This would (at least with the materials then available) require the blades to be heavy, fur-

ther increasing the forces involved. Nor does the patent address the difficulties involved in supplying fuel and oxygen to a combustion chamber on the end of an arm rotating at high speed. A further flaw is that his design has four separate combustion chambers: it would be difficult to control them, either independently or together, and any difference in thrust between the engines would put further strain on the propeller blades and make the assembly unstable. Wittgenstein described his idea in the patent as applying indifferently to 'aeroplanes, helicopters, dirigible balloons, or other forms of aerial machines', but the practical problems would probably be less serious in a helicopter, since the rotation rate of the rotor is typically lower than in an aeroplane propellor and the blades are much longer.

The point of labouring these design issues here is not so much to suggest that Wittgenstein was a poor engineer as to cast doubt on the common representation of him as a skilled mathematician. One could quite quickly estimate the moment generated by a combustion chamber rotating at high speed, and yet Wittgenstein's patent application takes no account of this. Indeed, it is very hard to believe that he made *any* calculations before he submitted it. The design shows ingenuity and imagination, it is true, but it would have needed a lot more work before it could become a practical engineering project.

1.3 The *Principles of Mathematics*

The plain, if somewhat unsatisfying, fact is that we do not really know what first led Wittgenstein to take an interest in the philosophy of mathematics. What we do know is that Wittgenstein's interest led him to Russell's *Principles of Mathematics*. How this came about is described in a much later reminiscence of Rush Rhees.

> Wittgenstein himself told me that while he was working in the Engineering Laboratory, he and two others doing research there began to meet for one evening each week to discuss questions about mathematics, or 'the foundations of mathematics'... At one of these meetings Wittgenstein said he wished there were a book devoted to these questions, and one of the others said, 'Oh there is, a book called *The Principles of Mathematics*, by Russell: it came out a few years ago.' Wittgenstein told me that this was the first he had heard of Russell: and that this was what led him to write to Russell and to ask if he might come and see him. I believe it was from *The Principles of Mathematics* that Wittgenstein learned of Frege.[7]

Wittgenstein may, for all we know, have read philosophy before this—he is reported[8] to have read Schopenhauer's *World as Will and Idea* in his youth, and as a consequence to have adopted for a time a version of epistemological idealism—but Russell's *Principles* is the first philosophical work whose influence

[7] *Recollections of Wittgenstein*, 213–14. [8] See von Wright, 'Biographical sketch', 5.

on him we can trace directly. (A copy which Wittgenstein bought in October 1912 has survived[9] but must surely be a duplicate or replacement for one he had already bought in Manchester.)

To modern readers (of whom there are not as many as one might expect, given its place in the history of the subject) Russell's *Principles* comes across as a transitional work: it contains extended passages which we can recognize as analytical philosophy in quite the modern sense, but these are juxtaposed to passages written in a style that strikes us as wholly antiquated, introducing bizarrely elaborate classifications for no apparent reason that develop into an architectonic of almost Kantian complexity. Whatever its faults, though, its influence on Wittgenstein is unquestionable. (Tradition has it that he continued to think highly of the book much later in his life.) It will therefore be in place for us to explain here some of the philosophical background from which it arose. Much of that background was supplied not by Russell but by Moore, from whom, on fundamental questions of philosophy, Russell said that he had derived all the chief features of his position.[10] Russell's later recollection was, more specifically, that the movement of which this book was part (a movement which led him to reject the neo-Hegelian idealism espoused by Bradley which was then popular in Britain) was born in conversations between him and Moore in 1898.[11] The first publications to exhibit this movement are Moore's articles on 'The nature of judgment' and 'The refutation of idealism', the central claim of which is that by conceiving of propositions as objective complex entities independent of any knowing mind we can resist the temptations of idealism. But if the overall shape of the project is clear, the details are not. The targets of Moore's criticism are broadly spread: although it is Bradley's post-Hegelian denial that absolute truth is ever attainable which is the principal target, at times Berkeley's view that *esse est percipi* or Kant's view that the relations the objects of experience bear to one another are supplied by the mind are also attacked.

Moore's conception of a proposition is embodied in two central doctrines. The first is that the entities of which a proposition is composed (which he called 'concepts') are themselves the items the proposition is about. Propositions are the objects of judgment, and the concepts that make up the proposition are therefore part of what we judge, but the view is nonetheless realist because this is 'no definition of them'; 'it is indifferent to their nature', he says, 'whether anyone thinks them or not'.[12] Concepts are, that is to say, objective entities, and a proposition consists of such entities somehow related so as to form a complex. Moore opposed this to Bradley's view that when I have an idea of something, that thing is itself part of the idea. This opposition is

[9]See Hide, 'Wittgenstein's books at the Bertrand Russell archive and the influence of scientific literature on Wittgenstein's early philosophy'. [10]*Principles*, xviii. [11]*MPD*, 54. [12]'The refutation of idealism', 4.

plainly not exhaustive of the possibilities, but once he had disposed (no doubt rightly) of Bradley's view, Moore seems to have seen no need of an argument for his own. Moore slid, that is to say, from conceiving of the components of a proposition as objective (which holds true, for instance, of Frege's senses) to concluding that they are the very same as the things the proposition is about (which does not). Nonetheless, the doctrine is central to the refutation of idealism as Moore conceived of it.

> Once it is definitely recognized that the proposition is to denote not a belief (in the psychological sense), it seems plain that it differs in no respect from the reality to which it is supposed merely to correspond, i.e. the truth that *I exist* differs in no respect from the corresponding reality *my existence*.[13]

It follows, Moore held, that truth

> does not depend upon any relation between ideas and reality, nor even between concepts and reality, but is an inherent property of the whole formed by certain concepts… The ultimate elements of everything that is are concepts.[14]

The lacuna in Moore's argument is significant for our present purposes because his conclusion—that a proposition must, if it is to be independent of the mind, contain parts of the external world—is one that Russell embraced wholeheartedly. Moreover, Russell did not, any more than Moore, consider at this stage any alternative resembling Frege's notion of the sense of a name. By the time Russell did come across Frege's conception, he seems to have been too deeply embedded in his own to be able to engage with it. When he confirmed in response to Frege's query that 'in spite of all its snowfields Mont Blanc itself is a component part of what is actually asserted in the proposition "Mont Blanc is 4000 metres high" ', he offered as his reason that 'if we do not admit this, we get the conclusion that we know nothing at all about Mont Blanc',[15] but did nothing to explain why this should follow. This disagreement between Frege and Russell is often expressed in terms of names rather than sentences. For Russell the part of the proposition that corresponds to the proper name 'Mont Blanc' is the mountain itself; for Frege it is not the mountain but the sense of the name. Russell's was, that is to say, what is sometimes called a one-step, Frege's a two-step semantic theory.

Moore's second central doctrine was that there are no internal relations between concepts—no relations between concepts that are part of the nature of the concepts related. What it is for a proposition to be true is just for the concepts it is composed of to be externally related to each other in a certain way. Once again, it is easy to see what the target is. Bradley had held that all relations are internal, and had concluded as a result that since in particular knowledge must be conceived of as an internal relation between the knower

[13]'Truth'. [14]Moore to BR, 11 Sep. 1898 (Griffin, *Russell's Idealist Apprenticeship*, 300). [15]To Frege, 12 Dec. 1904.

and the proposition known, the simple act of coming to know the proposition will turn it into something different from what it was. No truth, according to Bradley, is wholly true; truth is 'subject always to degree'.[16]

There is room to doubt whether Moore meant by 'internal' the same as Bradley. And even if Moore was right to reject Bradley's extreme conclusion that nothing is ever wholly true, it is much less clear why Moore should have said that there are *no* internal relations between concepts at all: as in the case of the first doctrine, he seems to have been oblivious of the need for an argument. Nonetheless, once again Moore's view was shared by Russell, who as early as 1899 confidently asserted that '*all* relations are external'.[17]

The shadow cast by these two doctrines, that names refer directly to their objects without the mediacy of sense, and that there are no internal relations, is long. For they were both not only adopted by Russell but maintained, in a certain sense, by Wittgenstein in the *Tractatus*. Wittgenstein did not maintain that propositions contain the parts of the world they are about, but he did side with Russell against Frege in rejecting[18] the notion that names have sense as well as reference. And his doctrine[19] of the logical independence of elementary propositions can be thought of as a reexpression in Tractarian terms of Moore's denial of internal relations between objects. Moreover, it is notable that the *Tractatus* contains hardly any argument in support of either claim. Indeed neither of them is discussed in Wittgenstein's surviving pre-Tractarian writings. It might seem perverse, therefore, for me to stress these two views in a book about the *Notes on Logic*, in which they do not occur. My ground for mentioning them nonetheless is that I think the most plausible explanation for Wittgenstein's failure to discuss them is that he never saw any reason to question them; and indeed they became so embedded in his conception that, like Russell, he found it hard to see the need for argument.

1.4 Logicism

The doctrine that there are no internal relations between concepts runs into an obvious difficulty in the case of identity statements. If the identity '$a = a$' expresses anything about a—a relation between a and itself—it seems clear that this must be internal. So if there are no internal relations, we are forced to conclude that it does not express anything at all. This is perhaps not so bad in itself, but we shall need to say something about the identity 'Hesperus = Phosphorus', which, apparently at least, expresses genuine astronomical information. And a lot more will have to be said about arithmetic, in which apparently informative identity statements (such as '$7 + 5 = 12$') play such a central role.

[16] *Appearance and Reality*, 321. [17] *CP*, II, 143. [18] 3.3. [19] 5.134.

The work in which this was first attempted was Russell's *Principles*. What Russell added to Moore's conception of propositions in order to account for arithmetic (and indeed for mathematics more generally) was the notion of a denoting concept. A denoting concept is what one might call an 'aboutness shifter':[20] its task is to enable a proposition to be about something else that is not itself part of the proposition. On the view that Russell had derived from Moore, let us recall, the proposition expressed by the sentence 'I met John' contains me, John, and the universal *meeting*. The proposition expressed by 'I met a man' will similarly have to contain me, meeting, and a third element expressed by the phrase 'a man'. But what is this third element? It cannot be any particular man, since it is just the same proposition whichever man it was that I actually met. We seem forced by this to hold that the third element is a concept that is somehow related to whatever man I might have met; but this concept, if a constituent of the proposition, is not one of the things the proposition is about.

> The proposition is not about *a man*: this is a concept which does not walk the streets, but lives in the shadowy limbo of the logic-books. What I met was a thing, not a concept, an actual man with a tailor and a bank-account or a public-house and a drunken wife.[21]

Yet there must be *some* connection between the man with the bank-account and the propositional component in question. In the *Principles* Russell calls the propositional component a *denoting concept*—elsewhere sometimes a *denoting complex*—and the relation it has to the man that of *denoting*. 'A concept *denotes* when, if it occurs in a proposition, the proposition is not *about* the concept but about a term connected in a certain peculiar way with the concept.'[22]

Russell seized on denoting as a central element in the epistemology of mathematics.

> The concept *all numbers*, though not itself infinitely complex, yet denotes an infinitely complex object. This is the inmost secret of our power to deal with infinity. An infinitely complex concept, though there may be such, can certainly not be manipulated by the human intelligence; but infinite collections, owing to the notion of denoting, can be manipulated without introducing any concepts of infinite complexity.[23]

A proposition about all numbers therefore does not itself contain all numbers; rather it contains a concept which denotes all numbers. The concept is finite, and hence capable of being grasped by our finite intelligence, even though what the concept denotes is infinite. In the *Principles* denoting concepts thus act as the bridge between what we are capable of grasping directly and what we are not; they enable a proposition to be about something (in this case the class of natural numbers) which is in a certain sense out of our reach.

[20] Makin, 'Making sense of "On denoting"'. [21] *Principles*, §56. [22] Ibid. [23] *Principles*, §72.

Merely to invoke the notion of denoting is not, plainly, to explain mathematics. A second element in the development of Russell's views occurred in 1900 when he attended the International Congress of Mathematicians in Paris and learned for the first time of the work of Peano, which demonstrated the expressive power of symbolic logic in expressing mathematics. The claim Russell made in the *Principles* was that all the propositions of mathematics could be reexpressed in the vocabulary of classes and would thereby turn out to be truths of logic.

Peano's work focused largely on the task of expressing the theorems of mathematics: he was much less concerned with the issue of how to prove them. However, a theory which aimed to achieve this had been developed by Frege, first in his *Begriffsschrift* and then in more detail in the *Grundgesetze*. Russell had been given a copy of the *Begriffsschrift* by Ward (one of the philosophers at Trinity), but had not read it.[24] He became acquainted with the first volume of the *Grundgesetze* around the beginning of 1901, but wrote the *Principles* in ignorance of most of Frege's writings. Only when the main text was complete did he make a study of them and add an appendix summarizing and criticizing them.[25]

1.5 Russell's paradox

Shortly before this, however, Russell had discovered a problem not just for Frege but for any prospect of a logicist reduction of mathematics. The problem was that we cannot unproblematically assume that for every propositional function ϕ there is an extensional entity, the class of ϕs, corresponding to it. To see why not, consider the class K of all those classes which do not belong to themselves. Then for every class x it is the case that x belongs to K if and only if x does not belong to itself. In particular, then, K belongs to K if and only if K does not belong to K. This is a contradiction, and the argument that leads to it is known as Russell's paradox.

We shall come in a later chapter to the elaborate theory Russell eventually devised to get round this difficulty. In the *Principles* he did no more than sketch the outline of a 'theory of types' that might resolve the matter, leaving the details as a matter requiring further work.

> The totality of all logical objects, or of all propositions, involves, it would seem, a fundamental logical difficulty. What the complete solution of the difficulty may be, I have not succeeded in discovering; but as it affects the very foundations of reasoning, I earnestly commend the study of it to the attention of all students of logic.[26]

[24]Russell, *Autobiography*, 65. [25]See Linsky, 'Russell's notes on Frege for Appendix A of *The Principles of Mathematics*'. [26]*Principles*, §500.

Perhaps it was natural that Wittgenstein would be intrigued by this problem and would take up Russell's recommendation to attempt a solution. The first evidence we have of Wittgenstein working on any philosophical problem dates from April 1909, when a friend of Russell called Philip Jourdain made the following note in his correspondence book.

> Russell said that the views I gave in a reply to Wittgenstein (who had 'solved' Russell's contradiction) agree with his own… In certain cases (e.g., Burali-Forti's case, Russell's 'class',… Epimenides' remark) we get what seem to be meaningless *limiting cases* of statements which are not meaningless.[27]

Jourdain was perhaps a natural person to approach, as he had already published on the topic,[28] but there is also something characteristic of Wittgenstein—his blend of confidence and diffidence—in the fact that he did not write to Russell himself but to someone he may well have known was in contact with him. Wittgenstein's letter to Jourdain has not survived: all we have is Jourdain's description just quoted of the views of his own which he offered in reply. It is hardly likely, though, that Wittgenstein, a self-taught novice, had come up with a 'solution' to the paradoxes of any interest or subtlety; and Jourdain, whose correspondence does not elsewhere display much grip on the concept of tact, will no doubt have explained his error to him with clinical directness.

Wittgenstein was thin-skinned at the best of times, and a brush-off from Jourdain might on its own be enough to account for the fact that it was another two years before he felt able to approach Russell in person. However, he may equally have been influenced by his father, who (at least on Wittgenstein's presentation of the matter) was 'disappointed in all his other sons' and 'very anxious this one should do something respectable like engineering and not waste his time over such nonsense as philosophy'.[29] His elder sister's later recollections leave us in no doubt about the strength of the conflict he felt during this period.

> Reflection on philosophical problems suddenly became such an obsession with him, and took hold of him so completely against his will, that he suffered terribly, feeling torn between conflicting vocations… It shook his whole being… During this time Ludwig was in a constant, indescribable, almost pathological state of agitation.[30]

The issue that so piqued Wittgenstein's interest was unquestionably important. As soon as it was discovered, Russell's paradox became the central problem in the philosophy of mathematics, a position it held at least until the publication of Gödel's incompleteness theorems in 1931. The attraction for Wittgenstein, a young man of ambition and talent searching for a field in which to make his mark, is therefore easy to understand: the problem was

[27]Jourdain's correspondence book, 20 Apr. 1909, quoted in Grattan-Guinness, *Dear Russell—Dear Jourdain*, 114. [28]E.g. 'On the question of the existence of transfinite numbers'. [29]BR to OM, 7 Mar. 1912. [30]Quoted in Rhees, *Recollections of Wittgenstein*, 2.

recent, simple, and intriguing; and a satisfying solution to it would be certain to bring its author attention. Not only was this apparently the first philosophical problem Wittgenstein worked on seriously, but the desire to solve it was probably what drew him to Cambridge, and therefore to philosophy as a career rather than a hobby (to the extent that a man of means like Wittgenstein recognized that distinction). At any rate, it was the philosophy of mathematics rather than philosophical logic that he stated to Russell as his interest when he arrived at Cambridge;[31] and there is ample evidence (in the diaries of his Cambridge friend David Pinsent, for example) that he continued throughout the two years he spent there to regard solving the paradoxes as one of his principal ambitions.

[31] BR to OM, 18 Oct. 1911.

Chapter 2

First steps

Whatever philosophical study Wittgenstein undertook in Manchester, it was plainly not something that could be described as a training and, although earlier influences on Wittgenstein are no doubt relevant at various points, any study of the genesis of the *Tractatus* naturally begins in earnest with his arrival in Cambridge to study under Russell. (Later, indeed, he told Ramsey[1] that the book had taken seven years to write, thus implicitly identifying his arrival in Cambridge as the point at which his work on the *Tractatus* really began.)

Wittgenstein once advised his friend Eccles of the importance of going to work in really first-class places.[2] In his choice of Cambridge (more particularly, Trinity) in 1911, as of Manchester in 1908, he certainly took his own advice. Even if it was not the centre of the universe that some of its members were (and are) inclined to suppose it, a college that boasted Russell, Moore, McTaggart, and Hardy—Whitehead had only recently resigned his Cambridge post and gone to work in London—was plainly the best place Wittgenstein could have chosen to pursue an interest in the philosophy of mathematics.

Wittgenstein's decision to approach Russell as a possible supervisor also conforms to a pattern. In 1908 he had sought out Lamb, whose book on *Hydrodynamics* he had been studying; now it was the author of the *Principles* he went to see. Russell's description[3] of their first meeting, in October 1911, suggests that Wittgenstein was in as extreme a state of nervousness as he had been when he met Lamb. This time, though, the outcome would be vastly more fruitful.

2.1 Cambridge

During that first Michaelmas Term Wittgenstein's attendance at Russell's lectures on the foundations of mathematics was evidently something of a trial subscription. He was still officially a research student at Manchester, had not matriculated as a member of Cambridge University, and presumably attended the lectures only on Russell's sufferance, staying in rented accommodation in

[1]Ramsey to his mother, 20 Sep. 1923. [2][July 1912]. [3]To OM, 18 Oct. 1911.