



KARL POPPER

*Lotte Meitner-Graf*

## Karl Raimund Popper 1902–1994

KARL POPPER was born in Vienna on 28 July 1902, and died in London on 17 September 1994.

### Vienna

He had two sisters: Dora, born in 1893, and Annie, born in 1898. Their father, Dr Simon Popper, had come to Vienna from Bohemia. In Vienna he took a law degree and afterwards became a partner in a law firm headed by the man who was to be the last liberal Mayor of Vienna. The firm's offices were on the first floor of a fine eighteenth-century building close by the cathedral. When Simon Popper took over as senior partner in 1896, a large adjoining apartment became the family residence. (Its present address is Bauernmkt 1; it was to become a Nazi headquarters.<sup>1</sup>) He was also a social reformer, writer, and scholar, with a fine library of some 10,000 books;<sup>2</sup> books were everywhere except the dining-room, where the grand piano reigned. Karl's mother, née Jenny Schiff, played the piano beautifully. Her whole family was musical, and two of her brothers had positions at the university. The Popper children were born into a cultured and wealthy home, and the family was well-connected. Konrad Lorenz was a childhood friend,<sup>3</sup> and the pianist Rudolf Serkin later became a lifelong friend.

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<sup>1</sup> KRP. (For sources and abbreviations see the Note on p. 684 below.)

<sup>2</sup> Melitta Mew.

<sup>3</sup> Karl Popper, *In Search of a Better World* (London 1992), p. 99.

Precisely on Popper's twelfth birthday Austria declared war on Serbia. At first the war did not have a too disturbing effect on the family, though all his cousins who were old enough became Army officers. His mother still took the family for summer holidays to the Alps. Rosa Graf, a sister of Freud's,<sup>4</sup> was a friend of the Popper family, and in the summer of 1916 she joined them in their mountain retreat. Her son visited them briefly on Army leave before going to the front. Soon afterwards news came that he had been killed, and the grief this caused deeply impressed Popper. Another family experience which he reported long afterwards concerned another family friend, Dr Karl Schmidt, a lawyer and now an Army officer, who was a regular visitor at the Poppers' home. On one occasion he told them over supper that he had been given the task of preparing the case for high treason against a certain Professor of Philosophy at the University of Vienna, now out of the country, called Tomas Masaryk. Masaryk was a traitor, he said, but also a wonderful man.<sup>5</sup>

By the time it ended the effects of the war were devastating. There came hunger-riots and inflation, and occasional shooting. The secure, comfortable world in which Popper had grown up was gone. At sixteen he left home and school, in a mood of private revolt. There followed a mixed-up, experimental period in which, while nominally enrolled at the University of Vienna, he tried being a manual worker, switched to being a cabinet-maker's apprentice, joined a workers' movement, became a Democratic Socialist and, briefly, a Communist. He earned a little money coaching American students. He and his friends walked in the mountains, read ravenously, and enjoyed music. Much of the Youth Movement's ethos, its belief in emancipation through education, its love of the open air and the mountains, its moral seriousness, became lifelong attitudes of his.<sup>6</sup> Its puritanism included a ban on all stimulants; in his childhood the sight of a drunken man falling under the wheels of a horse-drawn carriage had in any case given him a lifelong horror of alcohol.<sup>7</sup>

In 1919 he witnessed the shooting of some unarmed young

<sup>4</sup> His favourite sister, it seems (Ernest Jones, *Sigmund Freud: Life and Work* (London, 1953), vol. i, p. 11).

<sup>5</sup> This incident is touched on in *Unended Quest*, p. 15, and described at more length in a talk which Popper gave in Prague, in 1994, on receiving an honorary doctorate from Charles University.

<sup>6</sup> EHG.

<sup>7</sup> KRP.

Socialists outside a police station during a demonstration engineered by Communists. This turned him less against the police than against Marxism with its belief in revolutionary violence as a precondition for progress towards the dictatorship of the proletariat. As well as his break with Marxism, this year brought his first exposure to Einstein's revolutionary theory of gravitation which had recently passed Eddington's 'star-shift' tests. He attended a lecture by Einstein which left him in a whirl, but a fellow student took him through the theory.<sup>8</sup> He acted as an unpaid assistant to Alfred Adler, and worked with neglected children. Although, or perhaps because, he considered Schubert the last of the great composers, he attempted to get to know something about contemporary music by joining a society presided over by Schönberg. Later, in reaction against this, he entered the Department of Church Music in the Vienna Academy of Music. He was admitted on the basis of a fugue for organ he had written, and he continued to compose afterwards. (In 1992, at Bryan Magee's instigation, Gillian Weir played this fugue for him; sadly, he could no longer hear the higher notes.)

In 1925 he enrolled in the newly founded Pedagogic Institute. He was soon giving unofficial seminars to fellow students. He said that he learnt very little from the teachers there; but he now met two university professors, Karl Bühler and Heinrich Gomperz, who were important for his intellectual development. His investigations began to shift from the psychology to the logic of scientific inquiry. In 1928 he received his Ph.D., passing with the highest grade; his examiners were Bühler and Moritz Schlick. He now became a qualified schoolteacher. At this Institute he also met Josefine Henninger, or 'Hennie'; they married in 1930. It was the beginning of a remarkable partnership, each devoted to the other, which lasted until Hennie's death in 1985.

He was reading Kant intensively, and also works by contemporaries such as Rudolf Carnap, Kurt Gödel, Hans Hahn, Karl Menger, Hans Reichenbach, Richard von Mises, Friedrich Waismann, and Ludwig Wittgenstein. Gomperz introduced him to Victor Kraft, who was the first member of the Circle whom Popper had met and the author of a book on scientific method which he found valuable. He attended Carnap's seminar in about 1929, and an uncle introduced him to Herbert Feigl, who encouraged him to publish his ideas in a book. This launched

<sup>8</sup> 'I was fortunate in being introduced to these ideas by a brilliant young student of mathematics, Max Elstein, a friend who died in 1922 at the age of twenty-one' (*Unended Quest*, pp. 37–8).

him into the writing of *Die beiden Grundprobleme der Erkenntnistheorie* (published, in German, only in 1979). His typescript was much read and discussed by members of the Vienna Circle, including Neurath, Schlick, and Waismann, and he had intensive discussions with Carnap and Feigl in the Tyrol in the summer of 1932. Carnap presented some of Popper's unpublished ideas, with due acknowledgements, the following year, and Popper put out trailers for his book: a short note, 'A Criterion of the Empirical Character of Theoretical Systems', and another on the question of induction and the probability of hypotheses, published in *Erkenntnis* in 1933 and 1935.

The book's two basic problems were that of the demarcation of science from non-science and that of induction. He rather gave the impression that the first of these is a familiar problem which had been well recognised at least since Kant (in 1933 he referred to it as 'Kant's problem of the limits of scientific knowledge'), his contribution being to offer a new solution. Kant's demarcation problem, however, was different from his own. Of course inductivists, such as Bacon, Whewell and Mill, by laying down methods for science did thereby offer solutions to the demarcation problem; but that does not mean that they explicitly posed this problem. Once stated, it seems so obvious that one tends to assume that it must have been stated long ago, but I don't know of anyone who did explicitly state it before Popper. Anyway, the received view, as Popper read it, was that the empirical sciences are distinguished by their use of an inductive method; which brings us to his second basic problem. This goes back to Hume; he had pointed out that inductive inferences from observed to unobserved instances are logically invalid; but he also held that the belief-forming machinery with which humans, like other animals, are endowed by Nature works in an essentially inductive way. A logical puritanism that inhibited us from making such invalid inferences would be deadly. Fortunately, our robustly non-logical human nature excludes this.

Popper sought to solve his two basic problems at one blow with his falsificationist philosophy of science. What demarcates science from non-science (metaphysics, logic, mathematics, and pseudo-science) is not the verifiability but the falsifiability of its theories. The method of science also is not inductive; it does not start out from observations and generalise from them: it starts out from problems, which it attacks with bold conjectures. The latter are unverifiable and unjustifiable but, when well developed, have predictive implications which can be put to the test, the more severe the better. A test will be severe if made with

sufficiently discriminating experimental apparatus on predictions which deviate (as Einstein's did from Newton's) to a small but detectable extent from unrefuted predictions of the previous theory, or if made on predictions which break new ground. On this view, scientific inferences are all deductive, either from conjectural premises to a falsifiable consequence or from a falsified consequence to the negation of the conjunction of the jointly responsible premises. The problem of induction therefore drops out. (Whether it drops out completely is a question which will come up again.)

A shorter version of this book was published as *Logik der Forschung* in 1934. This went on to tackle several large problems opened up by its falsificationist view of science. One concerned the nature and status of the statements in the empirical basis against which scientific hypotheses are tested. Another concerned simplicity, which fell nicely into place: simpler hypotheses are desirable, not because they are more likely to be true, but because they are easier to eliminate if false. The longest chapter, on probability, took off from a seemingly insuperable objection to the book's main view, namely that probabilistic hypotheses, which had come to play a vitally important role in science, are strictly unfalsifiable. It was followed by a chapter on quantum theory.

The book was brought to Einstein's attention through musical connections. Popper's friend Rudolf Serkin played with the Busch Chamber Orchestra and had recently married Adolf and Frieda Busch's daughter. Frieda knew Einstein, now in Princeton, through his violin-playing. In April 1935 she sent him a copy of *Logik der Forschung*, explaining that the author was a Jew living in Vienna and hence had no prospects: 'Have the great kindness to read the attached book. Your judgement, in case it is favorable to Popper, could perhaps help him to get somewhere!'<sup>9</sup> Not long afterwards the young secondary school teacher received a letter from Einstein which began: 'Your book pleased me very much in many respects.' He liked its rejection of the 'inductive method' in favour of falsifiability as the decisive feature of scientific theories; purged of certain mistakes the book 'will be really splendid'. He offered to help in getting it known. Popper replied

<sup>9</sup> I am indebted here to material made available to me by John Stachel from his work on the Einstein papers. Popper (*Knowledge and the Body-Mind Problem* (London, 1994), p. 116) later used the following incident in Adolf Busch's life to illustrate what he called the 'centipede effect': Busch was asked by another violinist how he played a certain passage. 'He said it was quite simple—and then found he could no longer play the passage'. Popper said it was a passage in Beethoven, but it seems that it was actually in Mendelssohn (EHG).

deferentially, but at some length, standing his ground where he thought he had been right. This elicited the reply from Einstein that appears in Appendix \*xii of *The Logic of Scientific Discovery*.

As Frieda Busch's letter indicates, Popper had no hope of a university position in Austria at that time. Before the war, plenty of teachers at the University of Vienna had been Jews or of Jewish origin: now there was growing hostility towards them. Not that Popper regarded himself as Jewish. Both his parents were of Jewish descent, but had converted to Lutheranism, wanting to be assimilated. But that did not stop him from being seen as Jewish. Not only was becoming a university teacher out of the question, but remaining a schoolteacher was becoming difficult. He had been transferred to a school where most of the teachers were crypto-Nazis, and there, as he put it later, he was 'treated badly', while Hennie, who was teaching at another school and was not of Jewish origin, suffered for being married to a Jew.<sup>10</sup>

So anti-Semitism gave Popper one strong motive to emigrate, related to which was the deteriorating political situation. The city of Vienna had been under Socialist control since 1920, but there was a smouldering threat of civil war. In July 1927 he had been present when a large crowd of Social Democrats, protesting at an earlier shooting by rightists, were fired on by the police; nearly 100 people were killed. In 1934, not long after Hitler came to power in Germany, Social Democrats decided, under increasing provocation, to arm themselves. Popper had been against this: they would provoke violence from the right without knowing how to handle their new weapons properly.<sup>11</sup> (In Chapter 19 of *The Open Society* there is a discussion of Marxian ambiguity over violence, epitomised by Engels's *Take the first shot, gentlemen of the bourgeoisie!*) In February 1934 civil war broke out; after four days' fighting Dollfuss had won, and the Social Democrat party was declared illegal. (Dollfuss was murdered by Nazis a few months later.) Popper had been predicting a Nazi take-over for some time. (His prescience was attested by a friend from his youth, Frederick Dorian, who wrote to him from America in 1942 that they often recalled his remarkable predictions of the catastrophe.<sup>12</sup>) It was becoming urgent to find an opening abroad.

<sup>10</sup> Draft autobiography, Popper Collection (135, 10). This passage was subsequently deleted. I learnt of it through a reference (note 18) in Malachi Hacohen, 'Karl Popper in Exile', *Philosophy of the Social Sciences*, 26 (December 1996), 452–92.

<sup>11</sup> KRP.

<sup>12</sup> See above, n. 10, Hacohen, note 14.

*Logik der Forschung*, as well as being reviewed and discussed by Carnap, Hempel, Neurath (who called Popper the Vienna Circle's 'official opposition'), Reichenbach (whose review was harsh), and others, had been quite widely noticed in the English-speaking world. (A British publisher, Hodges, bought the rights in 1937 and the work of translating it into English was begun, but the results were judged unsatisfactory, and then the outbreak of war in 1939 put an end to it.<sup>13</sup>) At the International Congress of Scientific Philosophy in Paris in 1935, Susan Stebbing invited him to lecture at Bedford College, London. (She had invited Carnap the year before.) He received other invitations and spent altogether about nine months in England during 1935–6.

Popper decided to lecture not on his own ideas but on Alfred Tarski's semantic theory of truth. Tarski had been giving him tutorials on this in Vienna, and Popper was persuaded of its enormous importance. That truth is objective or 'absolute' was one of Popper's most enduring convictions, but attempts hitherto, by Wittgenstein and Schlick for instance, to elucidate the idea of a statement corresponding to a fact had failed. And now he learnt that Tarski had rehabilitated the correspondence theory of truth. One eye-opener for Popper was Tarski's introduction of a metalanguage in which one can talk in the same breath about linguistic entities (words, sentences) in the object-language and about things outside the object-language, thereby enabling one to elucidate such semantic notions as a formula being *satisfied* by a certain state of affairs.

Popper also lectured at Imperial College on probability, and gave a talk to the Moral Science Club in Cambridge. (Braithwaite was involved in this; Popper hoped that Keynes would be there, but Braithwaite had to disappoint him.<sup>14</sup>) Altogether he met a lot of people. Ayer, whom he had known in Vienna, took him to the Aristotelian Society, where he had an encounter with Russell, and also introduced him to Berlin, Hampshire, Moore, and Ryle. He also met mathematicians and scientists, including Schroedinger. Two people whom he met in London were to be of crucial importance to him later: Ernst Gombrich, then a Research Fellow at the Warburg Institute, and F. A. ('Fritz') Hayek, a Professor of Economics at the London School of Economics. Both men were from Vienna, but Popper had not met

<sup>13</sup> Hacothen; and KRP to EHG, 21 May 1943.

<sup>14</sup> RB to KRP, 12 March 1936.

Hayek before and Gombrich only fleetingly (although his father had worked in Popper's father's law firm). Now Popper and Gombrich, both living in 'horrible bedsitters' in Paddington, were meeting frequently.<sup>15</sup> Popper had brought along an early version of 'The Poverty of Historicism' which he presented at Hayek's seminar at LSE; he also gave Hayek a copy of *Logik der Forschung*. He was introduced to Walter Adams, the then General Secretary of the Academic Assistance Council (or Society for the Protection of Science and Learning as it became).

Popper liked England. One thing that impressed him was milk-bottles being left on London doorsteps; in Vienna they would have been stolen.<sup>16</sup> But the desperately needed job openings did not materialise. Then sometime in 1936 J. H. ('Socrates') Woodger drew his attention to an advertisement for a professorship and a lectureship in what was then the Department of Philosophy and Education at Canterbury University College, Christchurch, New Zealand. He applied for both posts. He gave Moore and Woodger as his referees, and submitted testimonials from Bühler, Carnap, Russell, and Tarski. In the meanwhile he went to the International Congress in Copenhagen in 1936 (where news came that Schlick had been killed).

After the Congress he stayed on for discussions with Niels Bohr at the latter's Institute. When he returned to Vienna he was faced with a choice. Felix Kaufmann, who admired him, had interceded on his behalf, and he was now offered academic hospitality in Cambridge, for one year at £150. Popper wrote to Kaufmann that he would never forget what he had done for him.<sup>17</sup> Shortly afterwards he was offered the lectureship at Christchurch. (The professorship went to an anthropologist called Ivan Sutherland, about whom more below.) He would have liked to go to Cambridge, but the New Zealand position was a permanent one, and with a starting salary of £400; moreover the Cambridge offer could be transferred to someone else. So he accepted the Christchurch offer and suggested that 'Fritz' Waismann be invited to Cambridge in his stead (which he was).

<sup>15</sup> Ernst Gombrich, 'The Open Society and Its Enemies: Remembering Its Publication Fifty Years Ago', Lecture at the London School of Economics, 12 June 1995 (LSE Centre for the Philosophy of the Natural and Social Sciences), p. 2.

<sup>16</sup> KRP.

<sup>17</sup> See above, n. 10, Hacohen, notes 25–7.

## New Zealand

He arrived in Christchurch in March 1937, at the start of the academic year. Out of his first salary cheque he sent a subscription to the Society for the Protection of Science and Learning.<sup>18</sup> After the German annexation of Austria in March 1938 he set up a refugee organisation in New Zealand. (Among the people this helped was a Viennese photographer called Bata, who later took the photograph which appeared on the dust jacket of the Schilpp volume on Popper.<sup>19</sup>)

New Zealand seemed light-years from continental Europe. His teaching duties were heavy; as the only philosopher, he had to do all the philosophy teaching in the department—logic, history of philosophy, ethics and politics, problems of philosophy, plus introductory courses. He also did courses for scientists under the auspices of the Royal Society of New Zealand, and WEA classes. If he was not entirely cut off when war came, this was largely due to the good offices of Carnap, Hempel, Oppenheim, and others in America who, among other things, subscribed on his behalf to the *Journal of Symbolic Logic*.<sup>20</sup> In his first year there he gave at a seminar what became a stunning little piece called ‘What Is Dialectic?’ (he was not yet banning *what-is* questions<sup>21</sup>). Against dialecticians who say that contradictions are welcome because they are fertile, it declared that they are fertile only so long as we strive to eliminate them.

He set about turning the talk he had given at Hayek’s seminar into an article—and then something unplanned and rather extraordinary happened. A short section on essentialism which briefly mentioned Plato started growing; and it went on growing and growing until it became volume one of *The Open Society*. There are interesting differences between this volume and volume two, the critique of Marxism. He had been thinking critically about Marxism for many years; when he started to write about it with an intention to publish, in 1935, he was still writing in German. The Plato volume was written in English from the outset, and is altogether more lively and arresting. His shift to English as the language in which he lectured, wrote, and thought was a traumatic experience,<sup>22</sup> but it had a bracing effect. (A radio critic

<sup>18</sup> Brian McGuinness.

<sup>19</sup> Alan Musgrave.

<sup>20</sup> KRP to RC, 5 July 1943.

<sup>21</sup> Published in *Mind*, October 1940; reprinted as ch. 15 of *Conjectures and Refutations* (London, 1963).

<sup>22</sup> KRP.

remarked on the clarity of a talk on Newton which he gave on New Zealand radio: perhaps the speaker's unfamiliarity with the English language had restricted him to simple words?<sup>23</sup> The implied prediction that as Popper's mastery of the English vocabulary improved he would become increasingly obscure was not borne out.) He took to the new language astonishingly well. Margaret Dalziel was a great help,<sup>24</sup> and Fowler's *Modern English Usage* became a bible for him.

The completion of Popper's great book was a heroic achievement. It was written under most adverse conditions.<sup>25</sup> As well as language problems and his heavy teaching load, 'hopeless' library conditions,<sup>26</sup> and the near-impossibility of acquiring new books, he had himself to procure the paper on which that big book was written and rewritten again and again in his large, round handwriting, and typed and retyped by Hennie. His not being allowed departmental paper for research purposes brings us to what was perhaps the main adverse factor, the bitter hostility which developed between him and Sutherland, his Head of Department. (Sutherland was to commit suicide in 1952.<sup>27</sup>) It seems that things turned really nasty once New Zealand was at war with Germany. In 1940 Popper made an official complaint to the Rector, Dr Hight, to the effect that Sutherland had been spreading the allegation that he, Popper, was disloyal to the British cause (there was talk of his attending a party given by a refugee to celebrate the fall of Holland). Hight, who was highly supportive of Popper,<sup>28</sup> replied that he was convinced of Popper's absolute loyalty to the British cause.<sup>29</sup> According to Popper, Sutherland swore that he would drive him out of New Zealand. He also said that Sutherland denounced him to the police, who interrogated him.<sup>30</sup> However, it is not clear that Sutherland took the initiative here. Technically, Popper was now an 'enemy alien', and the police would have interviewed him as a matter of course.<sup>31</sup> Perhaps they routinely contacted his Head of Department, who welcomed the

<sup>23</sup> KRP.

<sup>24</sup> JAP to EHG, 29 July 1943.

<sup>25</sup> See Colin Simkin, *Popper's Views on Natural and Social Science* (E. J. Brill, 1993), Appendix One.

<sup>26</sup> KRP to FAH, 9 December 1943.

<sup>27</sup> Roger Sandall kindly sent me obituaries of Sutherland which appeared in the *Journal of the Polynesian Society*, 61 (1952), 120–9.

<sup>28</sup> Colin Simkin.

<sup>29</sup> Popper Collection (366, 5).

<sup>30</sup> Colin Simkin.

<sup>31</sup> Peter Munz.

chance to cast doubt on his loyalty. The police do not seem to have pursued any allegations he made. Matters did not improve with time. In 1944 Popper asked for an official investigation into his situation in the department, rendered intolerable by Sutherland. He also complained that the method of distributing the money for examiners had been revised in a way that deprived him of most of his share, although he was still doing all the examining in philosophy. There was now a new Rector, and it seems that nothing was done.

In March 1945 Popper wrote to Gombrich: 'This is the ninth year that I am doing *all* the teaching in philosophy, and I am the only member of the staff who has not got a salary rise in this time—or rather, there is no member who has not got at least four rises in this period'. He added: 'those of my friends who protested on my behalf against this treatment were told something like this: "We know that he is too good for this place. This we cannot help; and we shall not try to hold him if he wishes to go elsewhere"'.<sup>32</sup> His salary did indeed remain fixed, at NZ £500, during his time in New Zealand. The advertisement for the lectureship, issued by the Universities Bureau in London in 1936, which gave the salary as £400 rising by two increments of £50 to £500, was ambiguous as between English and New Zealand pounds. He inquired about this at the time, but was obliged to sail before receiving an answer. It turned out that New Zealand pounds were meant; however, it was decided to set his salary at the equivalent of £400 sterling, namely NZ £500. So he started at the top of the Lecturer's scale. During 1940 it was decided to introduce a Senior Lecturer grade, with a minimum salary of £500 and a maximum of £650.<sup>33</sup> He was promoted as from January 1941, but put at the bottom of the scale, so his salary remained the same. Nor did he get any increment subsequently. The Poppers came to feel very poor. As well as a heavy mortgage, he had taken out an insurance policy to protect Hennie in the event of his death.<sup>34</sup> And there was the cost of the paper for the big book he was writing, to which would soon be added the cost of numerous cables about it, first to America and later to England.

With the Japanese drawing closer, Popper determined to finish it (its title was not yet decided), and worked at it day and night for days on end;<sup>35</sup> and he did 'finish' it (although no writing of his was ever safe

<sup>32</sup> KRP to EHG, 28 March 1945.

<sup>33</sup> W. J. Gardner, E. T. Beardsley, and T. E. Carter, *A History of the University of Canterbury, 1873–1973* (Christchurch, 1973), p. 292.

<sup>34</sup> Simkin, *Popper's Views*, p. 187n.

<sup>35</sup> JAP to EHG, 29 July 1943.

from subsequent revision) in October 1942. Now came the problem of publication. If he submitted it himself to a publisher in England or America and it was rejected, a year might go by before he got the typescript back (assuming it was not lost *en route* through enemy action). He needed someone in England or America to act for him. The paper shortage being more severe in Britain, he decided to try in America.

Some months later Hennie wrote: 'We soon had to realize that our friends to whom we had sent the manuscript were letting us down completely . . . . And now began an undescribably miserable time . . . . We felt completely abandoned and cast out.'<sup>36</sup> And in *Unended Quest* Popper wrote:

. . . the reaction of those friends in the United States to whom I sent the manuscript was a terrible blow. They did not react at all for many months; and later, instead of submitting the manuscript to a publisher, they solicited an opinion from a famous authority, who decided that the book, because of its irreverence towards Aristotle (not Plato), was not fit to be submitted to a publisher (p. 119).

Those friends have until recently remained shrouded in anonymity and ignominy. Their cover was first blown, so far as I know, by Hacoheh.<sup>37</sup> Popper had tried to get in touch with an old friend from Vienna, now in New York, called Alfred Braunthal; and things might have gone better if he had succeeded, but he had the wrong address and got no reply. Then, all wound up and impatient, he did something rather daft. The one person in America of whose address he was reasonably sure was a Professor Dorian (mentioned earlier, who had recently congratulated him for his prescience back in the 1920s about the impending catastrophe in Europe). Popper had doubts as to whether Dorian would be interested; but instead of taking the obvious precaution of first writing to ask whether he would accept this big undertaking, he sent him the typescript, together with 'very full instructions', without prior warning. Popper's fears turned out to have been justified; Dorian was not interested and did not read it,<sup>38</sup> but he did bring in another mutual friend from their Vienna days, Fritz Hellin. Popper also doubted whether this man would be sufficiently interested.<sup>39</sup> Then Popper at last got hold of

<sup>36</sup> JAP to EHG, 29 July 1943.

<sup>37</sup> See above, n. 10, Hacoheh, notes 56, 57.

<sup>38</sup> KRP to AB, 21 May 1943. This and other correspondence is in Popper Collection (28, 1–8).

<sup>39</sup> KRP to AB, 21 May 1943.

Braunthal's correct address, and he too was enrolled. He did read it, and at last there came a ray of sunshine. He reported: 'So deeply was I moved by your writing, so excited was I and even thrilled that I devoured the entire book as it were in one gulp.'<sup>40</sup> He now collaborated with Hellin, who in August sent Popper a cable saying that the manuscript was with Friedrich, and adding, 'Be patient'. Carl J. Friedrich was Professor of Government at Harvard and author of *Constitutional Government and Democracy*. Although Popper wrote afterwards of their soliciting an opinion from an authority instead of submitting the manuscript to a publisher, Hellin and Braunthal would rightly have regarded him as someone who, if his response were positive, might persuade Harvard University Press to take it. They could not know that it would be negative. At about this time 'The Poverty of Historicism' was rejected by Moore for *Mind*. Popper and Hennie, already in a state of exhaustion, were now in despair.

In April 1943 Popper switched the search for a publisher to England. He wrote to Gombrich that he had finished a book provisionally entitled 'A Social Philosophy for Everyman', and asked him to take over the task of finding a publisher. Gombrich cabled his willingness, and Popper sent him copies of the typescript.

He also wrote to Hayek, one of the rather small number of teachers at LSE (which was now evacuated to Cambridge) who had not been absorbed into the war effort. He had met Popper only about four times, in 1935–6, but he had been greatly impressed both by Popper's talk at his seminar and by *Logik der Forschung*, and the two men had had some correspondence since 1940. He now formed the intention of getting Popper to LSE. Abraham Wolf, who had held a part-time Chair in Logic and Scientific Method at LSE (and a full-time one at UCL), had retired in 1941. Hayek's idea was that the part-time chair might be converted into a full-time readership, to be occupied by Popper, but he would have little hope unless he could persuade his colleagues on the Appointments Committee, especially Morris Ginsberg, Professor of Sociology, that Popper was taking an active interest in problems relating to the social sciences,<sup>41</sup> so he was delighted to learn of Popper's switch to social philosophy. On July 17 he received from Gombrich one of the typescripts of Popper's big book. He immediately began reading it and was 'profoundly impressed'.<sup>42</sup> In those days the chief members of the

<sup>40</sup> AB to KRP, 24 August 1943.

<sup>41</sup> FAH to EHG, 12 July 1943.

<sup>42</sup> FAH to EHG, 18 July 1943.

Appointments Committee at LSE were, beside Hayek, Carr-Saunders as Director, Ginsberg, Laski, and Tawney. Laski, who was reading the typescript as a publisher's reader for Nelson, was enthusiastic about it.<sup>43</sup> Carr-Saunders liked it.<sup>44</sup> (I will touch on Ginsberg's reactions later.) Hayek also showed it to Lionel Robbins, then seconded to the Economic Section of the War Cabinet, and he too was enthusiastic. In November 1943 Hayek was authorised by the Appointments Committee to write to Popper telling him that a University Readership in Logic and Scientific Method, tenable at LSE, would be advertised, and asking him if he would be a candidate. Popper replied that he would. He wrote to Gombrich: 'We are of course terribly excited, and shaken up in consequence of Hayek's airgraph'.<sup>45</sup>

Gombrich and Hayek tried to find a publisher, but without success at first. Cambridge University Press turned it down, and there were several more rebuffs. As well as Plato-reverence, there was, in those days of paper rationing, dismay at the book's size; and some admirers of the book, such as Robbins, felt that the critique of Marx was too long and heavy.<sup>46</sup> Hayek eventually turned to Routledge, who were publishing his own *The Road to Serfdom*. There Herbert Read read it and was enormously impressed. It was at last accepted.

Now came the business of getting it ready for printing. Gombrich has given a delightful description of his role in this onerous task.<sup>47</sup> He received as many as 95 aerogrammes (miniaturised airletters), containing instructions, often intricate, for amendments, especially to the Notes. Then there was the question of the title. As well as 'A Social Philosophy for Everyman' Popper toyed with 'Three False Prophets: Plato-Hegel-Marx' and 'A Critique of Political Philosophy'. This is perhaps the place to mention John Findlay, then Professor of Philosophy at Otago, since he had something to do with the eventual choice. He and Popper often visited each other during vacations; he found their discussions immensely profitable, but also immensely exhausting as Popper never knew when to stop. (It seems that Findlay sometimes broke off to take a nap. A female student once opened the door to Popper's room and then ran off terrified on seeing what appeared to be a corpse laid out on the floor.<sup>48</sup>) He found the Popper's house on the

<sup>43</sup> FAH to EHG, 29 October 1943.

<sup>44</sup> FAH to KRP, 27 December 1943.

<sup>45</sup> KRP to EHG, 13 December 1943.

<sup>46</sup> FAH to KRP, 29 January 1944.

<sup>47</sup> See the lecture cited in n. 15, above.

<sup>48</sup> Colin Simkin.

Cashmere Hills amazingly beautiful, with its views towards the Southern Alps. He said that he there persuaded Popper to drop the 'False Prophets' title.<sup>49</sup> Findlay's widow claimed that he won Popper over to 'The Open Society'.<sup>50</sup> Not that those three words settled the problem. The contract with Routledge had, 'The Open Society and its Antagonists'. Popper cabled, 'Consider Enemies Better'; whereupon the post-mistress informed the police!<sup>51</sup> He was able to persuade them that he did not consider Hitler & co. better than Churchill (whom he admired enormously) & co.

Back now to the LSE readership. The post had been advertised and a Board of Advisors set up. The decision would be taken in March–April 1945. There were nine applicants.<sup>52</sup> Apart from Popper the only one taken seriously was Casimir Lewy; it was agreed that the readership would either be filled by Popper or else not filled; in the latter case Lewy might be appointed a lecturer at LSE. It was by no means certain how things would go. Proofs of *The Open Society* were available to the Board. Ginsberg, whose support Hayek judged essential, was wavering. He had liked Chapter 14, on 'The Autonomy of Sociology'; but Hayek had now sent him 'The Poverty of Historicism' which he (Hayek) was in process of publishing in *Economica*, and it seems that Ginsberg became uneasy about the latter's implications for sociology;<sup>53</sup> he was also worried by what he saw as evidence of intellectual arrogance. The internal 'expert' (H. F. Hallett, the Spinoza scholar at King's College London) was opposed. In addition, Hayek, as Popper was dismayed to learn, would not be present when the decision was taken. (*The Road to Serfdom* had now been published in America by Chicago University Press, and they had arranged a lecture tour for him. He had timed it so that he would sail after the Board met, but the meeting was postponed and Hayek could not forgo his berth in a wartime convoy.<sup>54</sup>) Much would depend on the two external 'experts'. One was Sir David Ross, a

<sup>49</sup> J. N. Findlay, 'My Life: 1903–1973', in *Studies in the Philosophy of J. N. Findlay*, ed. Robert S. Cohen, Richard M. Martin, Merold Westphal (SUNY Press, 1985), p. 26. Findlay incorrectly wrote that Popper had had this house built. It was a single-storey wooden house.

<sup>50</sup> She had written this on the back of a copy of a photograph of Popper and Findlay in Dunedin in 1944, which I received thanks to R. S. Cohen.

<sup>51</sup> KRP.

<sup>52</sup> This paragraph is mainly based on LSE archives.

<sup>53</sup> FAH.

<sup>54</sup> FAH to KRP, 19 Mar 1945. For a hilarious account of what happened when he got to New York see *Hayek on Hayek: An Autobiographical Dialogue*, ed. Stephen Kresge and Leif Wenar (London, 1994), pp. 104–5.

distinguished Oxford moral philosopher and Aristotelian scholar, now in his late sixties; would his reaction to *The Open Society* be like Friedrich's?

In the event he came down strongly in favour of appointing Popper, as did the other external, L. J. Russell. When Hayek returned to LSE he was asked to tell Popper the decision. Popper and Hennie had gone to the mountains for a holiday, on doctor's orders. They went to a hotel at the foot of Mount Cook called The Hermitage, and they returned by bus. (Popper had briefly owned a car, but had long since given it up.) At its first stop the local postmistress came out with a cable for 'Karl Popper, c/o Bus from Hermitage';<sup>55</sup> it was from Hayek. This cable may have caused even more pleasure than the letter from Einstein. They sailed from Auckland at the end of November.

### Early Days at LSE

*The Open Society* was published in mid-November, and was already being talked about when they arrived in England in January 1946. Popper was a bright new star on the English philosophical scene, and he was much sought after. By early February he had been slotted into a symposium at the annual Joint Session, to be held in July. The other symposiasts were Ryle, who wrote a glowing review of *The Open Society*,<sup>56</sup> and Lewy. (Popper wanted Lewy to join him at LSE, but this fell through.<sup>57</sup> J. O. Wisdom joined him in 1948.) After this came an invitation to talk to the Cambridge Moral Science Club, in October.

Bertrand Russell, recently reinstated in Trinity College, invited Popper to tea before the meeting, and I will first say a few words about their relationship. Among living thinkers Russell was, along with Tarski and Einstein, one of Popper's supreme heroes. Was this esteem reciprocated? When, quite a few years after this, Popper sent him a complimentary copy of *The Logic of Scientific Discovery* Russell wrote that he was very glad to get this translation of a book which he had read long ago, when it first appeared in German.<sup>58</sup> But the rather perfunctory testimonial he wrote for Popper in 1936 did not mention *Logik der Forschung*, and the pages of his complimentary copy of it remained

<sup>55</sup> KRP to FAH, 3 June 1945.

<sup>56</sup> *Mind*, 56 (April 1947), 167–72.

<sup>57</sup> CL to KRP, 15 July 1946; 4 October 1946.

<sup>58</sup> BR to KRP, 22 January 1959.

virtually uncut.<sup>59</sup> It seems that the complimentary copy of *The Open Society* which Popper had recently sent him might likewise have remained unread; for when Popper asked him to recommend it to the American publishers of *A History of Western Philosophy*, then in the American best-seller list, Russell asked to be lent a copy, explaining that he wanted first to ‘reread’ it, and having no house at present, his books were inaccessible.<sup>60</sup> When another copy came he did read it—and he was bowled over. He recommended it strongly to his publisher, and wrote Popper a testimonial calling it a work of first-class importance. In the annual National Book League lecture, on ‘Philosophy and Politics’, which Russell gave in October 1946, he said that the case against Plato has been ‘brilliantly advocated’ by Popper.<sup>61</sup>

There are varying accounts of this famous meeting of the Moral Science Club, and many of them contain identifiable mistakes. Even the minutes (which say that the meeting ‘was charged to an unusual degree with a spirit of controversy’) get the date wrong, giving 26 instead of 25 October 1946. Popper’s own account, in *Unended Quest*, gets his title wrong, putting ‘Are there Philosophical Problems?’ which might have been better than the actual title as it appears in the printed programme and in the minutes, namely ‘Methods in Philosophy’. (Three weeks later Wittgenstein gave a talk on ‘what the method of philosophy is’ which, according to the minutes, was a reply to Popper’s talk.<sup>62</sup>) Although Popper’s account mentions Russell’s presence, it does not mention what Russell called out to Wittgenstein, on which most other accounts agree, though Munz’s account has Russell calling out words which were surely spoken by Popper.<sup>63</sup> An American philosopher, Hiram McLendon, who was studying under Russell at the time, subsequently wrote an account of this meeting as part of a planned biography of Russell (it does not seem to have been published).<sup>64</sup> It is interesting, and includes comments made by Russell the next day; but it has

<sup>59</sup> I. Grattan-Guinness, ‘Russell and Karl Popper: Their Personal Contacts’, *russell*: the Journal of the Bertrand Russell Archives, (summer 1992), p. 8, n. 8.

<sup>60</sup> BR to KRP, 22 July 1946.

<sup>61</sup> *Philosophy and Politics* (Cambridge, 1947), p. 11.

<sup>62</sup> See Brian McGuinness and G. H. von Wright (eds.), *Ludwig Wittgenstein: Cambridge Letters* (Oxford, 1995), p. 324.

<sup>63</sup> Peter Munz, *Our Knowledge of the Growth of Knowledge: Popper or Wittgenstein?* (London, 1985), pp. 1–2.

<sup>64</sup> It is in Popper Collection (36, 12).

Wittgenstein eventually subsiding where all other accounts have him storming out.

I will reconstruct what happened from the various sources as best I can. The meeting was in Braithwaite's room in King's College. Wittgenstein, who chaired the meeting, sat on one side of an open fire and Popper on the other. Russell was in a high-backed rocking-chair. Others present included Elizabeth Anscombe, Richard Braithwaite, C. D. Broad, A. C. Ewing, Peter Geach, Norman Malcolm, Margaret Masterman, Stephen Toulmin, and John Wisdom (A. J. T. D., not J. O.). There were also various students. The secretary's invitation to Popper had said that 'short papers, or a few opening remarks stating some philosophical puzzle, tend as a rule to produce better discussions than long and elaborate papers'. The minutes say that Popper began by expressing astonishment at the Secretary's letter of invitation (a footnote explains that this is the Club's form of invitation). Wittgenstein seems to have mistaken Popper's opening remarks for a complaint against the Secretary, and sprang to his defence. But Popper was taking the wording of the invitation as expressing the Wittgensteinian thesis that there are no genuine philosophical problems, only puzzles; and he set out to counter this thesis by bringing forward some real problems. One concerned induction. Wittgenstein dismissed this as a merely logical problem. Another concerned the question of actual (as distinct from merely potential) infinities. (One of the two theses in Kant's first antinomy says that the world must have had a beginning in time, otherwise an actual or completed infinity of time will have elapsed. Popper rebutted this many years later.<sup>65</sup>) Wittgenstein dismissed this as a mathematical problem. As his last example, Popper gave the question of the validity of moral rules. Wittgenstein, who had hold of the poker and was waving it about a good deal, demanded an example of a moral rule, to which Popper replied: 'Not to threaten visiting lecturers with pokers'. There was laughter, and Wittgenstein stormed out, angrily declaring as he went that Popper was confusing the issues; whereupon Russell called out, 'Wittgenstein, you're the one who's causing the confusion'.

The next day Russell told McLendon that he had never seen a guest so rudely treated, adding that Popper had more learning and erudition than all of them; and he afterwards wrote to Popper: 'I was much shocked by the failure of good manners on the side of Cambridge. . .

<sup>65</sup> 'On the Possibility of an Infinite Past: a Reply to Whitrow', *The British Journal for the Philosophy of Science*, 29 (March 1978), pp. 47–8.

I was entirely on your side throughout, but I did not take a larger part in the debate because you were so fully competent to fight your own battle'.<sup>66</sup> In January 1947 Popper gave Russell's *A History of Western Philosophy* an encomium on Austrian radio: a great book and what makes it great is the man who has written it.<sup>67</sup>

*The Open Society* made a strong impression on some English politicians on the moderate left, such as Anthony Crosland, and on the moderate right, such as Edward Boyle. By the later 1950s, however, the book's ideas had rather lost their urgency in the West just because they had so largely won out. Hardly anyone believed any more in historical inevitability, let alone the inevitability of Communism, or in Utopian planning. But the book had still a tremendous potential appeal for intellectuals under Communist regimes. There would later be various samizdat translations of *The Open Society* and of *The Poverty of Historicism*.

Let me reproduce here a personal recollection from those days. A fellow student at LSE had told me it was worth going to Dr Popper's lectures 'to hear the great man thinking aloud'. I went, and I was riveted. He had no notes or other paraphernalia. Ideas seemed to flow from him. They were put forward, not as propositions for the audience to consider, but as hard-won truths; his combination of seriousness, lucidity, and conviction had an almost hypnotic persuasiveness. The seriousness was lightened by touches of humour and happy improvisations. On one occasion he was discussing whether 'All men are mortal' is a falsifiable hypothesis: suppose a man has survived various attempts to kill him; eventually an atom-bomb is exploded beneath him, but he descends smilingly to earth, brushing off the dust . . . This much Popper had, I believe, prepared beforehand: but then came a pause, a sudden smile, a new thought: 'We ask him how he does it and he answers: "Oh, it's easy; I'm immortal".' His audience, which had been small to begin with, grew to fill a large lecture theatre. He said later that LSE in those days was a marvellous institution.

His published work, during the later 1940s, was mainly on logic, more specifically on the theory of natural deduction. This brought him into contact with, among others, Paul Bernays, E. W. Beth, and L. E. J. Brouwer. The latter became a notable friend during Popper's early years at LSE (there is a letter from him, headed 'Waiting-room of Liverpool Street station, December 10th 1947', which begins: 'My dear

<sup>66</sup> BR to KRP, 18 November 1946.

<sup>67</sup> russell: the Journal of the Bertrand Russell Archives, (summer 1992), pp. 19–21.

Popper, your duality construction and your new definition of intuitionistic negation have delighted me'.)

Quite soon after he came to LSE the question of a professorship began to exercise him. He was in his middle forties, he had become a world figure, and he needed the money (in those days a London professor's salary was nearly twice a reader's). But proposals to create a chair for him had run into the difficulty that no special subject of philosophy was taught there; his logic and scientific method courses were only for optional subjects. Should he turn elsewhere? Various possibilities were in the air; some would have meant leaving England. In 1948 Victor Kraft approached him about taking up a chair, presumably Schlick's old chair, in the University of Vienna. When Carnap had asked him a few years earlier, shortly after the German surrender, whether he would consider going back to Vienna if offered a position there, Popper had answered, 'No, never!'<sup>68</sup> and he did not waver now. (Sixteen members of his family had died as victims of Nazism,<sup>69</sup> though not any of his immediate family; his father had died in 1932; Dora committed suicide in the same year; his mother died soon after the *Anschluss* from natural causes; and Annie got away to Switzerland.) At the time of his retirement in 1969 he was still deterred from the prospect of a professorship in Austria by concern about anti-Semitism there.<sup>70</sup> After Hennie's death in Vienna in 1985, he did take on a post there, as director of a new branch of the Boltzmann Institute, but only for a few months.

Another possibility was that he would fill the chair at Cambridge from which Wittgenstein resigned as from the end of 1947. Braithwaite seems to have been one of several people in Cambridge who wanted him to apply; he kept Popper posted about developments.<sup>71</sup> He seems to have been an admirer since Popper's talk to the Moral Science Club in 1936; it was he, as a syndic of Cambridge University Press, who in 1943 had encouraged Hayek to submit *The Open Society* to them.<sup>72</sup> He was not a devotee of Wittgenstein (who had heard him snore in his lectures),<sup>73</sup> and was on Popper's side at the 'poker' meeting,<sup>74</sup>

<sup>68</sup> RC to KRP, 30 May 1945, KRP to RC, 23 June 1945.

<sup>69</sup> As he reported in a subsequently deleted passage in a draft of his autobiography, Popper Collection (135, 1). Again, I learnt of this through a reference (note 33) in Hacothen, above n. 10.

<sup>70</sup> KRP to FAH, 24 October 1969.

<sup>71</sup> RB to KRP, 13 October 1947; 5 December 1947.

<sup>72</sup> FAH to EHG, 18 July 1943.

<sup>73</sup> Quoted in Ray Monk, *Ludwig Wittgenstein* (London, 1990), p. 476.

<sup>74</sup> RB.

complimenting Popper afterwards for being ‘the only man who had managed to interrupt Wittgenstein in the way in which Wittgenstein interrupted everyone else.’

Findlay also encouraged him,<sup>75</sup> and Popper seriously considered applying, but he eventually decided not to. Why? He must have learnt some discouraging information; Peter Medawar was an admirer who had quickly become a good friend; and he reports Popper asking him to say ‘with the utmost frankness whether there was anything about his manner or behaviour or reputation that stood in the way of his receiving the advancement he sought’.<sup>76</sup> It seems that Medawar told him of certain people who had been hurt by his tactlessness.<sup>77</sup> Popper himself had a nice story about himself and C. D. Broad, now the senior professor at Cambridge. Broad was interested in paranormal phenomena, and around this time Popper attended a meeting, with Broad present, at which a speaker claimed that it would be an ostrich policy to ignore the mounting evidence for such phenomena. In the discussion Popper rose to ‘say a word in favour of the ostrich’. Afterwards he suggested that this may have spoilt his chances.<sup>78</sup> (Georg von Wright got the chair, which is what Wittgenstein wanted.<sup>79</sup>)

Another possibility was that Popper would go back to New Zealand, to Otago where Jack Eccles wanted him to succeed Findlay. Matters came to a head with a cable from Dunedin which Popper received on 28 October 1948, offering him the chair and asking for a quick reply. Things moved quickly. Popper turned to Hayek who turned to Robbins, who took command. He drafted a letter for Popper to write to Carr-Saunders saying that he must decide soon and that he was tempted.<sup>80</sup> Popper sent the letter on a Friday. The following Tuesday he got an encouraging reply, and at the scheduled meeting of the Appointments Committee the next day it was agreed to ask the university to confer on him the title of Professor of Logic and Scientific Method.<sup>81</sup> His admiring lecture audience gave him a big round of applause on 15 February 1949, after seeing his professorship announced in *The Times*.

This was an exhilarating time for him. He had now accepted an

<sup>75</sup> JF to KRP, 22 November 1947.

<sup>76</sup> Peter Medawar, *Memoir of a Thinking Radish* (Oxford, 1986), pp. 113–14.

<sup>77</sup> KRP to PBM, 26 October 1948.

<sup>78</sup> EHG.

<sup>79</sup> Monk, *Ludwig Wittgenstein*, p. 507.

<sup>80</sup> KRP to JCE, 7 November 1948.

<sup>81</sup> LSE archives.

invitation from Harvard University to give the William James Lectures in 1950 (John Dewey and Bertrand Russell were among previous lecturers). He was paid ‘Hollywood rates’, as he put it (ten lectures at \$600 per lecture). His title was, ‘The Study of Nature and of Society’. It was his first visit to America. He found it ‘a marvellous country’,<sup>82</sup> and was full of enthusiasm for things American.<sup>83</sup> For instance, he said that while Negroes were admittedly a depressed class, they were the least depressed depressed class in the world. He found some of the work of Harvard graduate students ‘really outstanding’,<sup>84</sup> (perhaps he had Tom Kuhn in mind). He visited other Ivy League universities. He gave the Woodward Lecture at Yale (I have described my experience of this occasion elsewhere<sup>85</sup>), and at Princeton he gave a seminar talk on indeterminism with both Einstein and Bohr in the audience! It seems that Bohr rather took over the discussion, and then went on and on; six hours after the meeting started the room contained just him at the blackboard with Einstein and Popper as his audience. ‘He’s mad’, Einstein whispered.<sup>86</sup> In *Unended Quest* Popper described how he met Einstein three times, at the latter’s request, and tried to argue him out of determinism. A good many years later, when Bartley was editing the *Postscript*, Popper drew his attention to evidence suggesting that his (Popper’s) arguments may have had some influence on Einstein.<sup>87</sup>

Popper published a long, two-part argument for indeterminism in the first volume of the newly launched *British Journal for the Philosophy of Science*. This was the journal of the Philosophy of Science Group (later, the British Society for the Philosophy of Science), recently founded by Herbert Dingle. Popper was much involved with this, and became chairman of the group in 1951, succeeding his old friend Woodger.<sup>88</sup> In his chairman’s address, which later became chapter 2 of *Conjectures and Refutations*, he resumed his stand against

<sup>82</sup> KRP to FAH, 29 April 1952.

<sup>83</sup> See the 1950 preface to the second edition of *The Open Society*.

<sup>84</sup> KRP to FAH, 29 April 1952.

<sup>85</sup> In ‘Karl Popper: A Memoir’, *American Scholar*, 66 (spring 1997), pp. 208–9.

<sup>86</sup> KRP.

<sup>87</sup> See: WWB to KRP, 12 June, 1979; *The Born-Einstein Letters*, ed. Max Born, trans. Irene Born (London, 1971), p. 221; Karl R. Popper, *The Open Universe*, ed. W. W. Bartley III (London, 1982), p. 2, n. 2.

<sup>88</sup> Popper claimed that his 1935 lectures at Bedford College had aroused Woodger’s interest in Tarski’s work, which Woodger was now translating (*Unended Quest*, p. 108).

Wittgenstein (who had died the year before) on the reality of philosophical problems.

The money from the William James Lectures allowed the Poppers, on their return to England, to acquire what was to become a beautiful home, 'Fallowfield' in Penn, near High Wycombe. As Popper told Hayek, it had 'a really marvellous garden'. The centre of the living-room was soon occupied by an old Bechstein piano (traded in many years later for a new Steinway). They had also bought an adjoining plot, with the result that they were completely secluded. It helped to satisfy his love of nature and his desire for quietness. When Wolfgang Yourgrau first saw the garden he remarked jokingly that he could understand now why Popper's philosophy exuded such a sense of peace and calm! In New Zealand they had had a wireless and a gramophone,<sup>89</sup> but in Fallowfield the only things that made a noise, human voices apart, were the telephone, the piano, and the typewriter on which Hennie worked, usually in an upstairs room.

The house-hunting process left them exhausted, however, and at first the house filled him with dismay: 'we have ruined ourselves financially for a house which will cost many hundreds before it is really inhabitable. . . . [W]e have not slept properly for weeks because of mice and rats, and we have no proper heating arrangements, no proper hot water—nothing'.<sup>90</sup> He fell into a depression. Hennie became chronically ill and none of the doctors he got for her seemed any good. He confided to Hayek:<sup>91</sup> 'All this has somehow broken . . . me I feel a complete wreck . . . At the L.S.E. I have no friends . . . I have become highly dissatisfied with my teaching . . . Failure, failure wherever I look'. When I got back to LSE in 1950 after a year away I found his lecture audience diminished; the B.Sc. (Econ.) degree had been restructured and the student ethos seemed to favour accounting over philosophy. In early April 1952 he got a bad fright out at Fallowfield on finding Hennie collapsed and unconscious; she had suddenly fainted (and broken her cheek-bone in falling). He continued to be upset by the fact that Hayek had now left LSE for Chicago (with a childhood sweetheart as his new wife). In September 1952 he told Hayek that Hennie was much better (she had stopped seeing doctors; in those days a doctor's standard charge for a single visit was five guineas and, as she put it later, she

<sup>89</sup> JAP to EHG, 29 July 1943.

<sup>90</sup> KRP to FAH (undated, probably early November 1950).

<sup>91</sup> KRP to FAH, 23 September 1951.

became tired of being a five-guinea pig); but he still felt low: 'I try to like the School, but it has not been the same since you left, and I still feel a stranger there'.<sup>92</sup>

The charismatic glow in which he arrived in England had faded. When in 1951 Findlay, his old friend and admirer from New Zealand days, arrived at King's College London, across the Strand from LSE, he found Popper much changed: surrounded by a court of admirers, his conduct of his seminar magisterial, and his views of people moralistic and prejudiced.<sup>93</sup> Michael Polanyi was gravely offended by the treatment that Popper, as chairman, meted out to him when he read a paper (on 'The Stability of Beliefs', 6 March 1952) to the Philosophy of Science Group. David Armstrong, then an assistant lecturer at Birkbeck College, recalled that he 'went a few times to Karl Popper's seminar, but was repelled by the discipleship and the authoritarian atmosphere'.<sup>94</sup> Popper was considered by the Australian National University for a chair in Social Philosophy; they turned for advice to John Passmore, who had attended his seminar in 1948, and taken up the chair at Otago which Popper had declined. He was reported as saying that 'Popper lacked academic manners: one had to put up with being interrupted, misunderstood, prevented from getting a word in, and so on. Passmore thought this defect was compensated by the quality of what he had to say, but others thought him insufferable.'<sup>95</sup>

Popper was aware that something was wrong. I mentioned earlier his question to Medawar. Medawar consulted Ryle who told him that Popper had a reputation for being intolerant and overbearing.<sup>96</sup> Here is an entry from my diary for May 1951 (I was now an assistant lecturer at LSE): 'Popper drew me into his room and told me he was very dissatisfied with his relations with staff and students—only his relation with me seemed good. I told him that he bullied and awed people'.<sup>97</sup> People tended to find his critical intensity unnerving and sometimes invasive. He was not a good listener and had no knack for

<sup>92</sup> KRP to FAH, 10 September 1952.

<sup>93</sup> *Ibid.*, p. 40.

<sup>94</sup> David Armstrong, 'Self-Profile' in *Profiles*, vol. 4, ed. Radu J. Bogdan (Dordrecht, 1984), p. 13.

<sup>95</sup> S. G. Foster and Marjorie M. Varghese, *The Making of the Australian National University* (London, 1996), p. 105.

<sup>96</sup> Medawar, *Memoir*, pp. 113–14.

<sup>97</sup> In confirmation of this diary entry I may perhaps mention that in the letter to Hayek (10 September 1952) where he said that he still felt a stranger at LSE, he called me 'the only bright spot'.

humorous, easy-going, gossipy, academic give-and-take, tending either to dominate the conversation or to withdraw. He could deflate an ego with a flashing look of contempt; and he did not endear himself to those he accused of plagiarism.

Another negative factor with respect to LSE was his fading interest in problems to do with the social sciences. This never sank to zero: when *The Poverty of Historicism* came out as a book in 1957 he added a striking new preface and the dedication: 'In memory of the countless men and women . . . who fell victims to the fascist and communist belief in Inexorable Laws of Historical Destiny'. In 1963 he gave an interesting lecture in Harvard on the role of the rationality principle in economics and the social sciences—though it was twenty years before this was published in English.<sup>98</sup> The centre of gravity of his thinking, however, was swinging back to his pre-war concerns, centred on physics. He was especially concerned with quantum physics and the need for a thoroughly objective interpretation of it. (The third volume of the *Postscript* is given over to this.)

Then there was his attitude to tobacco smoke. In those days LSE had a no-smoking rule for lectures, but other public places tended to be smoky. In his earlier days at LSE, Popper occasionally attended the Academic Board. (When uncertain how to vote he would, he once told me, watch the then Professor of Public Administration and then vote the other way.) He had also joined in conversations in the Senior Common Room, but he stopped going to such public places as a result of his increasing allergy; this was well publicised, making him the moving centre of a smoke-free zone and causing nervous stubbing-out movements in those who strayed into it. He reduced the time he spent in the school to little more than was needed for his lecture and weekly seminar. The pattern began to be established that to have a serious conversation with him required a visit to Fallowfield. As time passed, the number of his LSE colleagues who had seen him in the flesh shrank to a handful. What Hacothen calls 'his legendary seclusion in his house outside London' had begun. Visitors to Fallowfield were usually drawn into whatever problem was absorbing him, but he occasionally disconcerted them by unburdening his bitterness about some alleged plagiarism or the misdeeds of an ungrateful pupil.

He was, however, a beacon for quite a few gifted young people in

<sup>98</sup> In David Miller (ed.), *A Pocket Popper* (London, 1983). The whole paper has since been published in Karl R. Popper, *The Myth of the Framework* (London, 1994).

the 1950s. Paul Feyerabend had met him at the Austrian College in Alpbach in 1948; they took a shine to each other and in 1952 Feyerabend came to LSE to work under him on problems relating to quantum mechanics.<sup>99</sup> Czeslaw Lejewski and 'Bashi' Sabra were also there at that time. Joseph Agassi came soon afterwards, followed by Bill Bartley and Jerzy Giedymin. Ian Jarvie moved over from Anthropology. Popper also had a good many 'corresponding' pupils who, without enrolling at LSE, kept in close touch; for example, Hans Albert generally stayed in Germany, where he battled with the Frankfurt School, though often meeting up with Popper in Alpbach.

In about 1955 Popper had begun work on an English edition of *Logik der Forschung*, to be entitled *The Logic of Scientific Discovery*. It would have many additions, some to be incorporated in the same volume as starred footnotes or appendices; others would go into a separate *Postscript*. One of the new appendices contained papers written recently during a campaign against Carnap over the nature of confirmation, or *corroboration* as Popper later preferred to call it. (Around this time Popper was also writing for the Schilpp volume on Carnap a contribution which he believed to be annihilating.) His main negative thesis was that corroboration is not probabilification. A corroboration appraisal, for example that theory  $T$  is well corroborated, or that theory  $T_2$  is better corroborated than theory  $T_1$ , is a historical report on how well the theories in question have stood up to tests so far. The degree of corroboration gained when a theory passes a test varies with the severity of the test. Let ' $p(e, h \& b)$ ' denote the probability of a predicted experimental outcome  $e$  given theory  $h$  and relevant background knowledge  $b$ , and ' $p(e, b)$ ' its probability given background knowledge alone. The severity of a test varies with the difference between  $p(e, h \& b)$  and  $p(e, b)$ . The former will normally be 1; the nearer the latter approaches 0, or the more surprising the prediction would have been if the theory had not been advanced, the more severe is the test.

The above contrast between probability and corroboration was one of several factors which had given Popper an abiding interest in the probability calculus. The long chapter on probability in his *Logik der Forschung* had contained an anticipation of modern chaos theory: namely, a way of generating from a mathematical formula, a perfectly determinate sequence of 0s and 1s which, however, would be 'random',

<sup>99</sup> Paul Feyerabend, *Killing Time* (Chicago, 1995), p. 71.

in the sense of gambler-proof, up to the first  $n$  places (after that it would start repeating itself). His first publication in English, in 1938, had presented an axiom set for the probability calculus. This was superseded by an improved version in Appendix \*iv of *The Logic of Scientific Discovery*. One problem that faced him was the following. Let  $e$  be a logical consequence of the universal hypothesis  $h$ . Then we want to say that  $p(e, h) = 1$ . But  $p(e, h) = \frac{p(e) \times p(h, e)}{p(h)}$ ; and Popper insisted that for any universal hypothesis  $h$  we have  $p(h, e) = p(h) = 0$ ; hence  $p(e, h) = \frac{0}{0}$ .

Another important contrast is between the probability of statements and the probability of events. Popper's understanding of the latter underwent an important change, in connection with his indeterminism. In *Logik der Forschung* he had adopted a frequency interpretation; he now shifted to a propensity interpretation. Take the statement that the probability of outcome  $A$  under experimental set-up  $B$  is one-half. The frequency interpretation reads this as saying that, if  $B$  were endlessly repeated, the proportion of  $A$ s in the sequence of outcomes would tend, in the limit, to one-half. That is consistent with each outcome being causally determined. The propensity interpretation reads it as saying that, even if set-up  $B$  were activated only once, or never, it always endows outcome  $A$  and outcome non- $A$  with the same weight; thus the outcome is never causally determined.

The propensity interpretation is objectivist in the sense that it postulates weightings 'out there' answering to numerical concepts in our theories. That is in line with Popper's metaphysical realism. The latter also manifested itself in his views, which he first presented in a series of one-page letters to *Nature* during 1956–8, about the 'arrow of time'. His fundamental conviction was that time has its unique direction independent of whatever processes occur in it; hence it is not tied to the direction of entropy-increase. Suppose that a system (which might be the universe) undergoes certain processes between  $t_1$  and  $t_2$ , at which time these processes are perfectly reversed. Then the system goes back to the state it was in at  $t_1$  but time goes forward to  $t_3$ . He also claimed that there are classical processes, not involving entropy, which are in fact irreversible (though we can easily imagine them being reversed); for instance, an explosion that sends out an expanding circle of disturbance towards infinity.

Popper spent 1956–7 at the Stanford Center for Advanced Studies.

He was a disappointing visitor because he was working furiously trying to finish the *Postscript*, and they saw little of him. He had the galleys with him when he conducted a seminar in Alpbach in 1958, but Bartley, who had recently arrived from Harvard to study under him, persuaded him that it needed a lot of revising, and he duly embarked on this. Then he began to have worrying eye trouble (he feared he was going blind). He gave the Annual Philosophical Lecture for the British Academy, in January 1960 (it was entitled 'On the Sources of Knowledge and Ignorance' and became the Introduction to *Conjectures and Refutations*), and a few days afterwards he left for Vienna for operations on both eyes for detached retinas. The operations were successful, but he could not bring himself to resume correcting and proof-reading the *Postscript*, which was set aside. (It was eventually published, in three volumes under Bartley's editorship, only in 1982–3.)

Popper was happier now at LSE. His department, though small, had acquired a considerable reputation. It is perhaps significant that Russell in 1960 recommended a philosophically interested sixth-former to go to LSE, where the philosophy 'has the merit of being vigorous', rather than to Cambridge.<sup>100</sup> Oxford philosophers' attitudes to Popper varied. He was largely ignored by J. L. Austin and new-wave Ordinary Language philosophers, whom Popper likened to people who are always compulsively cleaning their spectacles instead of looking through them at the world, but Hampshire, Hare, Kneale, Quinton, Robinson, and Ryle admired him, and he was coming to be accepted as part of the British philosophical establishment. True, his actual presence was not in great demand, at least in Britain; he tended to be the big man who was *not* invited, but his writings were much sought after, contributions being invited to such prestigious anthologies as *Contemporary British Philosophy* (1956) and *British Philosophy in the Mid-Century* (1957). In 1958 he was elected a Fellow of the British Academy. This was a busy year for him. At the International Congress of Philosophy in Venice that year he presented a beautiful little historical piece on the significance of Leibniz's criticisms of Descartes for the developments in physics from atomism to the Faraday-Maxwell field theories.<sup>101</sup> He also gave the presidential address to the Aristotelian Society (with Sir David Ross in the chair): entitled 'Back to the Pre-Socratics', it drew a

<sup>100</sup> Grattan-Guinness, 'Russell and Karl Popper', p. 8.

<sup>101</sup> 'Philosophy and Physics', reprinted in *Quantum Theory and the Schism in Physics* (ed. W. W. Bartley III, London, 1982), pp. 165–73, and in *The Myth of the Framework*, ch. 5.

distinction between a *school*, such as that of Pythagoras, which makes it its task to preserve a doctrine, and a *critical tradition*, such as that inaugurated by Thales, based on a new relation between master and pupil, with the master tolerating, even encouraging, pupils' criticisms.

He had a powerful friend and supporter in Robbins, who became something of a father-figure for the Philosophy Department. (There had been a temporary coolness between them when Hayek left LSE for Chicago; Robbins felt bitter about this, but Popper remained loyal to the man who had 'saved his life' by rescuing him from New Zealand.) Robbins used to say that at that time there were two people of genius at LSE (the other was Bill Phillips of 'Phillips machine' and 'Phillips curve' fame). Robbins was one of the very few people to whom Popper was deferential. Perhaps I may reproduce here an amusing illustration of this. As Chairman of the Trustees of the Covent Garden Opera House, Robbins regularly invited friends to join his party in the royal box. In due course he invited Popper, adding that unfortunately a dinner-jacket is essential. Although Popper had, as we will see, good reason to decline this particular invitation, it was to him like a royal command. Julius Kraft once remarked to me, with a touch of exaggeration, that in the Popper household 'cooking an egg causes a great deal of excitement.' Well, acquiring suitable evening wear caused even greater excitement. Urgent appeals went out in all directions for advice and assistance. On the day in question, I drove the Poppers to the Gombichs', where he changed. Eventually the bow-tie was fixed and everything seemed in place, but he had a last adjustment to make: he had brought a supply of cotton wool with which to stuff his ears, the opera being by Richard Strauss whose music he could not abide!

### Lakatos

Popper inspired in many of those who came to know him well an enduring affection, love, or devotion. Among those whose names have already come up one thinks of Jack Eccles, Herbert Feigl, Ernst Gombrich, 'Fritz' Hayek, Bryan Magee, Peter Medawar, Peter Munz, and Lionel Robbins. Others in this category include Donald Campbell, Ralf Dahrendorf, David Miller, Alan Musgrave, and Colin Simkin. But there was also a category of broken friendships, which came to include quite a few of his pupils: Joseph Agassi, Bill Bartley, Paul Feyerabend,

Imre Lakatos and, eventually, myself. (In Bartley's case friendship was restored, but that was exceptional.) Obituarists who avert their gaze from this area, as some do, leave out something emotionally important in Popper's life. There is no doubt which case carried the highest emotional charge; anyone who knew him at all well in his later years will have heard angry denunciations of Lakatos. Since his case is well documented, and has probably aroused more interest than the others, I will concentrate on it. Telling this substory uninterrupted will mean running on ahead of the main story.

His case exemplifies what was said earlier both about Popper being a beacon for younger people, and about the interest of his critique of Marxism for intellectuals under Communist regimes. Lakatos had been an enthusiastic party man under the Communist regime in Hungary after the war. While still in his twenties he had risen to an important position in the Ministry of Education. And then, in 1950, after returning from a year at Moscow University, disaster struck. He was caught up in the Rajk Purge and imprisoned. (He claimed, and I can believe it, that the strain of interrogation had proved too much—for one of his interrogators!) He was released in 1953, and gained asylum in the Hungarian Academy of Science, where he had the run of the library. He still believed that Marxism was grounded on a scientific theory of history; and now he learnt that a certain Karl Popper had exposed this as a sham.<sup>102</sup> After the Uprising in late 1956, he got to Vienna, where he found his way to the university and introduced himself to Victor Kraft (who had filled the chair which Popper declined). He learnt that there was a possibility of a Rockefeller Fellowship, and said that he would like to study under Popper. Kraft advised against: Popper was a difficult man.<sup>103</sup> Then was there anyone in England with similar ideas? Yes, there was Braithwaite. So Lakatos went to Cambridge, where he embarked on a Ph.D., but he retained his desire to join Popper. Some three years later he delighted Popper and his seminar with his dramatised case-study of the 'Descartes-Euler conjecture', and he joined Popper's department in 1960.

It was not to be expected that coexistence with this colourful, irreverent man with his sharp wit and inexhaustible energy would be smooth and uneventful. There were flare-ups and occasional rows, often over typing assistance; Hennie was still doing a lot of typing for Popper,

<sup>102</sup> IL.

<sup>103</sup> IL.

but her burden had been reduced by secretarial assistance in the department, and Popper was fiercely jealous of this. However, there was an underlying amity. Lakatos's humour brought out an unsuspected playfulness in Popper. Lakatos had a habit of sending out jokey postcards; he once got one back from Popper, in Alpbach; its picture had been touched up. 'It's good', Lakatos commented, 'but what work he puts into being funny.' Here is Popper, in 1962, writing to Lakatos a few months after some flare-up:

Dear Imre, It is very embarrassing that I have to tell you such a thing, but as, I suppose, you will prefer to hear from me direct rather than from another source, I better tell you quite frankly and openly to your face that . . . (I hope you are thoroughly frightened by this time) . . . I am about to receive an honorary degree of Doctor of Laws from the University of Chicago.<sup>104</sup>

(The President awarded it to him in hospital; he had had some cardiac trouble.) And here is Popper, in 1965, telling Lakatos of another honour: 'The postman brought a letter "On Her Majesty's Service". Hennie thought "Income Tax". When she opened it she got a terrible shock. . . . [S]he got me first to bed, and only then showed it to me'.<sup>105</sup> (It was from Downing Street, sounding him about a knighthood.) Popper was enchanted by Lakatos's paper 'Proofs and Refutations'. He told Lakatos that it was 'a flawless piece of art, and the greatest advent [*sic*] in the philosophy of mathematics since the great logical discoveries around 1930–32'.<sup>106</sup> Lakatos spent 1964–5 in La Jolla, and the letters which Popper received from him then were, he said, 'a real delight, and a real tonic'.<sup>107</sup> Popper was worried that he might not come back: 'I am sad that you are away, simply because I am much happier with you here. I hope you will come back (the sooner the better); it would be a very great loss for me personally (to say nothing about the department) if you don't.' This letter contains 'a clear declaration of love'.<sup>108</sup> In America Lakatos was planning a colloquium, to be held at Bedford College, London, in July 1965. Popper had at first been lukewarm about the proposal, but in the event he supported it whole-heartedly.

In the late 1960s Lakatos turned increasingly from the philosophy of mathematics, where his ideas could be seen as an extension into new terrain of Popper's conjectures-and-refutations view of human

<sup>104</sup> KRP to IL, 12 December 1962.

<sup>105</sup> KRP to IL, 10 January 1965.

<sup>106</sup> KRP to IL, 11 August 1964.

<sup>107</sup> KRP to IL and WWB, 11 October 1964.

<sup>108</sup> KRP to IL, 15 December 1964.

knowledge, to the methodology of the empirical sciences, where his ideas were increasingly in competition with Popper's. When he sent Popper a piece of his work, he now tended to scribble on it the advice not to read it, and Popper tended to follow this advice. The start of their big rupture can be dated rather precisely. Back in 1963 Popper had received a major philosophical honour: he would be the hero of a volume in the Library of Living Philosophers, edited by Paul A. Schilpp; but it took several years for the list of contributors to be finalised. Lakatos had originally been asked to write on Popper and mathematics, but Popper suggested that he take the opportunity to bring together his scattered criticisms and present them in an orderly form. Lakatos agreed. In the meanwhile, during 1968–9, with student revolutions going on at LSE and other places, Popper was working on his intellectual autobiography for the volume (later published separately as *Unended Quest*). He was due to retire from LSE in September 1969. Regarding the pension he would get as grossly inadequate, he had arranged to spend the autumn of 1969 teaching at Brandeis University to make some money. Until he had finished the autobiography he was not allowed to turn to the critical contributions to the Schilpp volume, and he was still working on it when he arrived at Brandeis. He finally sent off this first draft on 7 October.

He was now at liberty to turn to the mountain of critical contributions which awaited him (they would occupy nearly 800 printed pages in what would become a monster volume). The task of replying was all the more daunting because he had a genuine aversion to reading about himself. He had not read Lakatos's contribution when the latter came over from Boston to give a lecture at Brandeis on 9 October. There had been some tension between them during the previous year (the student troubles at LSE did not help), but after this meeting Popper wrote him a friendly letter beginning, 'My dear Imre, It was very nice to have seen you here'.<sup>109</sup> Then, on 30 October, he wrote him a furious letter, sparked by a hurried look at Lakatos's long contribution, not to the Schilpp-volume, but to an anthology on Kuhn.<sup>110</sup> There was worse in store in Lakatos's essay for the Schilpp volume. Entitled 'Popper on Demarcation and Induction', this raised difficulties with Popper's 'solutions' of his two basic problems. As to demarcation: Popper had

<sup>109</sup> KRP to IL; the date, 28 October 1969, is in Lakatos's hand.

<sup>110</sup> *Criticism and the Growth of Knowledge*, ed. Imre Lakatos and Alan Musgrave (Cambridge, 1970).

rhetorically asked in connection with Freudian pseudo-science what evidence ‘would refute to the satisfaction of the analyst not merely a particular analytic diagnosis but psycho-analysis itself?’<sup>111</sup> Lakatos agreed that Freudians had been nonplussed by this challenge; but what if we direct a similar challenge to Newtonian scientists; what evidence would refute not merely a particular application of it but Newtonian theory itself? As to induction: Lakatos did not of course claim that the method of science is inductive; he accepted that Popper had radically transformed our view of the way in which scientists, especially great scientists, typically proceed, but he did claim that without some inductive assumption Popper’s philosophy could provide no bridge from a scientific theory’s theoretical acceptability (‘acceptability<sub>2</sub>’) to its pragmatic reliability (‘acceptability<sub>3</sub>’).

I would have preferred to pass over what comes next. In January 1970, back from Brandeis and now retired, Popper paid a visit to his old seminar, now presided over by Lakatos and myself. He told a friend that he had come back to an icy house, and found the LSE ‘similarly icy’.<sup>112</sup> His subject was induction. Lakatos’s criticisms of Popper on induction were well known to most members of the seminar, and we were curious to know what he would say in reply. Lakatos had told him beforehand that he would have to slip away ten minutes before the two-hour seminar was due to end, at 4 o’clock. Popper went on talking, with no mention of Lakatos or his criticisms, until 3.50. It was not a happy occasion. Younger members of the department tended to take Lakatos’s side in the worsening conflict between the two men. After this one occasion he stayed permanently away from his old seminar.

In his written reply in the Schilpp volume, he declared that if Lakatos’s criticism on demarcation were true, ‘then my philosophy of science would not only be completely mistaken, but would turn out to be completely uninteresting’. The idea that what drives a good critical tradition is pupils criticising the master was overtaken, as Popper grew older, by a tendency to see those criticisms which he was unable to rebut quickly and effectively as threatening to deprive him of his achievements—‘all that work wasted!’ as he once bitterly put it to me. Popper never explicitly addressed Lakatos’s criticism on induction. His *Objective Knowledge* (Oxford, 1972) opens with the words, ‘I think I have solved a major philosophical problem: the problem of induction’.

<sup>111</sup> *Conjectures and Refutations* (London, 1963), p. 38n.

<sup>112</sup> KRP to CS, 22 January 1970.

A reference to Lakatos was expunged (though not from the index) and thereafter he never again referred to Lakatos in print. Lakatos died, quite suddenly, on 2 February 1974, before the Schilpp volume was eventually published, but Popper's bitterness towards him continued unabated.

### The 1960s

I now resume the main story, which had reached 1960. In that year Popper introduced a striking new idea at the first International Congress for Logic, Methodology, and Philosophy of Science, in Stanford, the idea of *verisimilitude*. He developed it further when he gave the Sherman Lectures, in November 1961 at University College London (with a flattering number of Nobel Laureates and Fellows of the Royal Society in the audience). Let theory  $T_2$  be an advance on its falsified predecessor  $T_1$ , and suppose that, as typically happens,  $T_2$  subsequently gets falsified in its turn. We might want to say that, though false,  $T_2$  is closer to the truth than  $T_1$ . What could that mean? He gave an original and deceptively simple and persuasive answer (published in chapter 10 of *Conjectures and Refutations*, 1963):  $T_2$  has more verisimilitude than  $T_1$  if: (i)  $T_2$ 's truth content (those of its logical consequences that are true) includes  $T_1$ 's, (ii)  $T_2$ 's falsity content (those of its logical consequences that are false) is included in  $T_1$ 's; and (iii) at least one of these inclusions is strict. He considered this a major achievement. It tended to mitigate what many saw as the pessimism of his earlier philosophy of science. Suppose we have a historical sequence of scientific theories,  $T_1, T_2, T_3$ , in which  $T_2$  gained a corroboration from a test which falsified  $T_1$ , and  $T_3$  gained a corroboration from a test which falsified  $T_2$ . In 1934 Popper could have drawn no conclusions from this pattern of corroboration as to the truth of the latest theory, but he now claimed that corroboration is an *indicator* of verisimilitude.<sup>113</sup> Some saw this as a welcome, others as an unwelcome, shift away from his original, austere non-inductive position. During a visit by Popper to New Zealand in 1973 Pavel Tichy showed him, in a seminar at Otago University, that his definition of verisimilitude breaks down. (This was also shown independently by David Miller.) Popper took this criticism calmly.

<sup>113</sup> *Objective Knowledge* (Oxford, 1972), p. 103, his italics.

Further striking new ideas were presented in 1965 in a lecture at Washington University entitled 'Of Clouds and Clocks'. This countered the thesis of classical atomism that seemingly 'cloudy' structures are really made up of clock-like bits with the contrary thesis that seemingly clock-like structures are really statistical aggregates of 'cloudy' bits. It also introduced the idea of a *plastic* (as opposed to *iron*) control. He had not paid much heed to Darwinism hitherto; there are no significant references to it in *The Open Society* and *Conjectures and Refutations*, and in *The Poverty of Historicism* he had endorsed a dismissal of the clash between Darwinism and Christianity as 'a storm in a Victorian tea-cup', but in this lecture he apologised for doing that, and declared the Darwinian theory of evolution very important. However, it was not easy for him, from his falsificationist standpoint, to account for the importance of this theory, which he declared 'almost tautological'.<sup>114</sup>

In the Herbert Spencer Lecture which he had given in 1961, but which was not published until 1972, he put forward a hypothesis which he undervalued and indeed actually forgot.<sup>115</sup> I call it the Spearhead Model of evolutionary development.<sup>116</sup> It argues from the assumption that an organism's control system and its motor system are sharply distinguishable, each with its own genetic basis, to the conclusion that in evolutionary developments control systems lead the way with motor systems following in their train; for control capacity may exceed motor power whereas the converse would be disadvantageous (he actually said 'lethal').

## Retirement

His retirement was long and unflagging. One of his oldest concerns was the mind-body problem, perhaps the most difficult of all philosophical problems. He brought to it ideas from various domains, including his old teacher Bühler's ideas about the expressive, signalling and descriptive levels of language. (He was always seized by the importance of language; one of his complaints about linguistic philosophers was that they had no philosophy of language.) He also brought in ideas from biology. At heart he was a Cartesian interactionist. (He once gave a talk,

<sup>114</sup> See *Unended Quest*, § 37.

<sup>115</sup> KRP to JW, 7 October 1969.

<sup>116</sup> In 'Popper and Darwinism' in Anthony O'Hear (ed.), *Karl Popper: Philosophy and Problems* (Cambridge, 1996), pp. 191–206.

at Oxford I think, contrasting his ideas with Ryle's in *The Concept of Mind*. His auditors repeatedly asked where Ryle had ever denied it. Finally, he declared that he believed in the ghost in the machine; they admitted that Ryle had denied that.) But his position on the mind-body problem became complicated by his increasing preoccupation with a World 3 of objective ideas existing independently of their origin in World 2 (subjective mind). The classic problem of relations between World 2 and World 1, or between minds and bodies, tended to get nudged aside by this other problem of the relation between World 2 and World 3. He presented his ideas on these issues in lectures at Emory University in 1969 (later edited by Mark Notturmo and published with the title *Knowledge and the Body-Mind Problem: In Defence of Interaction* (London, 1994). His concern to establish the reality of World 3 sometimes made it sound almost Hegelian, with scientific theories in it exerting, via World 2, an influence upon World 1. But he answered 'I don't think so' when asked whether World 3 ever *initiates* anything.<sup>117</sup> These ideas were developed in *The Self and its Brain* (New York and Berlin, 1977), half of which was written by him and the other half by Eccles.

In 1983 he published, in collaboration with David Miller, a two-page letter in *Nature* which aroused wide interest. Let  $h$  and  $e$  say respectively, 'All swans are white' and 'All observed swans are white'; since  $p(h, e) > p(h)$ , it looks as though inductive support and probabilistic support work together. However, Popper and Miller pointed out that  $h$  can be factorised into two conjuncts: (i)  $e$  and (ii)  $h \leftarrow e$  ( $h$  if  $e$ ), where (ii) represents that part of the content of  $h$  which goes beyond  $e$ ; and  $e$  actually *lowers* the probability of (ii) (and raises the probability of (i) to 1, of course).

Popper went through a harrowing time during Hennie's long terminal illness. After her death, in 1985, he sold Fallowfield and moved to Kenley, where he worked on indefatigably, now assisted by Melitta Mew. He was in the news in August 1988. It seems that the Soviet delegation, which had undertaken to provide simultaneous translations of talks from and into Russian at the World Congress of Philosophy at Brighton, preferred to go on a sightseeing tour on the day the author of *The Open Society* was to give an address. An expanded version of this address went into *A World of Propensities* (Bristol, 1990). He published a collection of essays and addresses in 1992 and another in 1994.

<sup>117</sup> *Knowledge and the Body-Mind Problem*, p. 44.

A walk from this new house took him past a glider club; he was fascinated by the takings off and landings, and persuaded someone there to take him up. After that he would have liked to take lessons. This did not happen, but he did go up a second time at the suggestion of a German television company.<sup>118</sup> He was now comfortably off and could indulge his love of antiquarian books; perhaps he was also driven by memories of his father's lost library. He acquired first editions of Galileo's *Two Dialogues*, Hobbes's *Leviathan*, Gilbert's *De Magnete*, Hume's *Treatise*, Kant's three *Critiques*, and several of Kepler's works. With Newton's *Principia* and *Opticks* he had to be content with second and third editions.

There were many honours and awards, including: Sonning Prize 1973; Companion of Honour 1982; de Tocqueville Prize 1984; Catalunya Prize 1989; Kyoto Prize and Goethe Medal 1992; Otto Hahn Peace Medal 1993. His work has been translated into some forty languages, including some unusual ones, such as Mongolian. In his old age he was much sought out by world leaders and elder statesmen, including the then German President (Richard von Weizsäcker), the Emperor of Japan, the Dalai Lama, Helmut Schmidt (who visited him several times), Helmut Kohl (who publicly saluted him as a champion of the open society), Mario Soares, and most recently Václav Havel. A man who delivered a new garden seat was told that it was a pity its predecessor was no longer usable; many famous bottoms had sat on it.<sup>119</sup>

I will conclude with some scattered observations. The sheer amount of work he got through, sometimes working right through the night, is extraordinary. This was despite intermittent poor health. True, some of this may have been due to overwork and he was a bit of a hypochondriac; but frightening attacks of tachycardia began in his mid-fifties and he had frequent bouts of pneumonia. Yet he travelled a lot, especially in his later years. He also kept up an enormous correspondence (Hennie was heavily involved in this, not only typing much of it, but complying with his demand to preserve everything.) He had virtually no relaxations, apart from music, and some reading of such authors as Jane Austen, Anthony Trollope, and Hugh Lofting. *The Times*, which used to be delivered at Fallowfield mainly for Hennie's benefit, was stopped in about 1960. Apart from Alpbach, which usually made a happy break

<sup>118</sup> Melitta Mew.

<sup>119</sup> Robin Watkins.

for him though he hardly relaxed while there, I remember him taking off only two days (they both happened to be in March 1965), once to be knighted and once to visit Whipsnade Zoo. His enthusiasm for routine lecturing waned after about 1950. He preferred answering off-the-cuff, and questions from the audience were increasingly encouraged. The kind of teaching he enjoyed most was a tutorial with just one gifted pupil; for instance, Elie Zahar benefited in this way—as a doctoral student at LSE he was allowed the use of Popper's room, and when Popper came in to eat his sandwiches he might explain how, say, Ernst Mach constituted minds and bodies out of the units of his neutral monism. He could be over-confident of his own opinions. When he was in his late eighties someone who became a colleague in 1948, and whose appearance had changed considerably since, said to him, 'I'm John Wisdom'; 'No you're not' came the reply!<sup>120</sup> He was physically small, with an expressive face. He could not dissemble his feelings; when he was happy they shone out and when he was angry they blazed out. He read with intense concentration, his eyes seeming to suck the meaning from the page.

He enriched the English language with some striking labels, or 'right-to-left definitions' to use one of them, for important ideas; for instance, 'the bucket theory of the mind', 'horizon of expectations',<sup>121</sup> 'moral futurism', 'the theory of manifest truth', and of course 'the open society'. He had a partiality for lowbrow English expressions like 'not my cup of tea' and 'I may pop off any time'. He took us aback at a conference on his philosophy at LSE in 1980 by announcing, after a talk in which Grünbaum had presented counter-examples to his thesis that Freud's psychoanalytical hypotheses are untestable, 'I may have to eat humble pie'.<sup>122</sup> He had a good line in self-deprecation; for instance, when answering Hilary Putnam in the Schilpp volume, he compared this 'leader of the younger generation of logicians' with himself, 'a tottering old metaphysician'.

As well as being a philosopher of unusual depth and clarity, the sheer range of his intellectual interests was astonishing. Among the more philosophically off-beat subjects on which he made interesting contributions are the rise of polyphonic music, and the very first

<sup>120</sup> Hans Reiss.

<sup>121</sup> David Miller tells me that Husserl had this expression.

<sup>122</sup> See the 1980 addition on p. 169 to his *Realism and the Aim of Science* (London, 1982); the acknowledgement to Bartley should have been to Grünbaum.

publishing of books (which he took to be Homer's *Iliad* and *Odyssey* in Athens around 550 BC; he suggested that 'printing' them, i.e. having them handwritten on papyrus by slaves, may have been less of a problem than marketing them).<sup>123</sup>

A remark in the Preface to *The Open Society*, 'Great men may make great mistakes', might have been a motto for much of his work; giant-slaying was a persisting motif. One thinks not only of individuals such as Plato, Hegel, Marx, and Freud, and perhaps Wittgenstein and Carnap, but also of various '-isms': he claimed to have slain logical positivism single-handed;<sup>124</sup> and there was essentialism, historicism, holism, probabilism, verificationism. But he also contributed importantly to our appreciation of various pre-Socratic thinkers, especially Anaximander, Heraclitus, Parmenides, and Democritus, and wrote generously and illuminatingly about various individuals; for instance, Kant, Schopenhauer, Boltzmann, and Tarski.

Natural scientists generally take a low view of philosophy of science, but some distinguished ones have been enthusiastic about Popper's ideas in this area. We saw Einstein taking them seriously in 1935 and again in 1950. Popper inspired a lifelong admiration in Eccles (already an FRS when he attended Popper's lectures on scientific method at the University of Otago in 1945),<sup>125</sup> and in Medawar;<sup>126</sup> both men found his conjectures-and-refutations view of science liberating. Popper's interest in problems to do with evolution and biology led to friendships with Ernst Mayr, and Alister Hardy and a renewal of his early friendship with Konrad Lorenz. Popper first met Erwin Schrodinger in 1936; after the war they often met in Alpach. This was one of several friendships resulting from Popper's lifelong concern with quantum physics; others included Alfred Landé, David Bohm, Jean-Pierre Vigié, and John Bell. Herman Bondi was another admirer and friend. Nobel Laureates who admired him included Percy Bridgman (they met in Harvard in 1950), Dennis Gabor, and Jacques Monod (who introduced his philosophy of science to the French-speaking world with his preface to a translation of *The Logic of Scientific Discovery*; his brother Philippe translated *The Open Society*). It is fitting that Popper was one

<sup>123</sup> See *In Search of a Better World* (London, 1992), ch. 7.

<sup>124</sup> See *Unended Quest*, § 17.

<sup>125</sup> See Eccles's contribution to the Schilpp volume on Popper.

<sup>126</sup> See Medawar, *Memoir*.

of the small group who have been Fellows of the British Academy and of the Royal Society.

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*Note.* I have drawn freely on Popper's autobiography, *Unended Quest* (Fontana, 1976), usually without footnote references. Another main source has been the microfilm copies, prepared by the Hoover Institution and made available to the British Library of Political and Economic Science, of papers in the Sir Karl Popper Collection in the Hoover Institution Archives, Stanford, California; letters are identified by author, recipient, and date, other papers by box and folder numbers. A reference consisting just of one person's name or initials will indicate a personal communication, oral or written.

I use the following abbreviations:

WWB = Bill Bartley; RB = Richard Braithwaite; AB = Alfred Braunthal; RC = Rudolf Carnap; JCE = John Eccles; HF = Herbert Feigl; JF = John Findlay; EHG = Ernst Gombrich; FAH = Fritz Hayek; IL = Imre Lakatos; CL = Casimir Lewy; PBM = Peter Medawar; JAP = Hennie Popper; KRP = Karl Popper; BR = Bertrand Russell; JW = John Watkins.