

## **CAMBRIDGE PHILOSOPHERS I**

**The first in a series of articles on Cambridge philosophers based on lectures originally given in Cambridge in 1991.**

### **F. P. Ramsey<sup>1</sup>**

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Frank Plumpton Ramsey was born in February 1903, and he died in January 1930—just before his 27th birthday. In his short life he produced an extraordinary amount of profound and original work in economics, mathematics and logic as well as in philosophy: work which in all these fields is still, over sixty years on, extremely influential.

Ramsey was a member of a distinguished Cambridge family, in which he was the eldest of two brothers and two sisters. Their father was the mathematician A. S. Ramsey, who was President—i.e. Vice-Master—of Magdalene College; and Frank's younger brother Michael went on to become Archbishop of Canterbury. In a radio programme about Ramsey which I compiled in 1978,<sup>2</sup> Lord Ramsey described how he and his elder brother Frank got on as teenagers:

Though we were at different schools, in holiday times we saw a great deal of each other and we spent a lot of time together hitting a tennis ball against the wall, the rudiments of squash rackets, or bowling a ball to each other in a wicket or that sort of thing: playing together, just us two, and talking a great deal about all sorts of things. He was interested in almost every-

<sup>1</sup> This is a slightly revised version of the text of a public lecture given in Cambridge on 27 February 1991, one of a series on Cambridge philosophers arranged for the University's Philosophy Faculty by Renford Bambrough. It draws heavily on a radio broadcast about Ramsey, 'Better than the stars', which I wrote and presented on BBC Radio 3 on 27 February 1978. Besides the interest of its subject matter, the lecture is reproduced here for two reasons: first, with the kind permission of those concerned, to put on record the contributions made to my broadcast by Ramsey's family and friends and by philosophers influenced by his work; and second, as a tribute to Mr Bambrough's distinguished and distinctive editing of *Philosophy* over many years. I am indebted to his successor, Professor Anthony O'Hear, for suggesting revisions to the text as delivered and, in particular, the reference to Quine.

<sup>2</sup> See note 1.

thing. He was immensely widely read in English literature; he was enjoying classics though he was on the verge of plunging into being a mathematical specialist; he was very interested in politics, and well-informed; he had got a political concern and a sort of left-wing caring-for-the-underdog kind of outlook about politics.

I was aware that he was far cleverer than I was and knew much more, yet there was such a total lack of uppishness about him that we just conversed in a friendly way and he never made me feel inferior though I was so vastly below par intellectually, and that was the wonderful joy of it.

The teenage Ramsey didn't only impress his admiring younger brother. Living in Cambridge, with his father at Magdalene, he met and impressed other Cambridge academics even before he went up to Trinity College in 1920 to read mathematics as an undergraduate. In particular, he impressed two redoubtable Fellows of Magdalene, C. K. Ogden and I. A. Richards, who had very wide interests in what were then called the Moral Sciences. In my broadcast I. A. Richards recalled their very first meeting with Ramsey:

Well, my old friend C. K. Ogden had a very queer place called 'Top Hole'—named after a war cartoon—above MacFisheries in Petty Cury, and one afternoon there, a tap on the door and in came this tall, ungainly, rather gangling boy. We knew who he was instantly—he looked so like his mother—and in no time almost he was at home. He was from Winchester where he'd been for some time with no one doing much more than saying 'The library is yours, just do what you want'. He was recognized clearly at Winchester as quite one of the wonders; and there he was, and we chatted along for some time, and then he turned to Ogden and said: 'Do you know, I've been thinking I ought to learn German. How do you learn German?'. Ogden leaped up instantly, rushed to the shelf, got him a very thorough German grammar—and a dictionary, Anglo-German dictionary—and then hunted on the shelves and found a very abstruse work in German—Mach's *Analysis of Sensations*<sup>3</sup>—and said: 'You're obviously interested in this, and all you do is to read the book. Use the grammar and use the dictionary and come and tell us what you think'. Believe it or not, within ten days, Frank was back saying that Mach had misstated this and that he ought to have developed that argument more fully, it wasn't satisfactory.

<sup>3</sup> E. Mach, *The Analysis of Sensations* (New York: Dover, 1959).

He'd learned to read German—not to speak it, but to read it—in almost hardly over a week.

Some time later, Ramsey had an opportunity to exercise his rapidly acquired mastery of German on a work that was even more abstruse than Mach's *Analysis of Sensations*: Wittgenstein's *Tractatus Logico-Philosophicus*.<sup>4</sup> Wittgenstein had written this in German and in a peculiarly condensed style. Ogden wanted to publish an English translation of it; but getting one which would do justice to the original was no easy matter, as I. A. Richards explained:

Well, he'd had a very hard time with the *Tractatus*, and all sorts of people were called in, and didn't like any version they could make of it. They couldn't make it make as good sense in English as—if it made any good sense in German—they thought it should. Moore had been insisting very much that it wasn't translatable—it would be much better left just as it was. After inventing a title—Moore's title—one way and another it got into a kind of discard; and then I don't know who suggested that Frank ought to have a try at it, and as soon as Frank and Wittgenstein got together over this it was clear that there was a possibility.

The *Tractatus* impressed Ramsey enormously. He, like everyone else, found it exceedingly difficult, and he took immense trouble to try and understand it. In the autumn of 1923, after graduating from Trinity College as a Wrangler—i.e. with a First in Part II of the Mathematics Tripos—he went to Austria to visit Wittgenstein, who was then living in a small village outside Vienna. In a letter home, Ramsey gave a vivid picture of Wittgenstein's life and of the intensity of their conversations.

Wittgenstein is a teacher in the village school. He is very poor, at least he lives very economically. He has one *tiny* room, white-washed, containing a bed, washstand, small table and one hard chair and that is all there is room for. His evening meal which I shared last night is rather unpleasant coarse bread, butter and cocoa. His school hours are eight to twelve or one and he seems to be free all the afternoon. He is prepared to give four or five hours a day to explaining his book. I have had two days and got through seven out of eighty pages. He has already answered my chief difficulty which I have puzzled over for a year and given up in despair myself and decided he had not seen. It's terrible when he says 'Is that clear?' and I say 'No' and he says 'Damn, it's *horrid* to go through all that again'.

<sup>4</sup> L. Wittgenstein, *Tractatus Logico-Philosophicus* (London: Routledge, 1922).

Ramsey had a particular professional reason for being interested in Wittgenstein's *Tractatus* at that time. He was after all a mathematician, and mathematics was naturally amongst his earliest philosophical interests: what is mathematics, how does it differ from other subjects, and in particular, what makes mathematical truths true? There were at that time—there still are—various competing answers to those questions. The view of Russell and Whitehead was that mathematics is simply an elaboration of logic: that logic is what makes mathematical truths true. They had tried to prove this in their monumental work, *Principia Mathematica*,<sup>5</sup> published in three volumes between 1910 and 1913, by deriving the whole of mathematics from purely logical principles.

By the early nineteen-twenties, however, their conception of mathematics as logic had run into serious difficulties. At the root of these, Ramsey thought, was Russell's treatment of mathematics as consisting of propositions statable in purely logical terms—or, as Ramsey put it, 'completely general propositions . . . not about particular things and relations, but about some or all things and relations'. But, Ramsey continued,

It is really obvious that not all such propositions are propositions of mathematics or logic. Take for example 'Any two things differ in at least thirty ways'; this is a completely general proposition and it may well be true. But as a mathematical or logical truth no one could regard it; it is utterly different from 'Any two things together with any other two things make four things', which is a logical and not merely an empirical truth.<sup>6</sup>

But what then *is* a logical truth? *Principia Mathematica* doesn't say. The *Tractatus* does: a logical truth is a tautology, that is, roughly, a proposition that turns out true whether or not any other proposition turns out true. But not all tautologies are mathematical: 'Either it's raining or it's not' is a tautology, since it will turn out true whatever the weather, but it isn't mathematical because it isn't general enough. 'Any two things differ in at least thirty ways', on the other hand, is general enough: it isn't mathematical because it isn't a tautology. Mathematical propositions, Ramsey said, must be both: completely general in content, and tautological in form.

This paper of Ramsey's on 'The foundations of mathematics' was the culmination of the *Principia Mathematica* programme of reducing mathematics to logic. It was read to the London Mathematical

<sup>5</sup> A. N. Whitehead and B. Russell, *Principia Mathematica* (Cambridge University Press, 1913).

<sup>6</sup> F. P. Ramsey, *Philosophical Papers*, D. H. Mellor, (ed.) (Cambridge University Press, 1990), 167.

Society in 1925, when Ramsey was twenty-two. And the way in which, like most of his papers, it took off from the work of other philosophers, illustrates something quite important both about Ramsey and about philosophy. Ramsey's friend and contemporary, Richard Braithwaite, who subsequently became the Knightbridge Professor of Philosophy at Cambridge, and died in 1990, reacted with characteristic vigour when in my broadcast I rather incautiously contrasted Ramsey and Russell in this matter of originality:

You're entirely wrong. Russell started completely from his curiosity about the foundations of mechanics, which was doubtless Mach, and Russell went on from this. I think you've got it completely wrong. Look here, Ramsey was not the creative beat artist, he was not a free expressionist in the slightest, certainly not. I really protest at your notion of what originality in a subject like philosophy consists in. It doesn't start in having original ideas *ab initio*: what it consists in is thinking of something which is an improvement on previous thinking. This is what Ramsey did in great numbers of spheres.

Another sphere in which Ramsey thought of an improvement on previous thinking was probability. The economist John Maynard Keynes, to whom Braithwaite introduced Ramsey in 1921, published his *Treatise on Probability*<sup>7</sup> in August of that year. In this work Keynes interpreted probability as measuring a logical relation of 'partial entailment' between propositions which we can detect *a priori* and which tells us how far our inductive evidence for a scientific hypothesis supports that hypothesis. His interpretation was later taken up by major philosophers of science, notably by Rudolf Carnap, who made it the basis of the inductive logic published in 1950 in his *Logical Foundations of Probability*.<sup>8</sup> But it did not satisfy Ramsey, whose objections to it—some of them published before he was nineteen—were so cogent and comprehensive that Keynes himself abandoned it. Ramsey's basic objection, to the whole idea of an inductive logic, is what he called

the obvious one that there really do not seem to be any such things as the probability relations [Keynes] describes . . . anyone who tries to decide by Mr Keynes's methods what are the proper alternatives to regard as equally probable in molecular mechanics

<sup>7</sup> J. M. Keynes, *A Treatise on Probability* (London: Macmillan, 1921).

<sup>8</sup> R. Carnap, *Logical Foundations of Probability* (Chicago: University of Chicago Press, 1950).

. . . will soon be convinced that it is a matter of physics rather than pure logic.<sup>9</sup>

These quotations come from Ramsey's paper on 'Truth and probability', first read to the Cambridge University Moral Sciences Club in 1926. In this paper, after criticizing Keynes, Ramsey went on to produce his own theory. This starts from the fact that people's actions are largely determined by what they believe and what they desire—and by the strength of those beliefs and desires. The strength of people's beliefs is measured by the so-called 'subjective probability' they attach to events. When people say it will probably rain, for example, at least part of what they mean is that they believe that it will rain more strongly than that it won't rain. But what they do as a result of this belief—for example, whether they take umbrellas with them when they go out—depends also on what they want: for example, on whether, and how much, they desire to avoid getting wet: or, in other words, on the so-called 'subjective utility' they attach to keeping dry. Subjective utility measures the strength of people's desires just as subjective probability measures the strength of their beliefs.

The problem is how to separate these two component causes of people's actions. A woman rushes out of doors bareheaded: is it that she wants to stay dry but expects sunshine? Or is it that she expects rain but for some reason wants to get wet? One of the things Ramsey's paper did was to show how to extract people's subjective utilities and probabilities from the choices they make between different gambles; and by doing so it laid the foundations for the serious use of these concepts in economics and statistics as well as in philosophy.

It took a long time, however, for this 1926 paper of Ramsey's to bear fruit. Only after the publication in 1944 of a now classic book by John von Neumann and Oskar Morgenstern, *The Theory of Games and Economic Behaviour*,<sup>10</sup> did utility theory begin to catch on and be applied in modern decision theory and games theory. And for many years no one realized how much of it had been anticipated in Ramsey's 1926 paper. And as with utility, so with probability, as Richard Jeffrey, the Princeton philosopher of probability and author of the equally classic book *The Logic of Decision*,<sup>11</sup> recalled in my broadcast:

<sup>9</sup> *Philosophical Papers*, 57, 85.

<sup>10</sup> J von Neumann and O. Morgenstern, *Theory of Games and Economic Behavior* (Princeton University Press, 1944).

<sup>11</sup> R. C. Jeffrey, *The Logic of Decision* (Chicago: University of Chicago Press, 1965).

It was when Leonard Savage, statistician, was working on his book on subjective probability theory, and he wished to find out what if anything the philosophers had to say on the subject, he went to the Ramsey article and read it, and he found that what he had done was to a great extent fairly describable as rediscovering another aspect of Ramsey's work in that article—the foundations of the theory of subjective probability. It was Savage's book, *The Foundations of Statistics*,<sup>12</sup> that was published in 1954, that made subjectivism a respectable sort of doctrine for a serious statistician to maintain; and the remarkable thing is that Ramsey in this little paper to the Moral Sciences Club in 1926 had done all of that already, but somehow he wasn't speaking to the right audience or the audience wasn't prepared or something, but it was only sometime later that his ideas were rediscovered by people to whom the learned were attuned somehow or other.

Why Ramsey's ideas on probability, utility and other matters weren't picked up in Cambridge at the time, in the nineteen-twenties and thirties, isn't very clear. Braithwaite was himself a considerable mathematician and philosopher of probability, and he included Ramsey's paper on 'Truth and probability' in the posthumous collection of Ramsey's work which he edited and published in 1931.<sup>13</sup> He could well have taken up and developed Ramsey's ideas in the thirties, but he didn't, as he apologetically admitted in my broadcast:

Now with regard to why his views of probability weren't accepted more, I'm sorry, I think I am myself to blame to a certain extent, because I edited the works and I thought they were very interesting; but this was the moment when Wittgenstein had descended on Cambridge and all of us in Cambridge took the next ten years trying to digest Wittgenstein.

With the benefit of hindsight, I think Braithwaite at least might usefully have spent some of those ten years trying to digest Ramsey; and indeed that, but for Ramsey's early death, Wittgenstein's own work might have been digested rather less uncritically. As A. J. Ayer put it in my broadcast,

There is a good deal of evidence that Wittgenstein had a respect for Ramsey's opinion and that Ramsey didn't swallow

<sup>12</sup> L. J. Savage, *The Foundations of Statistics* (New York: Wiley, 1954).

<sup>13</sup> F. P. Ramsey, *The Foundations of Mathematics and other Logical Essays*, R. B. Braithwaite, ed. (London: Routledge & Kegan Paul, 1931).

Wittgenstein whole. And I don't think that Wittgenstein's dominance in Cambridge in the thirties was at all a healthy thing for Cambridge philosophy. Moore, in some curious way—I think, in his modesty, in a way—felt himself not equipped to take issue with Wittgenstein. I think Ramsey would have done this, and would perhaps have stopped Wittgenstein from going in a direction that I've never thought at all a happy one.

But Wittgenstein's dominance in Cambridge in the thirties, healthy or otherwise, was certainly not the only, nor even the main, reason why most of Ramsey's work—not only his work on probability—was neglected or underestimated for so long. Another reason is, as Moore remarked in his Preface to Braithwaite's collection of Ramsey's work, that Ramsey sometimes

fails to explain things as clearly as he could have done, simply because he does not see that any explanation is needed: he does not realize that what to him seems perfectly clear and straightforward may to others, less gifted, offer many puzzles.<sup>14</sup>

Then there is the plainness and what Keynes called the 'easy grace' of Ramsey's writing, which is undoubtedly apt to conceal at first sight the originality, depth and precision of his thought. He does make it all look very light and easy—until one tries to think through the matter for oneself. That may well be one reason why he had a less immediate influence than Wittgenstein did. The final words of the *Tractatus*, for example, 'Whereof one cannot speak, thereof one must be silent' in the original English translation, positively reek of profundity. Whereas Ramsey's 'What we can't say we can't say, and we can't whistle it either'<sup>15</sup> sounds much less impressive, although in fact it sums up a serious objection to the *Tractatus*, whose approach Ramsey was, incidentally, influential in persuading Wittgenstein to abandon.

But I think the main reason it took so long for Ramsey's work to be properly taken up is just that it was so original and so hard, and he himself was so unassertive. As I. A. Richards put it:

It may have been a bit difficult for most people, even in his own new fields (it's the newer fields I think that have become so recognized), that might be it; and the other thing is of course that he never was a showman at all, not the faintest trace of trying to make a figure of himself. Very modest, gentle and on the whole he refrained almost entirely from argumentative controversy . . .

<sup>14</sup> *The Foundations of Mathematics*, viii.

<sup>15</sup> *Philosophical Papers*, 146.



Very interesting, he wasn't a controversialist at all. He felt too clear in his own mind, I think, to want to refute other people.

A trait which Braithwaite confirmed:

He didn't dominate conversations. He tended not to take the initiative, but if of course he was asked his opinion on something he would then speak in a paragraph or a few paragraphs. If he had something to say, he'd go on about it.

But Ramsey didn't 'go on' only about his work. He didn't achieve his extraordinary output in philosophy, economics, mathematics and logic by devoting himself to it full time. On the contrary, as his wife Lettice recalled:

He never worked for long hours. I think the work was so exacting that I wouldn't have said he worked for more than say four hours a day . . . he worked in the mornings, probably went for walks in the afternoon, played the gramophone in the evening. Something of that sort.

Music was in fact one of Ramsey's main interests, as Lettice Ramsey explained:

Oh he was immensely keen about music. He'd come on music I think fairly late, in his late teens or early twenties, and he was very very keen. He spent a lot of time listening to records, and of course the records in those days were not nearly as good as they are now, so always great efforts were made to get a better gramophone and a better sound box and all that; but he spent a lot of time on classical music, and he was very interested in Wagner. And going to concerts too. We both went. Well, I'm very interested in music too, but not as keen as Frank used to be. We went to the Wagner opera in London, but concerts in Cambridge.

Ramsey had first met his future wife, Lettice Baker, when they were both students: he in his first year, she in her third. But they didn't really get to know each other until she came back three years later to work in the psychology laboratory. Then, as she put it,

Frank remembered that he'd met me and invited me round to tea and then we got to know each other and that was how it happened really,

—and they were married in September 1925.

Lettice Ramsey had in fact studied philosophy as an undergraduate, but by the time she came back to Cambridge she had turned

to psychology, and they didn't discuss their work with each other much. As she said,

All the philosophy and ethics and logic went in one ear and out at the other, I think. That was far above my head. It wasn't my subject, really, at all; and he wasn't a psychologist. So we didn't discuss psychology.

What they did do was go hill-walking:

We went to the Pyrenees once and walked over onto the Spanish side and got lost. Then we had to cross back into France and struck a snowstorm and really would have got lost if we hadn't been taken in by miners: they popped us into the beds of the night shift, where we thawed out, and then conducted us over the frontier the next day. So we really had some very splendid walking holidays: walking from place to place, not staying in one place. We never went up any very high mountains—it was mountain walking rather than climbing. He was very afraid of heights, so that if we went any place where there was a height I had to hold his hand and lead him over the bad places.

Ramsey's head for heights hadn't always been that bad. I. A. Richards remembered an earlier excursion:

Well, he hadn't been and clambered about rocks ever, and he was kind enough to say he'd come and spend about a week in North Wales in midwinter—icy weather too, not good weather at all. And we got along very very nicely, did several quite decent climbs. I did feel when I had him on the rope that I had to be quite sure that I'd got a very solid belay because he was not quite built for climbing, and he'd be heavy in a fall. But he never fell off, it was all right, we got along very well, and he enjoyed it so much, you see. That was the point: he seemed to take a liking to it. But he never would have made a climber. No.

So there was walking, gramophone records, Cambridge concerts, London opera, conversation with his friends. Lettice Ramsey described her husband as

a very easy-going, very natural person, extremely natural. I mean, no inhibitions, very unshy of any ordinary sort of things. I mean, as people are now much more than perhaps they used to be. Very untidy: didn't care a hoot about what he wore, what he looked like. Had very untidy, rather poor hair. Was a very tall man—he was six foot three and bulky. No I wouldn't have said fat, but weighed sixteen stone or so.

Braithwaite added that Ramsey's head

was of a shape which is rather rare—he had a large head and it was pentagonal, the chin being the point. He was rather short-sighted and wore spectacles, steel-rimmed spectacles in those days. His movements appeared to be rather clumsy but as a matter of fact he was quite a good lawn tennis player, and when he became interested in music he was extremely adept at changing gramophone records. But he gave an impression to start with of clumsiness and that he would knock over furniture in a room—he didn't, matter of fact. Had a very loud laugh which was infectious.

Ramsey's good humour and tolerance could surmount quite profound differences on matters which he took very seriously, like religion. Lettice Ramsey described him as

a militant atheist—definitely. We were married in a Registry Office, wouldn't have dreamed of being married in a church, and he was really quite a militant atheist, not an agnostic—he was an atheist.

But when I asked her therefore how Ramsey got on with his younger brother, she replied 'Oh, I think on very friendly terms even though their outlook was so different'—an impression which Lord Ramsey confirmed:

Well, in I suppose early teenage stage he had been religious up to a point. He dropped religion, rejected it as an unsound and irrelevant occupation, and did move right away from it. He was certainly sorry that I went on being religious; he was sorry that I decided to become a priest in the Church of England; sorry indeed, but quite tolerant. In the last year or two of his life it came through to me that, while his non-religious attitude in general remained, he was aware of mysticism as a kind of phenomenon worth studying empirically, and even up to his last illness we had one or two talks about that, and I was struck by this readiness to respect mysticism as an empirical phenomenon.

Ramsey's friendships with his colleagues too seem to have been largely unaffected by his criticisms of their ideas. Keynes, for example, despite Ramsey's demolition of his theory of probability, encouraged his own college, King's, to make Ramsey a Fellow in 1924 at the ripe age of twenty-one—only the second non-Kingsman to become a Fellow of King's. Ramsey then began to lecture for the Mathematics Faculty on the foundations of mathe-

matics, and in 1926 he was made a University Lecturer in Mathematics, the post he held until his death four years later.

Given that he was a lecturer in mathematics, it's curious that on mathematics itself, as opposed to its foundations, Ramsey only published eight pages; and those eight pages have since had an even more curious history, as Dick Jeffrey recalled:

In the course of solving a problem in formal logic he thought that he needed to state and prove an abstract mathematical theorem—so he did. In point of fact that theorem wasn't needed to solve the problem, but it's fortunate that he thought it was because it was an extremely interesting theorem, and there's a small industry among professional mathematicians these days of pushing Ramsey's theorem and trying to extend it into new areas; so there is a thing that mathematicians will recognize by name, the name being Ramsey's Theorem, and it's something that people are still working at.

But meanwhile the logical problem that Ramsey used his theorem to solve a special case of was shown the year after he died to have no general solution, and there was therefore no longer the point that Ramsey thought there was in trying to solve it.<sup>16</sup> So Ramsey's fame in mathematics, which was his profession, rests on a theorem which he proved quite gratuitously in order to try and do something that we now know can't be done.

Keynes did more for Ramsey than help to get him his Fellowship at King's. He encouraged him to work on Keynes' own subject, economics. Ramsey's interest in economics in fact went back to his schooldays, and Ogden had got him to study the then much-discussed social credit proposals of a certain Major Douglas. I. A. Richards again recalled the upshot.

Soon after he'd done the Douglas credit thing, you know, A. S. Ramsey, his father, called up Ogden and said 'What have you been doing to Frank?', and Ogden said 'What's he been doing?'. 'Oh he's written a paper on Douglas Credit which would have won him a Fellowship in any University anywhere in the world instantly. It's a new branch of mathematics.'

Well no doubt his father was biased. But Keynes knew a good economist when he saw one, and here is what he had to say in his obituary notice of Ramsey:

Economists living in Cambridge have been accustomed from his

<sup>16</sup> See E. Nagel and J. R. Newman, *Gödel's Proof* (London: Routledge & Kegan Paul, 1989).

undergraduate days to try their theories on the keen edge of his critical and logical faculties. If he had followed the easier path of mere inclination, I am not sure that he would not have exchanged the tormenting exercises of the foundations of thought, where the mind tries to catch its own tail, for the delightful paths of our own most agreeable branch of the moral sciences, in which theory and fact, intuitive imagination and practical judgment, are blended in a manner comfortable to the human intellect.

When he did descend from his accustomed stony heights he still lived without effort in a rarer atmosphere than most economists care to breathe, and handled the technical apparatus of our science with the easy grace of someone accustomed to something far more difficult. But he has left behind him in print only two witnesses to his power—his papers published in *The Economic Journal* on ‘A contribution to the theory of taxation’ in March 1927, and on ‘A mathematical theory of saving’ in December 1928.<sup>17</sup> The latter of these is, I think, one of the most remarkable contributions to mathematical economics ever made, both in respect of the intrinsic importance and difficulty of its subject, the power and elegance of the technical methods employed, and the clear purity of the illumination with which the writer’s mind is felt by the reader to play about its subject. The article is terribly difficult reading for an economist, but it is not difficult to appreciate how scientific and aesthetic qualities are combined in it together.<sup>18</sup>

This paper of Ramsey’s set out to say how much of its income a nation should save, and got a general and definite answer, albeit under highly simplifying assumptions. It isn’t an easy paper, as Keynes remarked, but even a non-economist like me can appreciate Ramsey’s use, for the ideal state in which all possible enjoyment can be had now, so that there’s no need to save anything, of the technical term ‘bliss’! It isn’t only in economics that one wishes writers could more often be so apt and witty in their choice of words.

But as in probability and utility theory, so in economics it took time for Ramsey’s ideas to catch on. As Richard Stone explained in his part of the Introduction to an edition of Ramsey’s work published in 1978, only since 1960 have Ramsey’s ideas on saving been much developed, and his ideas on taxation only since 1970.

<sup>17</sup> F. P. Ramsey, *Foundations*, D. H. Mellor, (ed.) (London: Routledge & Kegan Paul, 1978), chs 10, 11.

<sup>18</sup> *The Economic Journal* 40 (March 1930).

Now, however, according to Stone, 'they are generally recognized as the starting points of two flourishing branches of economics: optimal taxation and optimal accumulation.'<sup>19</sup>

Of all the Moral Sciences, however, pure philosophy was, though not Ramsey's profession, what Braithwaite aptly called his 'vocation'. I can't of course summarize all his philosophical work, let alone its remarkable—though again mostly belated—influence and ramifications, on this occasion. So I will give just two examples, to illustrate two things that strike one again and again in Ramsey's writings—even in the sketchiest of his unfinished posthumously published notes—namely, the striking originality and profound simplicity of his thought, and the extent to which he anticipates much later and more laborious literature.

Take first Pilate's notorious question 'What is truth?'. What does it mean to call someone's belief true? That question is as old and unsettled as anything in philosophy. Ramsey calmly suggested, in a one-page aside to his paper on 'Facts and propositions', that it's the wrong question. 'It is evident', he said, 'that 'It is true that Caesar was murdered' means no more than that Caesar was murdered'—and that in essence is all there is to the concept of truth. To assert that something is true is just to reassert the thing itself. The real question, Ramsey argued, is not what it is for my belief that Caesar was murdered to be *true*. That's easy: it's just for Caesar to have been murdered. The real question is what it is to *believe* that Caesar was murdered—as opposed on the one hand to hoping, fearing or having some other attitude to Caesar's murder, and on the other hand to having a belief about something else. If we can answer those questions we shall thereby also, Ramsey claimed, 'have solved the problem of truth'.<sup>20</sup>

Ramsey himself didn't get very far in answering his questions: what distinguishes beliefs generally from other attitudes, and one belief from another? Or, putting the matter in linguistic terms, what distinguishes the meaning of one declarative sentence from the meanings of other such sentences? His general approach was clear enough: under the influence of Russell, and of the American philosopher C. S. Peirce, to whose work Ogden had introduced him, Ramsey took what he called a 'pragmatic' view of the matter, which he admitted to be 'very vague and undeveloped', but characterized by saying:

The essence of pragmatism I take to be this, that the meaning of a sentence is to be defined by reference to the actions to which

<sup>19</sup> Op cit. note 17, 14.

<sup>20</sup> *Philosophical Papers*, 38–9.

asserting it would lead, or, more vaguely still, by its possible causes and effects.<sup>21</sup>

So for example the meaning of the sentence 'The pub is open' is to be explained somehow by the fact that my believing that it's open will, among other things, make me go there if I want a drink: an action which will, if the pub *is* open—that is, if my belief is true—get me the drink I want.

Only in recent years, long after Ramsey's death, has this approach to truth, belief and meaning been seriously taken up;<sup>22</sup> but I think it is now quite clear that it provides by far the best way of understanding the nature, contents and truth of our beliefs, and hence the meanings of the sentences we use to express them.

My second example is Ramsey's paper on 'Theories', in which he seems to me to have been equally prophetic. In my broadcast, Braithwaite explained the background to the paper like this:

Well, the problem arose because during the whole of the nineteenth century physicists had been using more and more abstract concepts. Electric potential, current, fields of force and then, even more recently, photons, electrons and so on. What was the status of these things, what were they? In what did their reality consist? The tradition among the philosophers among whom Ramsey was brought up, particularly Russell, was to say that these things . . . were to be defined in terms of . . . pointer readings, electric shocks, sparks and so on—the phenomena which were naturally visible. This was the view which Ramsey came upon in first thinking about the problem; but he saw that this account of . . . the status of theoretical concepts (as they came to be called) won't do . . . Because, if this is so, theoretical concepts could only mean what they'd already been used for explaining, and there would be no possibility of developing a science by making different, novel uses of theoretical concepts. Whereas the whole of the development of physics . . . had gone entirely by developing notions originally produced for particular purposes, such as electricity and magnetism, developing the notions in an independent manner—and in this way Maxwell produced a unified theory, which then was used to explain light.

Ramsey produced therefore a very interesting view of how to consider these theoretical concepts. . . . it wasn't the case that the sentences about electrons and protons and so on were to be

<sup>21</sup> Ibid., 51.

<sup>22</sup> See e.g. B. Loar, 'Ramsey's Theory of Belief and Truth', *Prospects for Pragmatism*, D. H. Mellor, ed (Cambridge University Press, 1980).

translated directly into propositions about observables. These terms played their part in extremely complex sentences—in a form which were twenty years later called Ramsey sentences—which had both them and observables in as well. So that a treatise of physics would really be one big long sentence—it would be rather like a fairy story starting ‘Once upon a time there was a man who . . .’ or ‘Once upon a time there was a frog which . . .’, the rest of the story going on to describe the adventures of the man or the adventures of the frog. A treatise on electrons, in Ramsey’s view, starts by saying ‘There are things which we will call electrons which . . .’, and then goes on with the story about the electrons . . . only of course you then believe the whole thing, the whole ‘There is . . .’ sentence, whereas in a fairy story of course you don’t.

One immediate consequence which Ramsey drew from this view of his was that no single bit of a scientific theory can be understood apart from that theory; and bits of rival theories can’t be dismissed just because they don’t occur in our theory. Thus, as Ramsey put it,

If a man says ‘Zeus hurls thunderbolts’, that is not nonsense because Zeus does not appear in my theory, and is not definable in terms of my theory. I have to consider it as part of a theory and attend to its consequences, for example that sacrifices will bring the thunderbolts to an end.<sup>23</sup>

By the same token, if we want to work out whether or not some bit of a theory—like ‘Zeus hurls thunderbolts’ or ‘Electrons have such-and-such a mass’—is true, we can’t just assess it on its own. We have, as Ramsey said,

to think what else we might be going to add to our [theory], or hoping to add, and consider whether [it] would be certain to suit any further additions.<sup>24</sup>

Another thing that follows from Ramsey’s view of theories is that rival theories may give quite different meanings even to theoretical concepts which they appear to share—as Newton and Einstein arguably did to the concept of mass—so that there may be no straightforward way of comparing their theories, not even of saying that they are incompatible with each other. So, Ramsey said,

<sup>23</sup> *Philosophical Papers*, 137–8.

<sup>24</sup> *Ibid.*, 132.



The adherents of two such theories could quite well dispute, although neither affirmed anything the other denied.<sup>25</sup>

That all this is so, Ramsey took for granted; and his account of what theories in science are shows immediately and elegantly why it should be so. But the next generation of philosophers of science largely ignored these features of theories; and they were only rediscovered, through case studies in the history of science, in the nineteen-sixties, when the obvious problems they pose in explaining how one should choose between rival theories in science gave rise to a vigorous debate in the methodology of science.<sup>26</sup> But I know of no one who has yet given any better explanation of why these problems arise in the first place than Ramsey gave in 1929.

Here as elsewhere, the time it has taken the rest of us to catch up with and develop the implications of Ramsey's ideas is some measure of what philosophy may have lost by his dying so young. What he would have done had he lived, of course, we can only guess. Ayer saw in his unfinished papers 'hints of the kind of views that [the Harvard philosopher Willard] Quine much later developed' and thought that Ramsey might have become 'a sort of English Quine'. Although Ayer did not elaborate, Ramsey's remark about Zeus certainly fits passages like the following, from Quine's 1951 paper on 'Two dogmas of empiricism':

As an empiricist I continue to think of the conceptual scheme of science as a tool, ultimately, for predicting future experience in the light of past experience. Physical objects are conceptually imported into the situation as convenient intermediaries ... comparable epistemologically to the gods of Homer.<sup>27</sup>

This passage is also reminiscent of the comments Ramsey made on astronomy in a paper he gave in 1925 to the so-called Apostles, a Cambridge discussion society:

Where I seem to differ from my friends is in attaching little importance to physical size. I don't feel in the least humble before the vastness of the heavens. The stars may be large, but

<sup>25</sup> Ibid., 133.

<sup>26</sup> See e.g. T. S. Kuhn, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1962); I. Lakatos and A. Musgrave, *Criticism and the Growth of Knowledge* (Cambridge University Press, 1970).

<sup>27</sup> W. v. O. Quine, 'Two Dogmas of Empiricism', *From a Logical Point of View* (Cambridge, Mass.: Harvard University Press, 1961), 44.

they cannot think or love; and these are qualities which impress me far more than size does. I take no credit for weighing nearly seventeen stone. My picture of the world is drawn in perspective, and not like a model to scale. The foreground is occupied by human beings and the stars are all as small as threepenny bits. I don't really believe in astronomy, except as a complicated description of part of the course of human and possibly animal sensation.<sup>28</sup>

Braithwaite made a different guess about what Ramsey would have gone on to do had he lived:

This paper on theories and this thinking about belief was what had been occupying him the last year of his life; and I should think he would have gone on thinking about this. But in 1931 this extraordinary paper of Gödel's appeared, which caused a revolution in mathematical logic, in establishing conclusively that no formal system which was rich enough to have an arithmetic in it could be shown to be self-consistent.<sup>29</sup> Gödel's paper really made mathematical logic into a professional subject, and a specific and exciting branch of mathematics. I believe this would have excited Ramsey so much that he might have galloped down this for ten years or so, and left philosophy. But I do not know.

Dick Jeffrey agreed, up to a point:

It would be unlike Ramsey simply to go into metamathematics as pure mathematics. I think it would have stimulated some very interesting philosophical thinking from him too. I think though . . . Braithwaite's right, Gödel's result is just the sort of thing that would have fascinated Ramsey, that he would have gobbled up and done some very important, interesting work in it, I imagine: it's too bad he died just before that.' But, added Jeffrey, 'That's just an example. He was also of course an accomplished economist; so you know he was a mathematician, economist, philosopher, logician. God knows where he would have wound up, or what he would have done exactly. He was simply a terribly talented and lively mind.

I should like to let Ramsey have the last word. In the summer of 1929, shortly before he died, he wrote a note on 'Philosophy' which Braithwaite published in his 1931 collection of Ramsey's work. Most of Ramsey's work is within philosophy rather than

<sup>28</sup> *Philosophical Papers*, 249.

<sup>29</sup> Op cit. note 16.

about it; but this note expresses his view of the subject as well as his attitude towards it and his way of doing it. So here, to conclude, are some excerpts.

Philosophy must be of some use and we must take it seriously; it must clear our thoughts and so our actions. Or else it is a disposition we have to check, and an enquiry to see that this is so; i.e. the chief proposition of philosophy is that philosophy is nonsense. And again we must then take seriously that it is nonsense, and not pretend, as Wittgenstein does, that it is important nonsense!

In philosophy we take the propositions we make in science and everyday life, and try to exhibit them in a logical system with primitive terms and definitions, etc. Essentially a philosophy is a system of definitions or, only too often, a system of descriptions of how definitions might be given . . .

I used to worry myself about the nature of philosophy through excessive scholasticism. I could not see how we could understand a word and not be able to recognize whether a proposed definition of it was or was not correct. I did not realize the vagueness of the whole idea of understanding, the reference it involves to a multitude of performances any of which may fail and require to be restored . . .

Philosophy is not concerned with special problems of definition but only with general ones: it does not propose to define particular terms of art or science, but to settle e.g. problems which arise in the definition of any such term or in the relation of any term in the physical world to the terms of experience . . .

[But] it seems to me that in the process of clarifying our thought we come to terms and sentences which we cannot elucidate in the obvious manner by defining their meaning. For instance, . . . theoretical terms we cannot define, but we can explain the way in which they are used, and in this explanation we are forced to look not only at the objects which we are talking about, but at our own mental states . . .

I find this self-consciousness inevitable in philosophy except in a very limited field. We are driven to philosophize because we do not know clearly what we mean; the question is always 'What do I mean by x?' And only very occasionally can we settle this without reflecting on meaning. But it is not only an obstacle, this necessity of dealing with meaning; it is doubtless an essential clue to the truth. If we neglect it I feel we may get into the absurd position of the child in the following dialogue: 'Say breakfast.' 'Can't.' 'What can't you say?' 'Can't say breakfast.'

But the necessity of self-consciousness must not be used as a justification for nonsensical hypotheses; we are doing philosophy not theoretical psychology, and our analyses of our statements, whether about meaning or about anything else, must be such as we can understand.<sup>30</sup>

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<sup>30</sup> *Philosophical Papers*, ch. 1.