Interview with Alan Musgrave

Alan, you arrived at the London School of Economics from Manchester in 1958. What led you to the LSE, and to the Philosophy Department in particular?

Sheer accident! It was the LSE because I was a rather 'Bolshy' student in my later years at school and the school's Careers Adviser thought that the LSE was the place for bolshy people. He was quite wrong because, despite its left-wing reputation, the LSE was a right-wing institution. Any institution that has more than fifty economists in it is going to be right-wing.

Is that a new social law, or the conclusion of an inductive inference? But perhaps say why you chose philosophy?

Sheer accident again! I had applied and been accepted to study Law. I had teenage visions of starring in courtroom dramas, bewigged. I then learned that to make it as a barrister in England you needed a rich father who could see you through the first five years, until you built up your clientele. I did not have a rich father. Shortly thereafter, a letter arrived from the LSE telling me that any student accepted for anything could switch to this new course, Combined Honours in Philosophy and Economics.

So you switched?

Yes. Having only dim ideas about what economics was, and no idea at all about what philosophy was.

So you had not heard of Popper before then?

Of course not. I was a working-class lad from the north of England, who passed the '11-plus' and got to Grammar School. Nobody at my Grammar School had heard of Popper, so how could I have heard of him?

When did you first meet him?

We were ushered into the presence shortly after the term began. There were six of us. I don't remember what he said. I do remember what we said—nothing! I sensed that he was regarded as the important figure in the department, but I had no idea why. I don't remember him speaking to me again while I was an undergraduate.

Did you take any courses from Popper?

Yes. I attended his two-hour lecture period each week called 'An Introduction to Logic and Scientific Method'. They were unusual. There were sixty to eighty people there, ranging from first-year undergraduates, like myself, to postgraduate students from all over the place, and always a few visiting academics. There was no syllabus—Popper just talked about what interested him in his own research at the time. There was no reading list. Occasional queries—not from me—about what might be in the up-coming examination were met with 'I do not believe in examinations'. Interruptions were welcomed and the 'lecture' turned, more often than not, into a seminar discussion between Popper and a handful of others. Despite all, I found it fascinating. I attended those lectures every year for about five years.

So who did you talk to, if not to Popper?

The person I had most to do with was my tutor, Joseph Agassi. Mind you, to begin with I did not have much to do with him, either. As instructed, I went to see him. He asked me what I had to say for myself. Nonplussed, I hesitated. 'Come back when you do have something to say for yourself', he snapped. Despite this unpromising beginning, I did eventually go back. I went back because I had discovered that other students in other departments were writing essays. I suggested to him that I might write one too. 'If you want to write an essay, write an essay', he said. So I did.

Do you remember what it was about?

Yes, as it happens I do. I thought I had made a big discovery—of course I hadn't. God's omniscience about our future actions was the problem of free-will versus determinism in theological guise. I wrote a couple of pages about this. Agassi read the beginning: 'Full of jargon—write simply'. I rewrote the essay. And rewrote it again. Finally, Agassi declared that Professor Popper had read the essay and thought it showed promise. The essay was not marked, of course. Internal assessment had not been invented! That essay was the sum-total of my work in philosophy in my first year.

So how did you do in the exams at the end of the year?

There were no exams at the end of the year. There were only exams at the end of three years, on which the entire degree depended. I had nine three-hour exams in five days, morning-afternoon, morning-afternoon, and so on. Plus an oral exam in front of twenty London philosophers.

That is quite unlike university life in this part of the world now! Who else were you involved with? Imre Lakatos was at the LSE at that time?

Lakatos arrived to replace Agassi, who went to Hong Kong. That was in my final year. Lakatos had left Hungary after the 1956 invasion

and had got to Cambridge courtesy of the Rockefeller Foundation to do a PhD under Braithwaite. Lakatos was my tutor. But I had little to do with him. He quickly discovered that I knew little mathematics or physics, and wrote me off.

Then you became a research student?

Yes. That was Lakatos's doing. After the examination results were known he asked me what I was doing next. I told him that I was going to get married and be a schoolteacher. He laughed at both ideas. He told me I should do research, so I did. I did marry, however, in 1962.

Popper supervised your PhD, I believe?

Yes. I had to go to see him at his home in Buckinghamshire. You see, Popper only came to the LSE one day a week, to give his lecture and preside over the 'Popper seminar', as everybody called it. I asked him what I should do research about. 'Would you ask me who to marry?', he replied, adding with a twinkle in his eye 'A good thesis topic, like a good wife, should give you sleepless nights'. Nonplussed, I went back to the LSE and told Lakatos what had passed between us. He replied that rather than waste my time looking for a PhD topic in philosophy, I should remedy my illiteracy in mathematics and physics. He said he would help, and installed me at a desk in his office. He also bought me the books I was to study—I still have them. He acted as my private tutor. He was writing 'Proofs and Refutations' at the time. I would pass my latest problem-solution for him to check the maths, he would pass his latest page for me to check the English. I was made to feel that my activities were just as important as his.

What did Popper, as supervisor, say about all this?

He found out about it when I had to go to see him again at the end of the first term. He asked me what I had been doing and I told him. He asked to see my work. I showed him. He then produced a report-form he had to submit to the Graduate School, wrote on it 'His preliminary work on the thesis is progressing well', and asked me to hand it in for him when I got back to London.

How long did this go on?

Two years. I read four books and solved most of the problems in them. They were very happy years. When you solve a maths or physics problem you know that you have solved it. It is not quite the same in philosophy. Even so, I was a plodder and had no mathematical intuition. I would struggle with a problem for days and confess to Lakatos that I could not do it. He would glance at it and say, for example, 'The definition of uniform convergence might help'. He was almost invariably right. Mind you,

he told me that as a child before the war, he used regularly to win maths competitions printed in Hungarian newspapers.

And after the two years?

Well, I found a problem of sorts. To begin with, it was about psychologism in logic, the view that the laws of logic are 'laws of thought', descriptions of the way people actually reason. Both John Stuart Mill and George Boole subscribed to this view. For Mill it was a weapon with which to beat his opponents. For Boole it was an embarrassment that he struggled with. I wrote about this. Then the thing broadened into a criticism of psychologistic or subjectivist views in epistemology in general. I eventually ended up with a thesis called 'Impersonal Knowledge—a Criticism of Subjectivism in Epistemology'. The title was a dig at Polanyi's *Personal Knowledge*. But I jump ahead, to 1969.

At some point before then you became Popper's research assistant?

Yes, that was in 1962. In July of that year I had married. We had fifty pounds that my wife had saved from her grant—she had just completed a degree in social work at Manchester. We honeymooned in the cheapest flat in the whole of London. A month later my wife got a job in social work, I became Popper's research assistant, and we moved into a spanking new flat for married LSE graduate students.

A lucky break! But you would not want to say that somebody up there loved you?

True! I suppose that given my fortunate life, I ought to be religious. But I am not. After a totally secular childhood, I did 'get religion' at about sixteen and actually read the Bible. That finished me off as far as religion was concerned.

Being Popper's research assistant—how was that?

Interesting. Popper was a workaholic, of course. Every day—except Tuesdays when he came to the LSE—he worked. He wrote long-hand in huge letters, casting pages to the floor. His wife picked them up, numbered them, and typed them. What did I do? I opened his voluminous mail and replied to most of it. I ferreted out stuff for him. Most important, I read his manuscripts and criticised them.

Was that at his request?

Of course. Mind you, it was hard going. My first encounter was typical. He had written something and invited me to 'correct' it. He warned me that he was old and sick, so I should not be too hard on him. With the temerity of youth, I said that a comma was misplaced and that 'As to X' should be 'As for X'. Out came the OED, Fowler, and a host of other sources.

An hour later I was stylistically vanquished. Those for whom English is a second language know, and care, more about it than the English! After a day of this, the 'sick old man' drove me to the station at 10 p.m., and I promptly fell asleep on the train, exhausted.

For how long were you Popper's research assistant?

For two years. After that I got a temporary Assistant Lectureship, renewable each year. It was an insecure position. After two or three years of insecurity, my wife became pregnant and I decided that I should seek something better. So I applied for and was offered a job in Scotland. I asked for a few days to consider and went back to London.

But you never went to Scotland?

No. Next morning I told Lakatos I was leaving. He exploded. He berated poor John Watkins for trying to ruin the department by not getting me a permanent job. Watkins replied, sensibly enough, that it was not the right time of year to consider promotions. Lakatos stormed off to see the Director. That very afternoon I was summoned to an 'interview' and offered a tenurable lectureship, which I accepted.

So you owed your LSE post to Lakatos?

Very much so. He viewed matters such as the proper times for doing things as English nonsenses. Life for him was a series of emergencies. His persuasive powers were enormous.

At around that time there was the London conference which spawned the famous *Criticism and the Growth of Knowledge,* edited by Lakatos and yourself.

Yes, that was in 1965. Lakatos got various foundations to fund the conference, and managed to persuade a galaxy of big names to participate in it—Carnap, Tarski, Kuhn, Kreisel, Mostowski, Suppes, to name but a few. I could tell many stories about that conference and its aftermath—but I doubt that we have the time for me to tell them.

Tell some anyway.

OK. But where to start? Having got the conference off the ground, Lakatos went on leave to La Jolla, California, leaving the detailed arrangements to Watkins. Watkins decided that it would be nice to have the conference at Bedford College, in Regents Park, where the participants could be housed in the student dormitory. Shortly before the conference Lakatos returned, heard of this, and exploded. The likes of Tarski and Carnap were not to be placed in the rooms of women students. Did those rooms have 'vashbasins', for example? Watkins did not know. Lakatos phoned the lady bursar of Bedford College to find out. They did not—toilet facilities were

down the corridors. 'You English! Great logician X has a weak bladder and needs to pee in the night—he needs a vashbasin in his room. Great philosopher Y has a beautiful wife—they need a vashbasin in their room'. And so forth. Thereafter the lady bursar of Bedford College for Ladies blushed whenever she saw Lakatos.

So what happened?

At the last minute Lakatos persuaded the sponsors to come up with extra money to house the big names in hotels, at the height of the tourist season. I spent a couple of days on the phone finding hotel rooms. I was then deputed to meet the big names at the airport and explain the accommodation situation to them.

Who did you meet?

Many people. Quine, for example. I apologised for the small size of my car—for he was very tall. 'Never mind the car', he replied, 'Tell me if there is to be any modal logic at this conference'. I told him that there was not and he got into the car. I met the logician Paul Bernays. I had with me on that occasion, for identification purposes, the mad Hungarian logician Kalmar. The plane was late and Kalmar was getting hysterical. Eventually Bernays emerged. They embraced. Bernays said he wanted to send a telegram telling his wife he had arrived safely. At the telegraph office he opened his case, took out an old pen and a bottle of ink, and started to fill in telegram forms, several of them. I fidgeted. Kalmar said, as this lengthy process was coming to a close, 'Don't worry, he is just standardising the formulas'. Eventually we got to Bedford College where Bernays, seeing lots of arrows pointing to where we should go, said 'Look, implication signs everywhere!'

So all the 'big-shots', as you call them, stayed in hotels?

Not all of them, no. Not Bernays. Carnap, too, was quite happy to stay in a student hall. Mind you, when Popper sought him out the next day, Carnap did tell him that although the room was fine, the bed was a bit short. (Carnap was also tall.) Popper then embarked on an unlikely research programme: find Carnap a longer bed! Later that day I was surprised to see Popper leading a procession of four people, one at each corner of a bed, shouting 'Make way for Carnap's bed!'

What were the intellectual highlights of the conference, for you?

There were several of them. There was a Carnap-Popper confrontation on inductive logic, which ran into a second, specially arranged session. I remember thinking afterwards that Carnap had won the argument. There was Kuhn's paper and its discussion. And there was Tarski's contribution to a particular session, which was his sole contribution to the conference.

So Tarski did not give a paper himself?

No. Poor Tarski had a terrible time. You see, he was a chain-smoker and Popper was allergic to tobacco smoke. So it was announced at the outset that there would be no smoking in sessions attended by Popper. Tarski spent most of the conference sitting on a sofa in a back corridor, chain-smoking and holding court with a few people who knew he was there. However, Popper was away one afternoon, so Tarski was able to attend a session.

What was that session about?

The foundations of set theory. Mostowski, the great Polish mathematical logician, gave the paper. At the end Kreisel leapt up and started arguing with him. I had understood little of the paper—I understood nothing of the discussion. Various other people tried to say something but could not. At one point Kreisel snapped 'I am talking to Mostowski' to shut somebody up. Then Tarski held up a timid hand. The Chairman turned to him in relief; Mostowski and especially Kreisel fell silent. Tarski began: baby-talk, which even I understood; more baby-talk, building on the foregoing; and so on. After ten or fifteen minutes he concluded 'And that, if I understand it, is the issue between Mostowski and Kreisel'. Tarski had understood, of course. And so had everybody in the room. I seem to recall that a few people clapped. Kreisel and Mostowski had nothing more to say, and the session ended.

That obviously made an impression on you.

Yes. It has been my experience that great minds are, judging by their sayings and writings, simple minds—in the sense that they have an ability to simplify, to strip away the complications to get to the heart of an issue. Think of Einstein, for example, who was his own best 'populariser'. Think, in contrast, of today's 'professional philosophers'. I subscribe to *Mind*. But I pick up the latest issue with trepidation—usually I cannot understand most of it. However, I suppose the philosophers have done no more than joined the club and gone professional.

Would you class Tarski as a 'great mind'?

Yes, I would.

The proceedings of that conference were published as *Criticism and the Growth of Knowledge*?

Not exactly. That book was the fourth and last volume of the proceedings. The other three were published by North-Holland, one on general philosophy of science, one on philosophy of mathematics, and one on inductive logic. The fourth volume was delayed, largely because of Kuhn and Lakatos. North-Holland got impatient and cancelled the contract. Cambridge stepped in and just published the best-seller!

The 'best-seller' did not appear until 1970. By that time you had left the LSE and moved to New Zealand. Why did you leave the LSE?

Oh, several reasons—combined with life being a chapter of accidents again! I found the student troubles at the LSE in the late sixties depressing and destructive. I found the departmental troubles at the LSE in the late sixties even more depressing and destructive. By then we had two children, lived in a two-up two-down slum dwelling in North London, and were not exactly well-off. London-life is great, provided that you can take advantage of it. By then we might as well have been in Timbuctoo as far as concerts and plays were concerned. Then a job offer came out of the blue.

Before you say something about the job offer, say something about the student troubles and the troubles in your LSE Department.

Student troubles first, then. Most of the students wanted what all students want—better teaching, better facilities, representation on appropriate committees, and so on. There were genuine grievances. The radicals exploited these legitimate wants and grievances to further their objectives. What the radicals wanted was to convert the LSE from a bourgeois institution of 'objective inquiry' into a centre for the worker-student revolution. The LSE was targeted by the international student revolutionaries—no doubt about that. We had an application for PhD study from an American, Paul Hoch. He already had a doctorate from the States, so we turned him down. He showed up anyway—with a loud-hailer in a sit-in. One of my own ex-students, David Bouvier, showed up too. He had, he told me, been involved with Black Power in California and had been run out of the USA because of a murder investigation. At one meeting this guy jumped up, wielding a bicycle chain, and shouted that he was off to steal the 'secret files' from the Director's office. The 'secret files' were supposed to detail the LSE's secret investments in South Africa—which did not exist. It is hard to believe now, but the radicals, heavily influenced by the events in Paris in 1968, really did think that the 'worker/student revolution' was nigh.

At the other end of the political spectrum Lakatos was also politically active, as his 'Letter to the Director of the LSE' in his *Philosophical Papers* shows. But what of the troubles in the Department?

The difficulties were between Popper and Lakatos. Lakatos had criticised Popper's view that Newton's or Einstein's theory was falsifiable whereas Freud's or Marx's theory was not. I had, by the way, already hinted that things might not be so simple myself. I prepared the index to Popper's *Conjectures and Refutations* when I was his research assistant. The index contained a joke—not a very good joke, I admit, but what do you expect from an index? The entry for 'Marxism' contains as sub-headings 'Marxism made

irrefutable' and 'Marxism refuted'. Popper liked that, by the way—but that was long before Lakatos criticised him.

What do you think of the Popper–Lakatos dispute?

Well, at the time it generated an enormous amount of attention—and heat. I have since come to view it as a 'storm in an inkwell' (to use John Watkins' phrase). Popper knew that Newton's laws, by themselves, do not rule out any observable state of affairs, that you need to add 'initial conditions' or 'auxiliary hypotheses' to them. Lakatos's 'methodology of scientific research programmes' simply exploits this fact. Mind you, as an amateur historian of science I have found Lakatos's so-called 'Methodology of Scientific Research Programmes' a useful framework for approaching historical episodes in science. Which is not to say that I accept all of Lakatos's rather extreme claims.

Finally, how did you come to move to New Zealand?

Well, Otago University was looking for a new Professor of Philosophy. The Vice-Chancellor was in England and rang Popper to see if he knew of anybody. You see, Popper had been in New Zealand, at Canterbury College as it was known then, from 1937 to 1945 and had been incredibly influential. The Vice-Chancellor and several other important people at Otago had been students of Popper. So it was natural for them to approach him.

And Popper suggested you?

Yes. And I applied for the Chair. A few weeks later the phone rang and I was asked to go for an interview the following week. I had never been outside the UK and I had never been on a plane. So my first plane trip was just about the longest trip that there is. I was interviewed by about twenty people—I had expected two or three. To my great surprise I was offered the job. I was 29. My qualifications at the time would not get me an Assistant Lectureship in New Zealand these days. But times were different then. My income doubled. By the time I took up the job, in 1970, my income had trebled. You see what I mean about a fortunate life!

You had some distinguished predecessors in the Chair at Otago.

Yes, though I did not know it at the time. Raphael, Findlay, Passmore and Mackie had all been there.

What about your own work since you came to New Zealand?

Well, at first I published some papers in what David Stove calls the POP-LAK-KUHN-ABEND disputes. I suppose I am regarded as a 'Popperian'—though precisely what that comes to is not easy to say—it certainly does not come to the same thing for all of the self-styled 'Popperians'.

What do you mean?

Well, I have come to see that the situation with Popper's philosophy is very peculiar. There are these twelve (or is it twenty?) folk who think it is the bees-knees. These are the self-styled 'Popperians'. They talk to one another, they write for one another. The rest of the philosophical world takes no notice—and they take no notice of the rest of the philosophical world. They have even formed a club, pretentiously called 'Friends of the Open Society', and they publish a newsletter. I do not belong to the club, and I stopped getting the newsletter when they started charging money for it. Again, Popper's views about science are extremely popular with working scientists—but most philosophers of science think them fatally flawed.

Where do you stand on that?

The matter turns on Popper's claim that he has solved the problem of induction. Most philosophers reject this claim. Most philosophers think that Popper must smuggle induction in somehow if he is to have any convincing answer to Hume. This has been urged, one way or another, by dozens of his critics. I disagree. I think Popper does have a distinctive answer to Hume which does not smuggle induction in. (Whether that answer is defective on other grounds is another matter, of course.) But the self-styled Popperians reject my reading of Popper on induction and Popper himself has never endorsed it. As I say somewhere, I sometimes think I am the only person who understands Popper's answer to Hume, including Popper himself. But perhaps I should be cautious and say this: either Popper answers Hume in the way I think or he has no answer and his numerous critics on the point are right.

Where have you written about these things?

Oh, there are a couple of papers in inaccessible places. It also finds its way into my book *Common Sense, Science and Scepticism*, which was published last year.

Tell me about that book.

Well, its theme is that common sense and science and scepticism go hand-in-hand, and stand opposed to what the ancient sceptics called 'dogmatism' (the pursuit of certainty) and to idealistic views. I wrote it before I read David Stove's paper 'Idealism—a Victorian Horror Story' in his book *The Plato Cult and Other Philosophical Follies*. It would be a better book if it had been written after I read Stove. I only half-knew what Stove makes clear in his paper—that most good philosophers in the past three or four hundred years have been idealists of one kind or another. As an undergraduate I had been both fascinated and repelled by Berkeley's idealism. All that ingenuity wasted on a crazy view. Even today philosophers take it more seriously than they should.

Why do you say that?

My book was reviewed in *Mind* (April 1994). The reviewer talks of its 'egregious errors' and quotes two of these. The first is my statement that for Berkeley '[external] objects themselves do not exist at all'. According to the reviewer Berkeley held 'that the objects do exist, but that they consist merely of ideas'. This is, of course, what Berkeley tried to convince us of when he posed as a defender of common sense—a matter I later discuss in the book. Why are we still making excuses for Berkeley? The eighteenth century was not fooled, and neither should we be fooled. The reviewer goes on to say that 'the things I see are external to my mind even for Berkeley'. That is just, to coin a phrase, an 'egregious error'.

So you agree with Stove that most philosophers are idealists?

Yes I do, and profoundly so. Stove thinks philosophers are attracted to idealism by quasi-religious motives, the desire to have a universe that is congenial to us. No doubt there is much truth in that. But in my book I detect a second, more philosophical motive—the desire to vanquish the sceptic. This has been a chief motive for idealist views. Both motives, the religious and the anti-sceptical, are quite explicit in Berkeley. Which leads me to a disagreement with Stove.

What is that?

Stove says idealism died out in the 1940s. I disagree. Idealism is going strong among the luminaries of Western philosophy as we speak. For what is the dominant metaphysic of our age? It is a scarcely intelligible constructivism or conceptual idealism. You know what I mean, for you have written about it. 'There is no ready-made world', and all that guff. Good philosophers say, in all seriousness, that we can make stars with words, that the theoretical entities of science are the products of our talk, that there are no verification-transcendent truth conditions, that truth is by definition what 'ideal science' pursued to its 'limit' teaches, and so on. It is Kant generalised and relativised.

And it is connected with scepticism, you think.

Oh, most definitely. If there are verification-transcendent truth conditions then we will never know whether statements with those truth conditions are true or false. That ideal science pursued to its limit might be false is a typical sceptical scenario. There are more familiar ones, of course—Descartes' Evil Genius, Putnam's Brains-in-a-Vat. (By the way, I always ask my students what is the difference between Descartes' Evil Genius and Berkeley's Benevolent God. A nice question, that!) Philosophers are still obsessed with scepticism, despite protestations that they are not certainty-freaks any more, but fallibilists all. Rather than trying to beat the sceptics, we should join them.

So you are a sceptic?

Yes, in the sense that I think we might be being fooled by Descartes' Evil Genius (or Berkeley's Benevolent God), we might be disembodied brains in a vat of nutrient, the world might have popped into existence five seconds ago complete with people with memories and fossils in the rocks, 'ideal science' pursued to its 'limit' (whatever that means) might be false, and so on. All these things might be true. Of course, the sceptic gives no reason for thinking that any of them are true. Nor, for the purpose of discomfitting the certainty-freak, need she.

Alan, what are you working on at the moment?

On a book about scientific realism. Most of my recent papers have been, one way or another, defences of scientific realism against its many critics. I am trying to draw them all together into a connected piece. Philosophic idealism and anti-realism about science go hand-in-hand. For philosophic idealism contradicts everything that science, taken at face value or realistically, teaches us about the world. Making stars with words! Genes or black holes the products of our talk! Most of contemporary philosophy, being idealistic, is pre-Darwinian.

How so?

Well, think how badly non-human animals are treated in most of Western philosophy. For Descartes they are machines lacking souls and they do not go to Heaven like we do. For Berkeley they are 'sensible things' and they are composed of ideas put into our minds by God. For Kant they are 'phenomenal things', partly constituted by our concepts (noumenal cats and dogs exist, too, but we can know nothing of them). Nowadays philosophers think dogs and cats are made by our words or concepts. Darwin, on the other hand, thought there were non-human animals before there were any humans armed with words or concepts or whatever.

So you think that science and philosophy are in conflict?

Science and idealist or anti-realist philosophy are. Philosophers have been promising a 'scientific philosophy' for centuries. We are no nearer to it now than we were then. We will get nearer to it if we leave all forms of anti-realism behind us.

What is your new book called?

I don't know. A colleague of mine suggested 'Summa Contra Omnes—An Anti-realist Bestiary'. But that is a bit pretentious.

[Note: this interview was completed before the death of Sir Karl Popper on 17 September 1994 at the age of 92.]

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