

Lakatos, Popper, and Feyerabend: Some Personal Reminiscences

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Talk at UCL on 28 February 2011

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1. Introduction

The aim of this talk is to give some personal reminiscences of three famous philosophers of science whom I had the good fortune to meet while I was a graduate student working on my PhD. These were Lakatos, Popper and Feyerabend. But how did I come to know these characters? I had better explain the background.

In the period 1962-1966, I was an undergraduate at Cambridge. I studied mathematics for 2 years and then philosophy for 2 years. My first year of philosophy, which covered general philosophy, was therefore the academic year 1964-5. Wittgenstein had already been dead for 13 years by that time, but his spirit still haunted Cambridge. One of the lecturers (Michael Tanner) began his course by saying that the premise of his lectures was that Wittgenstein was the greatest genius of the 20th century. Note that he regarded Wittgenstein not just as the greatest philosophical genius, but as the greatest genius of any kind. I spent much of that year reading the *Philosophical Investigations*, and one of my contemporaries was rumoured to have learnt the whole work off by heart.

My second undergraduate year in philosophy (1965-6) was devoted to Logic and Philosophy of Mathematics and Science. As the academic year drew to its close, I had decided to try to do a PhD in philosophy of mathematics. I therefore read a lot of the recent literature in philosophy of mathematics to try to find an interesting topic to pursue. Of all the papers published on the subject in the last few years, one stood out as being easily the most brilliant and interesting. I think one of my lecturers (Tim Smiley) had recommended that I should read it. It was of course 'Proofs and Refutations' by Imre Lakatos which was published in the *British Journal for the Philosophy of Science* in the years 1963-64. I accordingly formed the plan of doing a PhD on philosophy of mathematics with Imre Lakatos. I had in mind some further investigation along the lines of Proofs and Refutations. I therefore wrote to Imre Lakatos to ask him if he would take me on as a PhD student. He wrote back suggesting that I should come to see him about it

in the London School of Economics. As this first meeting with Imre Lakatos marked a new era in my life, I will describe it in the next section.

2. First Meeting with Lakatos

In the summer of 1966 when I first met Lakatos, he was 43 years old and a lecturer in the department of philosophy at LSE. Yet he had not in fact been a professional academic for very long. This was because Lakatos had first planned to become a politician rather than an academic. Indeed Lakatos once told me that his first ambition had been to become prime minister of Hungary. Some details of his life in Hungary are to be found in Long (2002). In 1940 he started at the University of Debrecen where he studied mathematics, physics, and philosophy. It was during his undergraduate years that he started studying Marxism, and joined the illegal communist party. One of Lakatos' jobs in the party was to teach Marxism to new recruits who often didn't know much about it. This he did in an inspiring manner. After the war, the communists took power in Hungary, and Lakatos though only 23 became for a while a powerful official. However, Lakatos' years in power were not long. He suffered the fate of very many communist party officials. In the night of 20 April 1950 he was arrested and transferred to a basement cell of the secret police headquarters. From there he was sent in September to the Reck forced labour camp, which had been set up on the Stalinist model. He remained a prisoner for 3 years until 3 September 1953. After his release, it was obvious that his political career was in ruins. Lakatos got a job as a librarian, and started studying problems connected with the heuristics and development of mathematics. Then in 1956 came the Hungarian revolution, and the border with the West was opened briefly. Lakatos with his wife and her parents crossed the border on 25 November having walked 12 miles carrying bags. According to Long (p. 291): "They heard gunfire (the border was rapidly being resealed by that point). The first snow began to fall."

Once he arrived in the West, however, things went better for Lakatos. He obtained a Rockefeller Fellowship to do a PhD at King's College Cambridge on philosophy of mathematics with Professor Braithwaite. The thesis was completed in 1961, and Lakatos published his famous paper: 'Proofs and Refutations', an extract from the thesis in 1963-4. (Photos of Lakatos getting his PhD and as a Professor at LSE).

As soon as I met Lakatos, we plunged into a conversation about philosophy of mathematics and science. This was the first of many such conversations which often would go on for hours. What surprised me both at the time and later was how well I got on with Lakatos – at least initially, since we quarrelled later. This seemed odd since we had had such different experiences of life. I only found out many of the details of Lakatos' life in Hungary much later, but it was obvious from the start that I had led a much more sheltered life than he. Perhaps the secret of our rapport was just that we had a common interest in the history and philosophy of mathematics and science.

Lakatos was a most entertaining person with whom to discuss philosophy. I remember that at our first meeting, it was not long before I uttered the name: 'Wittgenstein'. Lakatos immediately replied: "Wittgenstein was the biggest philosophical fraud of the twentieth century." Naturally, as I was used to the Wittgenstein cult at Cambridge, this statement came as quite a shock to me. So I replied: "Dr Lakatos, what you say is truly surprising for me because I have just finished writing an essay in which I maintain that there are close links between your concept of mathematical

proof and Wittgenstein's." (Initially I addressed Lakatos politely as 'Dr Lakatos', but after a few months, he summoned me to his office and uttered the following words: "Donald, I am old enough to be your father, but still you must call me 'Imre'." This I did thereafter. Lakatos was 21 years older than me, and so what he said was true, though I could not help reflecting that he did not greatly resemble my own father.)

At my next meeting with Lakatos, he took up the Wittgenstein theme again. He said: "Regarding Wittgenstein, I looked through my copy of his *Remarks on the Foundations of Mathematics*, and I was surprised to find that I had written enthusiastic notes in the margins. But these notes were written in Hungarian which means that I must have written them ten years ago, just after I had arrived in England."

What were Lakatos' opinions at the time of our meeting? There is no doubt in my mind that Lakatos in the first two years I knew him (1966-68) was a sincere admirer of Popper and indeed regarded himself as a loyal follower of Popper. Lakatos probably made contact with Popper quite soon after his arrival in England – though I am not sure exactly when. He read an earlier draft of *Proofs and Refutations* at the Popper seminar in March 1959, and in his acknowledgements to the published version of the paper says (1963-64, p. xii):

"The paper should be seen against the background of Pólya's revival of mathematical heuristic, and of Popper's critical philosophy."

It is obvious, moreover, that the full title of the work: *Proofs and Refutations. The Logic of Mathematical Discovery* refers to two of Popper's most famous books: *Conjectures and Refutations* and *The Logic of Scientific Discovery*. This is not to say that Popper was the only influence on Lakatos' *Proofs and Refutations*. There are clear signs of the influence of the philosophies which Lakatos had studied in Hungary, namely Marxism and Hegelianism.

I began working on my PhD with Lakatos in October 1966. The first thing Lakatos told me was that I should begin by reading the entire works of Popper, because they were essential. This is another indication of the great enthusiasm which Lakatos had for Popper's philosophy at that time. Initially I objected to this advice on the grounds that I wanted to write a PhD thesis on philosophy of mathematics and that Popper had written very little on this subject. However, Lakatos quickly disposed of this objection by saying that one could not do philosophy of mathematics properly without a thorough knowledge of philosophy of science. So I duly started reading Popper's works. In fact I had at that time read very little of Popper. I had attended a lecture course on philosophy of science in my last year at Cambridge. It was given by Hugh Mellor, then in his first year as a lecturer in the department of philosophy. Hugh Mellor had recommended the students on the course not to read Popper. This was symptomatic of the feud which existed at that time between Popper and Cambridge. I was to become all too aware of this feud as time passed. Despite Hugh Mellor's recommendations, I read one small piece of Popper at Cambridge, namely Chapter V of *the Logic of Scientific Discovery*, entitled: 'The Problem of the Empirical Basis'. I remember thinking when I began the chapter that it would probably not be much good, but that I would look through it for the sake of completeness in the reading I was doing for my essay. To my surprise, I found the chapter extremely interesting and impressive. It put forward a view on the subject which was new to me, and was quite different from what I had been expecting. Still, until Lakatos gave his orders, I had not followed up on this initial favourable impression. However, in obedience to Lakatos, I started reading Popper carefully, and soon found myself completely enchanted by Popper's philosophy.

I will return to Lakatos later, but it is time now to introduce another character of our drama. I was the PhD student of Lakatos when I arrived in the LSE philosophy department, but Lakatos was by no means the dominant figure in the department at that time. Lakatos, and everyone else in the department, was very much in awe of the commanding figure of Popper. In the next section I will describe Popper and his activities in 1966 when I first got to know him.

3. Popper and the Popper Seminar

In the autumn of 1966 when I first joined the department of philosophy at LSE as a PhD student, Popper was at the height of his fame. He had been knighted the previous year (1965), and, properly speaking, I should refer to him as Sir Karl, but I will continue all the same just to call him: 'Popper'. At that time, Popper was 64 years old, Lakatos was 43, and I was 22. So we were representatives of 3 generations of philosophers. Popper lived on the outskirts of London in Penn, Buckinghamshire with his wife. They were a devoted couple, but had no children. I visited Popper in his house in Penn on two occasions. It was a pleasant house surrounded by a spacious garden. I think it was also near open country where one could go for walks. However, it was a long way from the centre of London, and Popper only visited LSE on one day a week – Tuesday. The rest of the time he spent at home working on his research and writing. Popper and Lakatos were contrasting personalities, and even in this small initial detail we can see a marked difference between them. Lakatos enjoyed coming into LSE and socialising with staff and students. If anything, Lakatos always seemed rather reluctant to go home in the evening, but then he had no devoted wife to await him in his bachelor flat. Popper, quite to the contrary, was reluctant to leave his rural home and to come in to LSE.

Because Tuesday was the only day on which Popper visited the department, there was a noticeable air of excitement each Tuesday. Popper always gave a lecture in the morning, and I attended these for two successive years. Waiting in the lecture hall for Popper to appear was not without some amusement, because a ritual was always performed before the great man entered the door. Two of Popper's research assistants would come into the room before him, open all the windows, and urge the audience on no account to smoke, while writing: NO SMOKING on the blackboard. Popper had indeed a very strong aversion to smoking. He claimed that he had a very severe allergy to tobacco smoke, so that inhaling even a very small quantity would make him seriously ill. When his research assistants had reported back that the zone was smoke-free, Popper would enter the room. (Photos of Popper)

Popper had a powerful and very deep voice. He always spoke slowly and deliberately, and his tone was nearly always serious, even solemn. He also had a very marked Austrian accent. I later learnt from Bryan Magee that Popper had a complex about his accent, and was afraid that it would make him appear ridiculous. However, he had no reason for this fear, since, at that time, an Austrian or German accent lent authority to an intellectual in Britain. I think this was because of the many talented intellectuals who had fled to Britain to escape the Nazis. The intellectual power of so many of these refugees led to the common idea that an intellectual with an Austrian or German accent must be a deeper thinker than an intellectual who spoke English with a native accent. There is no doubt that Popper's slow and serious Austrian tones carried great weight. Once again Popper's solemn manner was in striking contrast to that of the lively and vivacious Lakatos who spoke rapidly and was always making witty remarks and cracking jokes.

Popper's lectures did not constitute a lecture course in the usual sense of the term. He did not seek to expound some branch of philosophy of science in a systematic fashion from carefully prepared notes. In fact he began his lectures by saying that he thought this manner of exposition was quite wrong. He told the story of the man who attended the lectures of a professor, then married and had a son who twenty-five years later attended the lectures of the same professor. The lecture course was the same, but the professor's notes had become yellow with age. Popper stated emphatically that he wanted to avoid this kind of behaviour. He said that each week he would prepare some topic to talk about, but that he preferred the class to ask him questions which he would then try to answer. He was ready to take questions on any subject. Popper did not issue any reading list for his course either, but he did give some advice about reading. He said that, at the beginning of his time at LSE, someone had proposed that the school should offer fast reading courses to enable the students to study in a speedier and more efficient manner. He had then put forward a counter proposal that the LSE should introduce slow reading courses. This was because, although fast reading courses did indeed enable people to read faster, research had shown that their level of comprehension dropped. Popper said that of course it was legitimate to skip and skim through a paper or book to find out something about what was in it. However, one should never confuse this with the proper study of the paper or book. This could only be achieved by reading it slowly. As far as studying philosophy is concerned, I think that Popper was definitely right on this.

Popper's lecture course was naturally rather fragmented and disjointed, but it was well worth attending because of the interesting points which Popper made on a variety of topics. He did mention some things connected with his current research – for example his new model of problem solving which is to be found in his 1972 book. He also discussed the historical example of Galileo, and in particular how we can understand why Galileo made what we now regard as mistakes. He explained that Galileo had rejected the view that the tides are due to the Moon's gravitational pull, because he saw this theory as mixed up with astrology and so as irrational. A discussion of Galileo's mistaken theory of the tides is also to be found in his 1972. On one occasion, Popper discussed the relation between theoretical and applied science by giving the example of the law of conservation of energy. This law showed that engineers should not try to construct perpetual motion machines as these were impossible. On the other hand, the law also acted as a challenge to engineers to make their machines as efficient as they could in order to waste as little energy as possible. I also heard Popper make the famous pronouncement: "Induction is a myth." As far as I can remember he continued by saying something like: "and those who use the term 'induction', do not know what they are talking about." Popper's preference for answering questions, rather than giving the lecture which he had prepared, proved useful to me. At the beginning of the course, I was too timid to ask the Master a question, but, towards the end, I acquired more courage, and asked some questions about probability. Popper's answers were very useful for my research on this subject. I attended these lectures just out of interest and not for credit. So I don't know if there was any exam at the end of the course. If there was, I'm not sure what form it could have had.

When his morning lecture was finished, Popper went to his office, where he ate some sandwiches for lunch. He never had lunch in the LSE's staff common room, because he feared that it would contain some tobacco smoke and so affect him adversely. After lunch at 2pm, the great event of the department's week began – the Popper seminar. Before we come to this, though, let us pause for a moment while Popper is eating his sandwiches in his office, and examine in more detail Popper's aversion to smoking.

As I have remarked, Popper explained this aversion as owing to a strange and very serious physical allergy which he had to tobacco smoke. Even the smallest inhalation of such smoke would, so he claimed, result in his falling seriously ill. However, there were some doubts about whether his claim of a specific physical allergy was valid. I heard a story about this from someone – unfortunately I now forget who it was. My informant claimed to have got the story from Popper himself. Popper, according to this account, went to a specialist in allergies. Popper described his allergy to tobacco smoke to this expert, and asked him to find out more about it by carrying out tests. The allergy expert did all the tests, but reported back to Popper that he had found no sign of any allergy to tobacco smoke. Popper's comment on the result was: "This goes to show how backward medical science still is."

Even though Popper may have been wrong about his allergy, his belief in it was probably good for his health. He did live to the age of 92. Moreover, advances in medical science have, in a sense supported him, since, in the last fifty years or so, more and more has been discovered about the negative consequences for health of inhaling tobacco smoke, even passively. Nowadays the smoke-free zones, which Popper tried to enforce for himself, have become routine.

In general, Popper's state of health did not seem very good. Sometimes he had to stay at home rather than come in on Tuesday for his lecture and seminar. At other times when he did come in, he was suffering from some complaint. I remember him having to rest his leg on a footstool during one of his seminars because he had water on the knee. He did often look frail and ill, and I remember thinking more than once that Popper was not long for this world. But appearances were deceptive. In fact Popper lived a further 28 years after I first met him. By contrast, Lakatos who was always lively and energetic when I first knew him and who was more than 20 years younger than Popper, lived less than 8 years after our first meeting. In reality it was Lakatos, not Popper, who was not long for this world.

After this digression, we can suppose that it is 2pm and the Popper seminar is about to begin. Hacohen has this comment to make about it in his biography of Popper (2000, pp. 526-7):

"Popper ... thought that his seminar was exemplary of free criticism. An American visitor observed, in contrast, that it resembled the House Committee on Un-American Activities."

The seminar took place in a long room with a table running most of its length. At the top of table sat the speaker with Popper on his left and Lakatos on his right. Behind the speaker was a blackboard. Most of the participants at the seminar sat along the two sides of the table. However, beyond the foot of the table there was a space with some chairs, and here the more timid and lower status participants sat (graduate students and a few undergraduate students). This was where I usually sat with Colin Howson who also came along to these seminars. We could watch the action, while remaining at a safe distance!

Now in most academic seminars, the speaker will read his or her paper, or give his or her talk, for between 40 minutes and an hour, and, after he or she has finished, there will be discussion involving all the participants. The Popper seminar did not follow this customary pattern. Usually the speaker was allowed to talk for only about 5 or 10 minutes before he was interrupted by Popper. Popper would leap to his feet, saying that he wanted to make a comment, and then talk for 10 to 15 minutes. A typical intervention by Popper would have the following form. He would begin by summarising what the speaker had said so far. Then he would produce an argument against what the speaker had said, and he would usually conclude with a remark like: "Would you

agree then that this is a fatal objection to your position?” As can be imagined such an attack would often have a very disconcerting effect on the visiting speaker. It is easy to see that while, from Popper’s point of view, his seminar could be seen as a perfect example of “free criticism”, it could have seemed to the speaker, very much like a session of the committee on un-Popperian activities.

In a typical Popper seminar, Popper would talk more than anyone else, including the speaker. However, there was still some time for questions and comments from the floor. Lakatos would usually liven things up by making a witty remark. As I got to know both Popper and his philosophy better, I began to understand how Popper constructed his interventions. He would listen to the speaker with great attention and concentration, while, at the same time, comparing what the speaker was saying with his own views on the same subject. If there was a contradiction with what the speaker was saying, Popper would immediately interrupt and put forward a counter argument, since Popper always had at his finger tips arguments in favour of his own views, and against opposing views.

I have to say that I really enjoyed the Popper seminars. Far from feeling annoyed that Popper talked more than anyone else, I was pleased by this, since what Popper had to say nearly always seemed to me more interesting than what the speaker was saying. It must be remembered that I was, on Lakatos’ instructions, reading Popper’s writings at this time. The combination of studying Popper’s texts with hearing the Master expound his views at his seminar made me more and more enchanted with Popper and his philosophy. My fellow student, Colin Howson, was more sceptical and critical regarding Popper, and when he came to develop his own philosophical views they were very different from Popper’s.

Let me now list some of the qualities which I came to admire in Popper. First of all there was his amazing breadth of knowledge. A very wide range of subjects was discussed at the Popper seminar. Indeed Popper used to boast that his seminar took the whole of knowledge as its area. Whatever the subject, Popper seemed to have studied it, and know a lot about it. Next came Popper’s ability to formulate views and arguments clearly and concisely. Very often the speaker, like so many speakers, would put forward a position in a confused, obscure, and rambling fashion. Then Popper would intervene and summarise the key points in a few clear and incisive sentences. Popper was also a very creative man, and had a great facility for putting forward novel and interesting opinions on any subject which he considered. Finally, and perhaps most impressive of all, was Popper’s skill at argument. He had a remarkable ability to spot the weak points in any position, and could the plunge in the knife with a decisive counter-argument.

Occasionally a speaker was able to put Popper down, but this did not happen very often. I do remember one occasion when Lakatos was giving a paper at the seminar, and Popper was, as usual, interrupting every few minutes. Lakatos was obviously getting more and more irritated, and indeed looked close to tears. Eventually he said: “Karl, sit down and keep quiet or I will leave the room.” To my surprise, Popper did indeed sit down and never spoke again for the rest of the seminar. Such occurrences were very rare, however.

What I did not realise at the time, was that, while I was enjoying the seminars a great deal, they were giving a very negative impression to others. In retrospect it is obvious that Popper’s treatment of the visiting speakers was making him a great number of enemies, and I certainly became aware of this later when I met some of these visiting speakers in other contexts. I think that Popper’s way of behaving was partly the result of his suffering from a psychological blindness. Popper wanted to prove that he was the best philosopher in Britain. He thought he could do this by inviting the others to his seminar, and demonstrating that he could demolish

their views with superior arguments. I think he may have imagined that they would then come to recognise that he was the best and give him his rightful place at the top of the hierarchy. However, human psychology being what it is, his procedure simply made him enemies who tended to disparage his philosophical abilities and character. A visiting speaker might really have been defeated philosophically, but he or she would attribute this defeat not to Popper's skill as a philosopher but to his unfair tactics in interrupting constantly and not allowing the speaker time to develop his or her position properly. Hacoen in his 2000 shows that Popper started as a student of psychology and only after his first dissertation did he change to philosophy. In my view, this was a very wise change for Popper to make since he did not seem to have very good insights into the psychology of other people. It was partly this lack of insight and Popper's consequent way of behaving towards others which made him so many enemies within the philosophical community.

4. The Great Quarrel between Lakatos and Popper

When I first met Lakatos in the summer of 1966, there is no doubt that he was a great admirer of Popper. He advised me to read all of Popper's works as an essential prerequisite for research in philosophy of mathematics and science. At that time, Lakatos saw himself not just as a loyal follower of Popper, but even as Popper's deputy and champion. He was prepared to defend Popper's approach to philosophy of science against those philosophers of science who were then Popper's leading rivals in the field.

Lakatos' plan to act as Popper's champion and defender was carried out by his rather creative editing of the proceedings of the 1965 conference at Bedford College. The volume on the Problem of Inductive Logic could be called the Carnap volume, because Carnap, one of Popper's principal rivals, contributed a paper, and Lakatos wrote a long paper defending Popper against Carnap. The production of this volume in no way disturbed Lakatos' relations with Popper, since Lakatos always preferred Popper to Carnap. While Lakatos was working on this volume, I acted as his research assistant, and even had a desk in Lakatos' office. The Carnap volume was published in 1968, but sent off to the printers sometime late in 1967.

So late in 1967 and early in 1968, Lakatos was still a great admirer of Popper. If we move on just over 5 years to the lent term of 1973, the whole picture has completely changed. We know what Lakatos' views were at this time since he gave a course of 8 lectures at LSE on Scientific Method which were recorded and then transcribed by Sandra Mitchell with the help of Gregory Currie. These lectures were published by Matteo Motterlini in the volume he edited in 1999. In them Lakatos attacks Popper in a ruthless fashion. Lakatos still sees some merit in Popper's political philosophy, but says that there is nothing of value in Popper's philosophy of science. To quote Lakatos himself (Motterlini, 1999, pp. 89-90):

“Allegedly, Popper's three major contributions to philosophy were: (1) his falsifiability criterion – I think this is a step back from Duhem; (2) his solution to the problem of induction – where I think his is a step back from Hume ... ; and (3) his literary masterpiece “The Open Society by one of its enemies” ... what is it called? The Open Society and its Enemies. ... The Open Society is frankly a literary masterpiece: not being a political philosopher I cannot comment on its contents, but I certainly think it is a marvelous book. So, in conclusion, two-thirds of Popper's philosophical fame is based on mis-judgement.”

To this Lakatos adds a little later (Mottlerlini, 1999, p. 92):

“I think that the fact Popper’s philosophy survived for so long is a sociological mystery. Popper’s immortality is secured by this idiotic result.”

How did this enormous change in Lakatos’ attitude to Popper come about? When the Carnap volume was finished, Lakatos turned to the last volume of proceedings. This could be called the Kuhn volume since it contained a paper by Kuhn and various other papers discussing Kuhn’s position. I am sure that when Lakatos started editing this volume, his plan was that it would take the same form as the Carnap volume. That is to say, Lakatos planned to write a long paper defending Popper against Kuhn. However, Lakatos’s ideas developed in a way different from his original plan. His study of Kuhn convinced him that Kuhn was right on some points on which Popper was wrong. This is not to say that Lakatos became a convert to Kuhn’s philosophy. He remained critical of many aspects of Kuhn’s position. However, he also became critical of many aspects of Popper’s position.

The Kuhn volume was finally published in 1970 as *Criticism and the Growth of Knowledge*, edited by Imre Lakatos and Alan Musgrave. It contained a long paper by Lakatos, namely: Falsification and the Methodology of Scientific Research Programmes. However, this paper was not a defence of Popper against Kuhn, but a criticism of both Popper and Kuhn. It also developed a new position: ‘The Methodology of Scientific Research Programmes’ which was somewhat intermediate between Popper and Kuhn. I will next summarise briefly the criticisms which Lakatos made of Popper, and then give some of my reminiscences of how these criticisms lead to the great quarrel between Lakatos and Popper.

Lakatos’ criticisms of Popper were developed when Lakatos was studying Kuhn. Yet these criticisms were based not so much on Kuhn as on the Duhem thesis. In his 1970, Lakatos speaks of the Duhem-Quine thesis, but in fact Quine’s philosophy had little impact on Lakatos, while he used to study Duhem with the very greatest attention. It is not by chance that Lakatos, in one of the passages just quoted from his last lectures on method, speaks of “his falsifiability criterion – I think is a step back from Duhem ...”.

Duhem expounds his thesis as follows (1904-5, p. 187):

“In sum, the physicist can never subject an isolated hypothesis to experimental test, but only a whole group of hypotheses; when the experiment is in disagreement with his predictions, what he learns is that at least one of the hypotheses constituting this group is unacceptable and ought to be modified; but the experiment does not designate which one should be changed.”

If we accept the Duhem thesis, it would seem to be impossible to falsify an isolated hypothesis, and hence that the falsifiability criterion is unsatisfactory. The Duhem thesis poses the following further problem. If experience disagrees with a group of hypotheses, how can we know which of the hypotheses should be changed? Lakatos proposes his methodology of scientific research programmes as a solution to this problem of Duhem’s.

Lakatos developed his criticisms of Popper and his new non-Popperian account of scientific method mainly in the years 1968 and 1969, and it was during these years that the great quarrel broke out between Lakatos and Popper. It should be added that these years were Popper’s last as Professor at LSE. Popper retired in 1969 at the age of 67.

The main public forum of the quarrel was the Popper seminar, where Lakatos presented his new ideas on some occasions and Popper replied. Lakatos’ style was a harsh attack. Popper too

sometimes lost his temper, but, for the most part, his tone was more in sorrow than in anger. Characteristically Popper would claim that Lakatos had failed to understand his (Popper's) position and had distorted it by selective quotation and failing to mention some passages. I remember Popper once saying that until recently he had thought that Lakatos was one of the people who best understood his (Popper's) position, and that it was a great disappointment to learn that this was not the case. Of course, Popper also produced answers to some of Lakatos' objections. For example, I remember Popper saying that according to Lakatos Newton's theory was not falsified, but that if Mars started moving in a square instead of an ellipse, everyone would take this as having refuted Newton's theory. As a graduate student sitting at a safe distance below the end of the long table, I thoroughly enjoyed these heated and emotional exchanges, and looked forward to attending when one occurred. In retrospect, however, I think my attitude was a bit frivolous since the quarrel between Lakatos and Popper undoubtedly had a very bad effect on the academic standing of the Popperian approach to philosophy. This would anyway decline sharply from about 1975 on, and, although there were other reasons for this decline, the quarrels within the school certainly accelerated the decline.

5. Feyerabend at LSE

I now turn to the last of the three famous philosophers of science whom I met during my time as PhD student. This was Paul Feyerabend. Feyerabend was not a member of staff of the LSE's philosophy department, and so I did not get to know him as well as Lakatos and Popper. However, he did visit LSE quite frequently in the late 1960s and early 1970s. Let me first explain how this came about.

Feyerabend was at the height of his international fame at this time. As a result, he was offered well-paid professorships all over the world. Feyerabend accepted not just one of these, but several. This is how he describes the situation in his autobiography (1995, p. 127):

"In the late sixties I was still highly marketable. I received offers from London (a chair in the history and philosophy of science), Berlin (a new chair in the philosophy of science), Yale (a professorship in the philosophy of science), Auckland, New Zealand (a professorship – or was it a chair? – in the philosophy of science). I was invited to become a fellow of All Souls College, Oxford, ..."

Of these offers, and there were more, the most lucrative seems to have been the one in Berlin. Of this Feyerabend says (1995, pp. 131-2):

"In Berlin I had two secretaries, one for German, one for English and French, and fourteen assistants. ... I had a large room with an impressive desk and antique chairs, as well as an anteroom and a secretary in it."

As far as I can gather from his narrative, Feyerabend accepted 3 or 4 of these offers, and so divided the academic year between different universities. Thus he says (1995, p. 127):

"I was already spending one term in Berkeley, one in London, again a term in Berkeley, and so on. While in London I would also work in Berlin, commuting by plane once a week. In 1968 I interpolated a semester at Yale."

Feyerabend's success and fame was partly of course because of his individual achievement, but also partly because history and philosophy of science was very fashionable in those days. The

leading exponents of this approach to philosophy were at that time international stars with all the concomitant privileges. All this was to change soon, when history and philosophy of science became unfashionable, and came to be seen as a fringe discipline.

Feyerabend's professorship in London was not at the LSE, but in the department of history and philosophy of science at UCL. Much later I was to become a member of this department, when it had acquired its new name of Science and Technology Studies, but, as a graduate student, my connections were with LSE and Cambridge, and so I did not know what was going on in history and philosophy of science at UCL. I cannot therefore say what Feyerabend did at UCL. I only got to know him because he visited LSE on a regular basis.

Most visitors to the department of philosophy at LSE at that time would come to the Popper seminar, but Feyerabend did not do so. I only remember seeing him once among the audience of the Popper seminar, and I never heard him speak at the seminar. He had been a student and research assistant of Popper's in the early 1950s, and his relations with Popper had already gone through the usual cycle of friendship, followed by disagreements and estrangement. By the mid-1960s, Feyerabend seemed anxious to avoid Popper as much as possible. On the other hand, Feyerabend was very friendly with Lakatos, who was his favourite partner for philosophical discussions. So Feyerabend mainly visited LSE to see Lakatos.

As a result of this, I first met Feyerabend when I was working at my desk in Lakatos' office. Feyerabend entered the office to call on Lakatos, and I was introduced to him. Feyerabend was a well-built man with striking good looks of a blond, northern European kind. Most of his photographs do not capture his good looks. Perhaps he was not photogenic. He had been partly crippled by a bullet in the spine during the war, and walked with crutches. I learnt later that his war wound gave him a great deal of pain for the rest of his life, but I did not guess this from meeting Feyerabend himself, as he was invariably friendly and affable and always appeared to be in good spirits.

Feyerabend, like Lakatos and Popper, was an unusual character, but, while I have some rather tentative hypotheses about Lakatos' and Popper's psychologies, I have to say that Feyerabend, whom I knew less well, always remained mysterious to me. There seemed to be contradictions in Feyerabend which I could never understand. I would say from my personal acquaintance with Feyerabend that he was always kind and reasonable. In his correspondence with Lakatos, Feyerabend always comes across as kind and pleasant, and altogether lacking the harsh streak which existed in Lakatos' character and which one finds in his letters as well. Yet Feyerabend tells us in his autobiography (1995, p. 39) that, during the war, he considered joining the SS. Admittedly Feyerabend was only 14 when Hitler took over Austria, so that, subjected to Nazi propaganda as a naïve school boy, he may have become partly convinced. Still, it seems surprising that someone of a kind disposition should ever want to join the SS. Feyerabend was always very reasonable in everyday life, and yet in his philosophy became the prophet of irrationality, writing a book with the title: *Farewell to Reason*. By contrast, Lakatos who always defended rationality in his philosophy was often carried away by emotion in everyday life.

Feyerabend was particularly nice towards students, and, despite his international fame, never 'pulled rank'. I lived in a flat in London which I rented with some fellow students. On one occasion, we decided to give a party, and I invited Lakatos, who regularly invited me to parties at his flat. Lakatos said he would come, but could he bring Paul Feyerabend? Naturally I said I would be delighted if Feyerabend came along. Feyerabend appeared to be completely in his element at this student party. I remember him sitting on a sofa, and telling me that there was no reason to regard the writings of Marshall McLuhan as any less scientific than the writings of

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mainstream physicists. Marshall McLuhan was a well-known media guru of the 1960s, who became famous for the slogan: *The Medium is the Message*.

Although Feyerabend was employed by UCL, Lakatos persuaded him to give a course of lectures at LSE. Unlike Lakatos and Popper, Feyerabend prepared his lectures carefully, and delivered them in a brilliant fashion. Feyerabend was very interested in theatre, and was even offered the job of assistant to Bertold Brecht in Berlin. Feyerabend comments in his autobiography (1995, p. 73):

“I said no and stayed in Vienna. I once thought (and said so in print) that this was the biggest mistake of my life. Today I am not so sure.”

Feyerabend’s showed great interest in Galileo, and wrote extensively on him. This interest may have been partly stimulated by Brecht’s play: *The Life of Galileo*. The very dramatic style in which Feyerabend delivered his lectures must surely have been influenced by his love of the theatre. As Feyerabend was somewhat crippled by his war wound, and used a crutch, this might well have put him at a disadvantage as a lecturer; but he managed to turn this apparent disadvantage to an advantage. While lecturing he moved about with surprising agility, and gestured with his crutch for emphasis, or to point to the blackboard. He reminded me of Long John Silver in the well-known film of *Treasure Island*. His style may also have been influenced by Laurence Olivier’s film portrayal of *Richard III*.

Feyerabend naturally adopted the historical approach to philosophy, and obviously had a very strong interest in history. Since he was an avid reader, he had acquired a remarkably extensive historical knowledge. He used his great erudition to give brilliant outlines of the general character of historical periods. I remember very vividly his account of the Middle Ages. He quoted from a book giving instructions to priests on how to preach. The author advised the priest to describe in great detail the sufferings of the damned in hell. This should be continued “until at least two or three women have fainted”. At this stage, the priest could pass on to another point. Feyerabend also described the illnesses with which people were afflicted in the Middle Ages, adding that not just people but also their cattle, sheep, pigs and chickens were frequently ill.

Feyerabend’s course of lectures was not disjointed, like that of Popper, but on the contrary was unified by a main theme. This was the attempt to demonstrate that during the period of the Copernican revolution, the study of astronomy and mechanics was less rational and empirically based than the contemporary study of witchcraft. Feyerabend used as his source for the theory of witchcraft, a book called *Malleus Maleficarum* (Hammer of Wicked Women). In his 1978 (p. 92), he says: “In 1484 the Roman Catholic Church published the *Malleus Malificarum*, the outstanding textbook on witchcraft.” He adds that the *Malleus* is “superior to almost every physics, biology, chemistry textbook of today.” This last remark is very much in line with the lectures which I heard him give.

Feyerabend brought to his lectures an old leather-bound copy of the *Malleus Maleficarum* in Latin. As very few, if any, of the audience could read Latin, this put us at a disadvantage in argument, since we had to rely on Feyerabend’s translations into English. Feyerabend would characteristically proceed in something like the following way. He would read out a passage from the *Malleus Maleficarum* in Latin, translate it into English, and then explain why it was such a good example of sound empirical reasoning. He would emphasise the large amount of empirical evidence which the authors gave for their views, how they considered not just one explanation of the evidence, but several, and how they used careful reasoning to decide which of the various explanations was the correct one. After this praise for the passage from the *Malleus Maleficarum*,

he would then quote a long passage from Galileo, and point out all its defects. Galileo does not give any significant amount of empirical evidence for his views, his criticism of opposing views is weak, and contains logical errors, he relies on rhetorical devices to support his position rather than sound argument, and so on. This was all part of his general claim that the Copernicans succeeded by successful propaganda, and not because they had superior empirical and rational arguments. At times Feyerabend might have sounded like an apologist for Roman Catholicism, but I don't think he was in reality at all religious. He just liked to attack views which were held by most liberal-minded philosophers of science.

Feyerabend would emphasize how strong the empirical arguments against Copernicanism were. One such argument, which was popular at the time, was that if the Earth really rotated, then someone who jumped in the air would land some distance away. I remember Feyerabend explaining this argument in characteristically dramatic fashion by jumping in the air, and saying: "Look if the Earth really rotated, I should have landed on the other side of the room." This mode of exposition made the argument unforgettable for me.

Lakatos would often come to these lectures and sit at the back of the room. Sometimes, when Feyerabend was arguing for some particularly outrageous thesis, Lakatos would no longer be able to control himself, and would utter a protest. I remember on one occasion when Feyerabend was arguing that Aristotelian mechanics had a greater empirical content than Newtonian mechanics, because it dealt with all kinds of change such as change in colour, and not just change of place. From the back, we heard Lakatos' voice exclaiming: "Oh Paul! How can you say such a thing!"

As can be imagined, these lectures were very enjoyable indeed, but I could not help wondering whether Feyerabend really believed what he said. Feyerabend never used formal or mathematical logic, but he had a brilliant command of informal logic and argument. I suspect that he must have learnt his skills in argument, at least in part, from Popper. This skill in argument, combined with enormous historical erudition, meant that Feyerabend was always able to defend his position against objections. But did he really believe that in say 1640, the theory of witchcraft was better confirmed by empirical evidence and rational argument than Galileo's science; or that Copernicanism triumphed over the Ptolemaic theory just because its advocates were more skilful propagandists? Did he believe these things, or was he just saying them to create a stir, and make himself well-known for his outrageous views? There was something very enigmatic about Feyerabend, and I was never sure what he really did believe.

In this respect, Feyerabend was very different from Lakatos and Popper. Whatever their other faults, there was little doubt that both Lakatos and Popper said what they thought, and that they were both trying to give a correct account of science, even though each of their accounts of science had, naturally enough, its limitations and defects. But was Feyerabend really trying to give a correct account of science in his 1975 book: *Against Method?* Did he really believe that 'anything goes', and that scientific medicine should not be considered superior to the ministrations of witch doctors? Many philosophers of science have accused Feyerabend of frivolity, and of using his great intellectual talents to provide an ingenious defence of obviously absurd positions. He has also been accused of bringing history and philosophy of science into disrepute by giving what to outsiders must have seemed like a *reduction ad absurdum* of the whole approach. Certainly there was a sharp decline in the popularity of history and philosophy of science after 1975, and Feyerabend may have been partly responsible, though I am sure that there were many other causes, quite apart from Feyerabend's writings, of the change in philosophical fashion which occurred after 1975.

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My own view is that Feyerabend's general position is indeed absurd and unbelievable, but that, nonetheless, his writings have great value, not least because they are so well argued and draw on such a range of examples from the history of science. Skilful argument for an absurd position is always intellectually valuable, because it poses a problem. In this case, nearly all philosophers of science would agree that there are rational and empirical reasons for preferring science to witchcraft, but Feyerabend's writings show that there are surprising difficulties in defending this common sense position. He therefore poses to philosophers of science the problem of overcoming these difficulties.

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