

Simon Schaffer

Historian of science. Born 1955.

Life story interview by Alan Macfarlane.
Available online at www.livesretold.co.uk

Contents

1. My Parents
2. Early Life in Australia
3. Back to England
4. Trinity College Cambridge
5. Harvard
6. PhD at Cambridge
7. Paris 1979
8. Imperial College
9. Back to Cambridge
10. Broadcasting
11. Museums and Exhibitions
12. Being Jewish
13. Reflections

The text of this life story is transcribed, with thanks and acknowledgement, from the collection of Filmed Interviews with Leading Thinkers at the Museum of Archaeology and Anthropology at the University of Cambridge. The interview was carried out by Prof. Alan Macfarlane on 27th June 2008, and was transcribed by Sarah Harrison. The video is here: <https://www.sms.cam.ac.uk/media/1130259>

1. My Parents

I only knew my mother's parents; both my father's parents died while we were in Australia. He had two sisters who were my closest relatives when we returned and were very important to me. They were solid, London, lower middle class, Jewish, originally from Lithuania, arriving about 1905. I was told very little about this; there had been family in the Netherlands, most of whom were murdered by the Nazis.

There was an extended family in North London who looked after us when we came back. My sense was that my father was treated as the delightful, eccentric intellectual, which every Jewish family should produce. My mother's father was a shoe salesman from South London; an adorable, benevolent man. My grandmother was also delightful; they had been evacuated to Torquay which is where my mother grew up and went to Torquay Grammar School for Girls; they retired to Bournemouth.

My parents are the most important people in my life. My father, who died in 1984, was very careful not to actively direct me yet he always provided any resources that I needed. I was a ridiculously spoilt child; the assumption was the good life consisted of music, books, long walks, and serious conversation. I was always treated like a grownup. My mother treats everybody in the same way and I think I learnt from that deeply principled egalitarianisms. Snobbism was loathed by my parents. I liked my teachers and they were very good at their job. I remember them all pretty well but they were anything but inspirational. It was a rigid, robust curriculum at primary school.

2. Early Life in Australia

I was born in Southampton in 1955; the first and only child. In less than ten months we had left for Australia. Father was teaching in Political Science at Southampton University and my mother was working as a trainee librarian. We got an assisted passage, passing through the Suez Canal just before the Suez crisis. Father had got a post in Political Science at the University of Queensland, Brisbane.



The Suez Canal.

I spent the next decade there; my mother became a librarian at the University and my father, a Professor in Political Science and the nascent discipline of Development Studies. My father did a lot of work in New Guinea. I remember my father coming back with objects, maps and pictures of an extraordinary other world. Papua New Guinea was administered by Australia in this period.

I remember cricket, swimming and Christmas pudding. My parents built their own house on the outskirts of Brisbane. They grew papyrus in the septic tank and I remember trying to make paper. It was a halcyon period for a child; but complete blindness to Aboriginal society. There was a Salvation Army school next to my primary school with some Aboriginal children who came to our school, but very tightly controlled. Travel anywhere was restricted at that time.

When we travelled back to England it was by boat again, just before the Six Day War. We couldn't land in Aden but were able to spend time in Egypt and to see the pyramids. We went to Sydney by train which changed gauge between Queensland and New South Wales.

It was a blissful childhood except for mosquitoes. Primary education was entirely nineteenth century - copperplate handwriting, mental arithmetic, colonial history with Australian history beginning with the arrival of the first fleet in 1788. The school was divided in to houses with each house named after an heroic Australian explorer. There was a Thursday night radio broadcast 'Window on the West', reporting the Anglo-American world; a Commonwealth world view.

3. Back to England

I came back in 1965 to Britain to a very different school. The Robbins Report and the expansion of British universities led to the return of a remarkable cohort of expatriates. One new university was Sussex which set up an Institute of Development Studies and offered a job to my father.



University of Sussex.

We moved to Brighton and my parents stayed there from then on. It was unrecognisably different from Brisbane starting with the fact that the beach had no sand. We lived in a rather chilly house opposite the secondary school. I came back in summer 1965 when I was ten when there was still the 11+ regime. I wondered whether I should go to primary school and take the exam or go directly to secondary school. I was taken to an educational psychologist and sent straight to Varndean Grammar School for Boys.



Varndean Grammar School, Brighton.

It was a very good school, ridiculously selective, single-sex, school uniform, prefects, houses, gowns for teachers, Latin. I realize that the teaching was of a very high order. As we lived opposite the school I could go home for lunch. I accelerated through the school and missed out the fifth form. I took 'O' levels at fourteen and 'A' levels at sixteen.

Several of the teachers were inspirational, especially the teacher of mathematics, Ron Woolner, a descendant of the artist Thomas Woolner, who absolutely inspired me. I thought myself very bad at arithmetic and he sat me down one day and said that mathematics was not arithmetic. The English teacher, Mr Cunningham, was also very good, letting people work at their own pace, spotting people who could read quite fast. He encouraged us to start our own drama club independent of the school drama club. Alongside the usual school play, normally Shakespeare, we could do our own production of modern playwrights, eg. John Arden. All this happening in an institution out of time, in late 1960's Brighton. With my father's rather special and enthusiastic support I did science 'A' levels; he was delighted as an unquestioning admirer of the sciences.

I did play hockey for the school until I got a hockey stick between the eyes which damaged my confidence. Also, with my Australian background, I did a lot of swimming. I played chess and the flute. Music has meant a lot to me emotionally, but also as a way of getting together with others, I also found it interesting to think with - a source of metaphor, collaboration, collective playing, to follow a score, think about how composition works - things to reflect on when thinking about creativity and discovery. I listen to music all the time when writing or reading. I find silence very disturbing; my taste is eclectic; now find I favour the autobiographical which remind me of my past; jazz and the classics that my parents liked.

As a student I liked early music but have gone back to my father's loves - Mahler etc.. Writing is the moment that I have ideas, so while I am at a keyboard, listening to music, rather than in quiet repose, the auditory is blanking out other sound but giving something rhythmic for concentrated reflection. It is not uncommon for people to use that sort of environment both as a resource and as insulation. I have very few flashes of insight but rely on 99% perspiration. My technique when writing is that I never perfectly finish a piece in the evening as the next morning re-starting will be very difficult. I remember that as a science undergraduate it was much the same; I am obsessive re-writer and a very bad cutter.

I was never a scientist at a high level; I suspect that processes of creativity don't respect disciplinary boundaries; there are things that one can generally say about how people create which are not significantly different between making a poem and making a theory or experiment. There are some systematic and institutional differences, ones to do with the tools of the trade. Now, at a superficial level, all use computers, despite disciplines marking out increasingly separate spheres,

There is the military entertainment complex where skills at pattern recognition on a screen with a keyboard move from Gameboy to nuclear war, Now the manual, practical, work that people did when being creative have converged; ironically, reflection on creativity across disciplinary boundaries might have become easier because one is dealing with bits of practical behaviour which have got more similar over the last fifty to sixty years. There ought to be robust principles to deal with what it is like to have smart new thoughts that work when you are a "typist at the cinema", the standard situation of almost all workers and intellectuals now.

If you were thinking of applying to Oxbridge you stayed on for an extra term. In Autumn 1971 went into the "upper bench" and did the entrance exam in December. I got an Exhibition to Trinity, Cambridge; and had from then until October 1972 to do what I wanted.

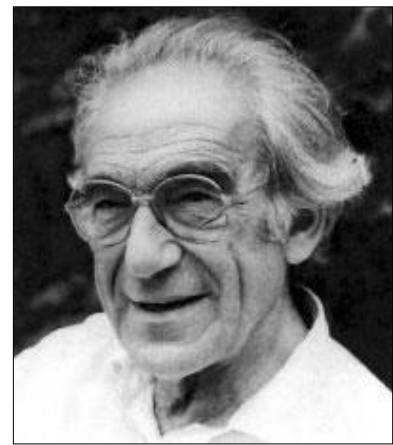
The first half of 1972 very important for me as learnt that I could survive on my own. I became disillusioned with various forms of political Zionism; I did a lot of archaeological work on a major dig under the south-west wall of the old city of Jerusalem. It is a very significant place historically and politically. The level they had reached was Byzantine when I was there. The meticulousness of an archaeological dig is very interesting. I found a coin and a piece of glass and learnt a lot about patience, attention, and reading the signs.

4. Trinity College Cambridge

I came back to England in the Summer after youth hostelling and then came up to Trinity with an Open Exhibition to read natural sciences. Trinity was an amazing experience as an undergraduate; still single-sex, enormously supportive, though pretty stern with no central heating and cellars flooding regularly.

The pattern of training was rather nineteenth-century for the tripos; the bulk of significant teaching took place within the college, a collegiate rather than university experience. J.J. Thomson had been terrified of the effects of the big science labs on college life, but thirty years ago Trinity controlled all the teaching of its students. It was not until the Spring of 1974 that my centre of gravity began to switch from college to the Department of History and Philosophy of Science.

At that stage the Department was not in the Whipple building but in a series of buildings on Lensfield Road. These tiny seminar rooms became rather inspirational for me, partly because of the teaching. Gerd Buchdahl was significant for the way he taught and his egalitarian humility. He was a classic central European Jewish intellectual who had been interned in Australia during the Second World War. In the prison camp he started a reading group in philosophy of science. After the war he got a post at Melbourne University and more or less single-handedly started the first department of history and philosophy of science.



His central area of interest was critical philosophy of Kant. He came to Cambridge in the 1950's more or less with the express function of setting up a programme within Cambridge like the one in Melbourne. He hired Mary Hesse and they forged the Department as it is now. By the time I met him he was already a grand, stately, hilarious man who almost completely failed to communicate the content of what he was teaching but succeeded in communicating its importance. He lectured on Locke. Martin Rudwick also taught a course on nineteenth-century earth sciences.

In the summer of 1974 I noticed that the National Maritime Museum offered internships for two months, to work in Greenwich on some aspect of the collection. That was the first time I worked behind the scenes at a museum and thought about scientific hardware. The collection was catalogued on index cards. I was given a Polaroid camera, and two dozen sextants, for example. I had to take them to bits, photographing each bit, pasting the photo onto a card, writing a description for each bit, then putting the instruments back together again. It was revelatory for me that you could learn things from this hardware, that minute differences matter. I was working

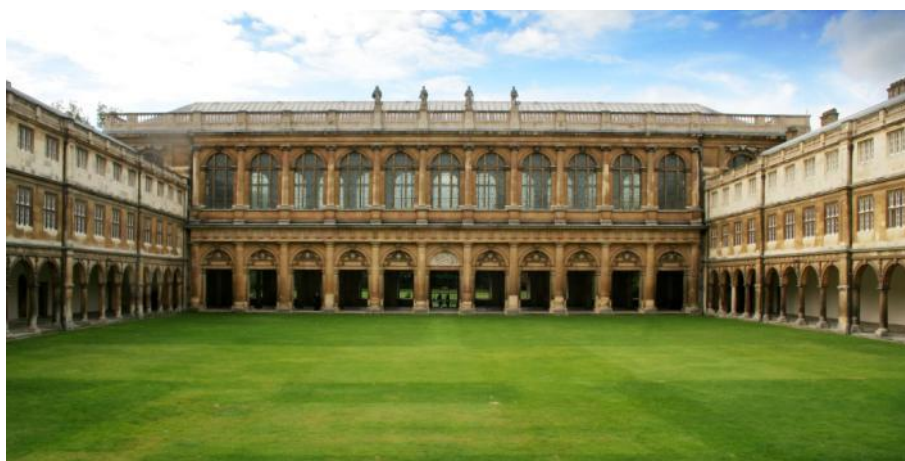


with a small group of curators and assistants who were world experts - Commander Waters and Alan Stimson. I found material for a dissertation topic for my final year.



The Old Cavendish Laboratory.

By then I was utterly committed to doing history and philosophy of science in Part II; an agonizing decision as I really wanted to stay with physics. Mine was the last year that studied physics in the Old Cavendish as in 1975 the scientists moved to West Cambridge. I was more interested in the archaeology of what was left on Free School Lane, especially as it was now where History and Philosophy of Science was, than by what would happen in West Cambridge. It was made very clear to me that I was giving up the possibility of research in science.



Neville's Court, with the Wren Library, Trinity College Cambridge.

I had a splendid time in my final year with a lovely room in Neville's Court, and learnt the wonders of the University Library. The dissertation topic proved to be viable and interesting.

At this time my father was on secondment to the Shannon Free Airport Development Corporation as an advisor on ways of improving development in the west of Ireland. I was involved in my parents moving to a wonderful cottage in a village called Corofin in Clare, about fifteen miles from Shannon. In the vacation I

went there and wrote my dissertation. Our landlord, Ignatius Cleary, worked to create a museum in a disused church. My parents helped and I wrote labels.



Corofin, County Clare.

In the first half of 1975 I couldn't quite make up my mind what to do. A strength of Cambridge for me was that one could change courses. In my last two terms I applied for everything, including the Foreign Office. The most amusing aspect of that was being vetted by a retired policeman from Hove. At the time there was thought in Whitehall that there were not enough people with science degrees working in the Civil Service. There was some discussion of me going on to study a "hard" language, meaning Arabic or Chinese; I was very clear that if I took that route I would study Chinese.

5. Harvard

In the end I was offered a Kennedy Scholarship to go to Harvard for a year, and did that because Harvard has the world class history of science programme. It taught a thorough, systematic, Masters programme which Cambridge did not and many eminent scholars were working there. This confirmed me in this discipline and convinced me that I wanted to work on early modern natural philosophy - Newtonianism etc..

Bernard Cohen (right) was one of the eminent scholars at Harvard. The programme balanced intense seminar teaching with directed reading courses. I did the latter with Franklin Ford. I reflected on staying but was working on Isaac Newton and it seemed silly to stay there so came back to Cambridge. I was there at nineteen and twenty and enjoyed it (I had come to Trinity at seventeen). It was a year of maturing and of travel. It was a country in crisis after Watergate and Vietnam. I was impressed and excited by hybridity, pluralism and optimism, and depressed by introversion and the deeply ill-informed attitude to the rest of the world.



I had actually been to the States when my father was on sabbatical at Cornell in 1967 which was also a crisis-ridden period with Cornell being a centre of anti-war feeling. I had been at Dewitt Junior High in Ithaca and was taught how well the green berets were doing in Vietnam and what to do if there was a nuclear attack.

Educationally the most important thing about the Harvard programme was the course offered by Everett Mendelsohn (right). He was deeply active politically on the liberal left, a major peace campaigner. He was teaching a course on the sociology of science. The course was oriented partly on classics of sociology - Talcott Parsons and Robert Merton - explaining sociologically the asocial quality of the sciences. Another part of the course was on alternative ideologies in the sciences - Schumacher etc.. Mendelsohn encouraged his students to do whatever they wanted and resourced that as far as possible.



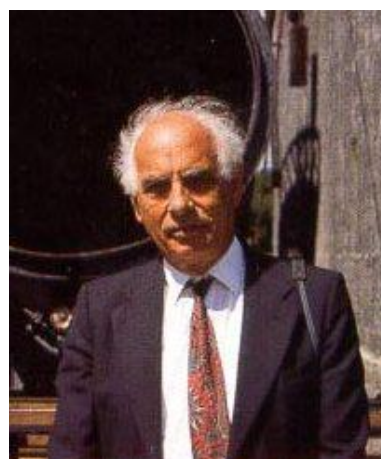
It was an interesting time for micro and macro controversies about the sciences around Harvard. David Baltimore was doing research on recombinant DNA and there was the polywater affair. I learnt how to do oral history by interviewing scientists on the latter. I wrote up the results and sent it off to a new journal, 'Science Studies', edited in Edinburgh by David Edge. It was rejected but the referee, Harry Collins at Bath University, wrote pages of comments which I learnt a lot from.

6. PhD at Cambridge

Aged 21 I was back in Cambridge at Trinity who had been kind enough to give me a graduate scholarship. My supervisor, Michael Hoskin, convinced me to write on Newton; he pointed me to David Castillejo's typescript in the University Library. He had been the first scholar to look at Newton's theological papers.

David Brewster, the greatest of Newton's biographers, had access to all Newton's papers, then in the hands of the Portsmouth family, the bulk of which were alchemical and theological. As an evangelical Christian he both ignored and explained away this material. Brewster had transcribed some of this material so it was in the public domain. In the 1890's the whole collection was offered to Cambridge University, which took the mathematical and natural philosophy papers but returned the rest to the family. These were auctioned in 1936. Maynard Keynes bought some that are in King's and the rest ultimately went to the Hebrew University in Jerusalem.

Castillejo was the first to tackle the subject and his typescript seemed a good start for a Ph.D. I worked on this between 1976-1979 and produced a thesis: "Newton's Cosmology and the Steady State". While I was doing that I was also doing other smaller bits of research. Michael Hoskin (right) had founded the 'Journal of the History of Astronomy' in 1970; Tom Whiteside had published in the very first volume a piece on Newton's path to the 'Principia' and was then editing Newton's mathematical papers. I was not equipped to learn as much as I should have done from him, and he was not the easiest person to get along with, but he was a towering pillar of scholarship. In 1977 at a conference at Churchill College to mark the anniversary of Newton's death, he was presented with the Sarton Medal.



I was honoured to be asked to give a paper which was based on my thesis work. I got interested in issues of cosmology and cosmological development and found and transcribed a couple of papers by Edmund Halley, which were published in 'Notes and Records of the Royal Society'. At the same time William Herschel's manuscripts were available in Churchill College Library and I used them to work on Herschel's astronomical and cosmological beliefs which were also published.

The most important person that I met in my field in the late 1970's was Roy Porter. He was the only member of the Cambridge History Faculty remotely interested in what the historians of science were doing. He was a professionally trained historian from Christ's; he had been close to Martin Rudwick and his Ph.D. thesis was on eighteenth-century earth history. He showed us how you could be an historian of the sciences which was not obvious at that time.

The history of science begins in Cambridge in the 1930's through the work of Joseph Needham, Ernest Rutherford, Arthur Eddington etc., all scientists, who started the Cambridge Committee for the History of the Sciences. After the war, members of the Communist Party dominated discourse on the past of the sciences in Britain.

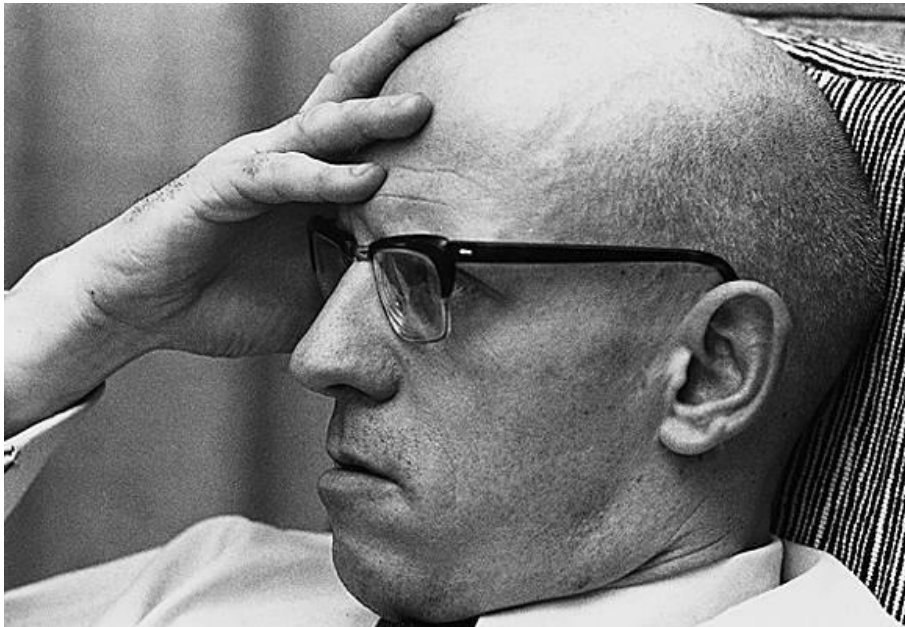
Herbert Butterfield responded by saying that the past of science was too important to be left to scientists, especially if they are historical materialists. There was a series of famous disputes which resulted in left wing scientists writing materialist history of the sciences and conservative, idealist historians writing intellectual history of scientific knowledge. By the 1970's idealism and intellectualism are firmly ensconced within the Cambridge programme of history and philosophy of science and Roy Porter (right) brought reasonably judicious social history back into the picture. Roy got me to give a couple of talks to his final year special paper on eighteenth-century sciences, also to review material for a journal he was then editing, 'History of Science'.



7. Paris 1979

In 1979-1980 I went to France. I was just finishing my thesis and was encouraged by my parents to spend time in Paris. I was given a Thank-offering to Britain Fund award, a Norwegian award, paid through the British Academy. It enabled me to spend a few months at the Centre Koyré, a section of École des Hautes Etudes, a centre for the study of science. It was then in rue Colbert next to the Bibliothèque Nationale in a romantic building. I was encouraged to wander the Paris archives.

I also went to the College de France every Wednesday morning to Foucault's lectures on the practice of confession. The lecture audience consisted of a small layer of grande dames in furs, a largish group of mainly East Asian students with portable tape recorders, and people like me at the back. As a lecturer he was quiet, high-pitched, very charismatic, allusive and rapid. He spoke off the text and his breadth of knowledge was remarkable.



Michel Foucault.

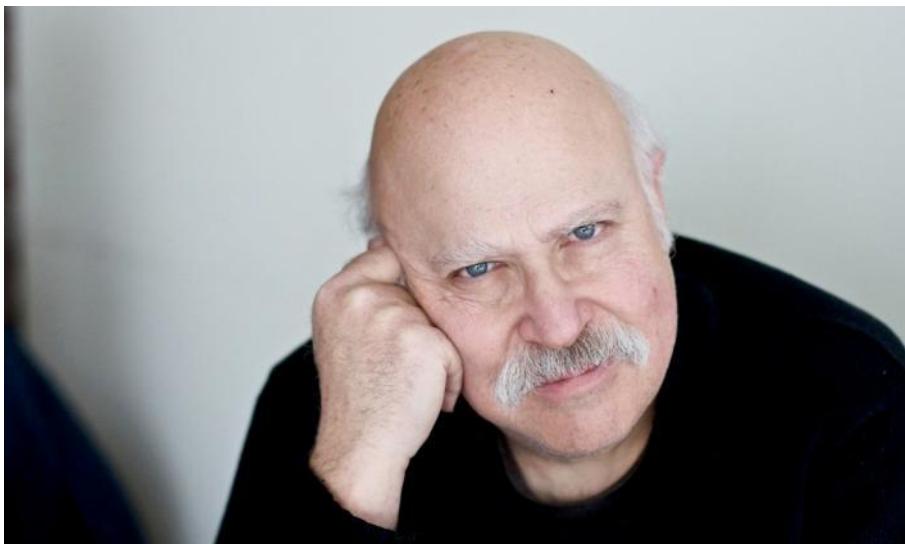
Being in Paris was rewarding and I was able to encounter a lot of French philosophical and epistemological writing which was simultaneously Martian and indispensable. A good example was my work on Herschel, a Hanoverian musician who came to Britain to practice as an organist and choirmaster, and eventually moved to Bath in the 1770's. He joined a local philosophical society and began to study natural history. He got his own microscope and was fascinated by coral.

He also taught himself how to build telescopes, building increasingly large and strange mirror telescopes. He set out to survey the heavens. The result was the discovery of Uranus in 1781 which brought him international fame. The King gave him a place at Slough where for the next thirty years he ran a detailed survey of just those parts of the sky astronomers were not interested in.

That was my puzzle. By late eighteenth-century standards he was not an astronomer so what was he? He said he was doing the natural history of the heavens; he was

applying Linnaean methods to stars, to classify stars into species, observing their growth cycle. Herschel gives us the notion of light year and that when looking into space one is looking deep into the past.

Foucault's work seemed really useful as a tool for describing that; I wrote an article 'Herschel in Bedlam' which is half Foucault and half Herschel. I sent it to the 'British Journal of the History of Science' and they published it. It is full of my Paris experience. I gave it as a paper at Bath in March 1980, at the first joint meeting of the British Sociological Association and the British Society for the History of Science.



Steven Shapin.

The meeting brought together a new generation of sociologists of scientific knowledge and historians of science. The paper went well and I met Bruno Latour. The paper that impressed me the most was given by Steven Shapin on the dispute between Leibniz and Samuel Clark. The final chapter of my thesis was on the same thing.

Shapin was much better at analysing what was at stake in that metaphysical-philosophical dispute showing the political issues involved in these different cosmologies. It footnoted anthropologists whom I had never read like Mary Douglas. When it came to choosing examiners it seemed obvious that the internal should be Roy Porter and the external, Steven Shapin.

On a cold, rainy day at U.C.L. I had my viva, and passed; since then Steven Shapin has become indispensable for my work and life. Collaborative work is hard but he makes it easy. His writing (as shown in the Bath paper) is witty, profound and without jargon, with a deep empiricism.

He is respectful of genuine expertise and delighted by craftsmanship. He moved from a liberal arts college, Reed, to Penn, Philadelphia, one of the headquarters of the social studies of the sciences. He did his thesis on the social world of science in the Edinburgh enlightenment. Tom Whiteside got him a job initially at Keele University, to catalogue their library of mathematics books. Then he went to Edinburgh. He is a charismatic, generous person; he did important work on the

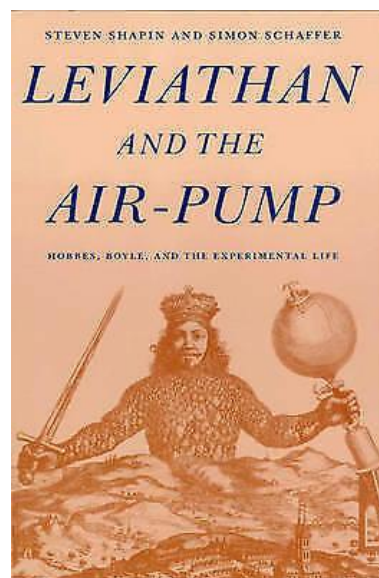
controversy between Boyle and Hobbes, with notable use of anthropology, particularly of Evans-Pritchard.



William Herschel and his sister and collaborator Caroline polishing a telescope lens, probably a mirror. 1896 lithograph.

8. Imperial College

In 1980 I got a job in London at Imperial College. Living in Paddington, I could walk to work; it was a wonderful time. Between 1981-1983 Steve and I wrote a book. As he was in Edinburgh and I was in London we typed letters, cut them up and pasted them together and sent them back; every so often I would go and stay with him. It was an intensely cloistered project and we barely discussed it with anybody else or gave lectures or seminars about it. The only grant we applied for was a tiny grant from the Royal Society to help us make sure we had covered the extant air-pumps. If the book ('Leviathan and the Air-Pump') has virtues one of them is that it was a back stage project yet intensely conversational. An experiment in re-thinking an event in the history of science. I think the book fell still-born from the press and almost all the initial reviews were uninterested or hostile. There were only two exceptions, by Harry Collins and Owen Hannaway.



It changed life for me as it helped me get a job back in Cambridge in 1984. Much later, in 2005, it won the Erasmus Prize. The book is often cited; it is Steve's book, I helped. The argument that the book makes is that really important innovation happens in the course of detailed, meticulous labour. There are metaphysical questions about the repetition of experiments. Subsequently both Steve and I have done work on similar questions. For me, that work gave me a certain confidence in the kinds of questions I was trying to ask.

9. Back to Cambridge

When I was appointed to a lectureship in Cambridge, probably on the basis of that book in press, I was actually replacing Gerd Buchdahl, though not replacing him with any of the things he did. At the same time my father died of a heart attack on 10th May 1984 and I have never recovered from that. I still have the experience of pointing things out to him.

On the other hand it has clearly brought myself and my mother Sheila (right) much closer. She threw herself into politics in Brighton from the mid 1980's onward as part of the struggle against Thatcherism. She became a Labour councillor, an active militant in the anti-nuclear and the green movements, and taught me a lot. I came back to Cambridge and could see what my career was going to be.



I think the main theme of my professional life has been working out the consequences of the idea that communicating knowledge is also making it. There is a common idea that the scholar is working out new knowledge, truths, and that somewhere else this will be communicated. My experience is that that is the wrong picture and that almost all my good ideas have happened while communicating, deliberating, etc..

Working with students and with teams planning museum exhibitions, and broadcasters, has provided me with places where there are opportunities to find out more. I have spent the last twenty-five years doing television programmes on the history of science. I began when working in London with Channel 4. Bob Young had persuaded Central Television, as it then was, to run a programme on science; it was called 'Crucible' and Bob Young brought in colleagues to each make a programme. Donna Haraway made an extraordinary programme on primatology, on how people studied apes.

I was asked to make a programme about Newton, called 'Portraits of Newton'. It was fun. I learnt how to talk to camera and what you could and could not do in an hour. I learnt about shooting ratios. Editing was still done with Steenbeck and scissors. I was privileged to work with Lawrence Moore as director who allowed me to sit with the editor. The film was an attempt to summarize what I think about the way in which the reputation of a great scientist is made in culture, and changes, and the way in which iconography helps you understand history.

That carried on through the 1980's and 1990's and I got involved in a number of television projects which were rewarding intellectually. The most important of them was 'The Day the World Took Off' under the aegis of Gerry Martin. Gerry Martin stands for the argument that I am trying to make, that trying to communicate is the same as making knowledge; I got to know him through museum work; my Department in Cambridge has one of the best collections of scientific instruments and books in the world, the Whipple Museum; museum endowed by Robert

Whipple, manager of the Cambridge Scientific Instrument Company, who in retirement made an astonishing collection of objects and books.



Simon Schaffer in a still from the first episode of *The Day the World Took Off*, a six-part documentary TV series on the origins of the industrial revolution. It was commissioned by Channel 4.

Robert Whipple donated his collection to the University in 1944, and the Department grew up around the museum in the 1950's. Here and in Oxford, scholarship in history of science grew up round the collection. The greatest curator of the Whipple was Jim Bennett (right), an historian of astronomy and a brilliant teacher. He organized exhibitions and publications and through him I met Gerry Martin, a wealthy, entirely self-effacing catalyst of innumerable initiatives, projects and programmes. I have never met anyone who could provoke ever more intense, energetic, committed work, without once being offensive or dictatorial.



It was a kind of Aristotelian pattern where Gerry provided ever slightly receding goals to which one moved rather than pushing you in any direction. Working on the exhibitions he helped sponsor in the mid-1990's was an eye-opener for me. It radically changed the questions that I wanted to ask. An exhibition on science in Britain and Germany a hundred years ago directed my questions to science and imperialism, Victorian technology and material culture, standardization of measurements, the significance of the trained eye, and the importance of industrial feedback loops.

I think this revolutionized the historiography of nineteenth-century science and technology. Simultaneously he was working with and sponsoring a loosely structured team of scholars around the theme of innovation and discovery, and the problem of genius. At our meetings he would often bring us back to basics with a bag full of neolithic tools to late nineteenth-century treasures. The principal lesson I learnt from him on material culture is that no object is so powerful that it means the same everywhere, or so weak that how you describe it defines what it is for.

It also meant the forging of a real intellectual community with doctoral and post-doctoral students, working on the physical sciences in the eighteenth to twentieth centuries, who have gone on to great success. When Shapin and I went to the Netherlands to receive the Erasmus Prize I was very keen that this group of people be there too; Jim Bennett also came but my principal regret was that Gerry wasn't there. I think he would have found it hilarious.

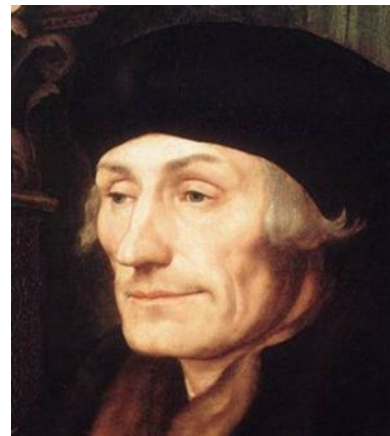
Editor's note. Readers may be interested to see the text of the acceptance speech by Simon Schaffer at the Erasmus Prize award ceremony:

Your Majesty, Your Royal Highnesses, members of the Board of the Erasmus Prize Foundation, friends, colleagues and fellow-students:

It is with enormous gratitude that I acknowledge the award of this year's Erasmus Prize, on behalf of the field of history of science in relation to culture and society.

We have just listened with pleasure to a performance of J.S.Bach's Well-tempered clavier, with its display of an instrument which could play in tune in all keys.

Temperance has been indeed the key to the working of many instruments, and Desiderius Erasmus was perhaps one of the most eminent exponents of the measured judgements of temperance in public life. Previous laureates have therefore often elegantly referred to the works and significance of Erasmus himself. Five centuries ago, somewhere between Rome and Chelsea, he composed his astonishing set of orations in the person of Folly herself. This seems an apt precedent for our own orations here. But sometimes Erasmian temperance seemed to fail. In his Praise of Folly, he ironised with some force on the condition of the sciences. The sciences were a plague of mankind. It was better to live in a state of naturally ignorant innocence than impiously to probe nature's secrets. Consider the poverty of scholars, gnawing on radishes, fighting off the lice and fleas. And the only sciences, so he joked, which were taken remotely seriously were those which most resembled foolish commonsense: medicine and law.



All this might seem to stand somewhat apart from the admirable terms of the citation of this year's Erasmus award: the sciences most embody the enlightened ideals of progress and rationality, thus making them the carrier of European socio-cultural identity. The place of the instruments of temperance may seem less clear to us now. So we need now to make sense of how the sciences work and what they mean. There is perhaps something characteristically Erasmian about the attitude of our field. We agree that there is much to be learnt from the way a society treats its scholars and experts as well as its poor and ill. We agree that what is taken as sacred is too often hedged round with bans on profane and sacrilegious curiosity. And we certainly agree that there are profound relations between our most reliable forms of knowledge and the practices of everyday commonsense. We have sought to show that scientific knowledge is made in local and mundane ways. It depends on no especially inspired nor excessively rational methods. It relies on the tough struggles of persuasion and credibility. The aim has been to get at the cleverly

artful work which makes what people know and to understand how that work is organised and challenged.

One of the many pleasures of this year's Erasmus prize award is the strong sign it offers that the field of social history of science has major importance for public knowledge and contemporary debate. There is an immense public appetite for more reliable knowledge, especially an understanding of the very processes by which this knowledge is produced. Too often this appetite is countered merely with second-rate simplifications in hock to vested interests or to the dictates of the information markets.

Our field offers indispensable resources which can learn from and contribute to the provocative enterprises of museums, exhibitions, electronic media and mature public debate. Our interest in ingenious hardware, in training and recruitment, in publicity and performance, in application and controversy flows from the salient roles of all these features of the life of the sciences. The field depends entirely on the work of many colleagues and students, allies and informants. The principle that knowledge is made through co-operation ought to be illustrated by the way we try to pursue our own work.

To understand, for example, how trust in knowledge is and has been distributed in our society is precisely to engage in some of the most profound contemporary questions of politics and ethics. Reflect on the appalling inequalities in access to knowledge and social resources which dominate the global economy; consider the bellicose assertion of allegedly self-evident principles as the sole basis on which any good society can be organised. Because they help stipulate what is human and what is natural, the sciences are entirely implicated in these conflicts. This is why it matters so much to sustain the conversation about how reliable knowledge works and who can take part in its making.

In his eloquent acknowledgement of the 1990 Erasmus prize he won for his work as archaeologist, my pre-eminent Cambridge predecessor the late Sir Grahame Clark observed that his own work showed how cultural values, rather than what he called mere biological imperatives, governed human development. He insisted that what it was to be human was to engage in cultural choices. This seems to me one of the principal, optimistic, consequences of our own work too. Against a range of fundamentalisms which would stipulate in advance our own natures, we must insist on the freedom to choose the way we live and how we know our world.

I cannot close this acknowledgement of the award of this year's Erasmus prize without reference to my co-winner. Steven Shapin has for a quarter-century been my guide and colleague, a constant source of wit, acuity, scholarship and sheer commonsense. His ability to communicate in clear, jocular and profound terms has been an inspiration for me, and for many of the colleagues gathered here. He taught me how to work collaboratively and how to think better about what we might otherwise take for granted. Indeed, these principles of straightforward language, collective action and critical scrutiny seem to me to be some of the most important values of the entire field which is rewarded here today.

10. Broadcasting

Linked to that are two themes that I think the work depended on. One is broadcasting, which I find both extremely entertaining and very frustrating. I worked on 'The Day the World Took Off' with David Dugan as producer/director, another media genius. Alan and Gerry had a scheme of focus, moving backwards in time and outwards in space. Historians don't begin with abstract social theory from which they derive a particular consequence. They make the instances that they have mastered and analysed, count backwards genealogically as telling lessons for much more global questions.

Theory is absolutely about voyaging elsewhere to alien places and learning and systematizing particular bits, then putting them back together. The work that we did for 'The Day...' and a handful of other programmes that I have been involved in and am proud of is exactly like that; the pressure there is always on the telling, and weaving it into a set of images and sounds that hint at and imply a much more global narrative.



BBC photo of Schaffer for the 'Light Fantastic' BBC4 documentary series.

I was later involved in a series on light for the BBC from 2001-2003. I wanted to be involved in a project where I could tell stories in a way that I wanted to tell them. Fortunately I was contacted by a team from the BBC who wanted to make a largish programme on optics. In conversation with the various producers involved, particularly Paul Sen, it emerged that there was a possible scheme to do something on light rather than optics, which would broaden and deepen the scope of what we might want to talk about.

The other important thing there was to rethink the nature of chronological narrative when thinking about the development of the sciences. What we did was to choose overlapping chronology for each programme. I was most proud of the programme on electric light. It coincided with Olafur Eliasson's weather project at Tate Modern which made the entire argument of this series in one installation. In sum, it is possible to make very complicated ideas into material objects, then to extract from them new, better, refined ideas, models and theories, which had not been obvious to me before.



Olafur Eliasson, The Weather Project, Tate Modern 2003.

It has culminated in my being offered the chance to become a trustee of the National Museum of Science and Industry, again an eye-opening experience, and an area that I shall be working on for the next few years.

11. Museums & Exhibitions



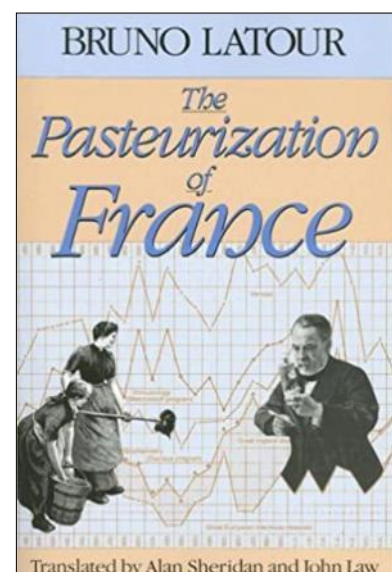
Bruno Latour, philosopher, anthropologist and sociologist.

Related to this is the theme of the museum and public exhibition on which I have worked with Bruno Latour. Latour has become the best known exponent of something like science and technology studies in the last decade. My impression of him and his work is radically different from that of many of my colleagues. I first met him in Bath in 1980. My regular trips to Paris for research kept me in contact with him. He was the first person to take the book that I did with Steve Shapin seriously though his interpretation differs radically from ours.

He was trained as a theologian, an anthropologist and a philosopher. He was based for a substantial part of his career at the Centre for the Sociology of Innovation at the School of Mines. With his colleague Michel Callon, he developed a project to analyse how the sciences are pursued. As an intellectual he is one of the major figures that the revolution in the understanding of the sciences has produced. Personally he is charming, hospitable and generous.

In the late eighties I published an article which was somewhat critical of one of his masterpieces, 'The Pasteurization of France'. Bruno responded by coming to Cambridge with the very best of his family's wine and sat with me for a couple of days talking through what I meant and what it meant for him. I thought that was an act of intellectual honesty and rigour that you don't often find.

He is also committed to making sure the work we all do is public work, not wrapped up in jargon. In collaboration with Peter Weibel he has done a couple of astonishing public exhibitions at the Centre for Culture and Media, Karlsruhe. He encourages



colleagues and friends to participate in knowledge-producing, culture-producing activities, what he calls assemblages which I think are some of the most exciting projects I have ever worked on.

Although we never quite agree he has provided me with some of the most productive public and academic activities in which I have been engaged. He is also a model, as is Steve Shapin, for how to deal with trainees and students, especially in history of science where there is no background that is irrelevant, so perversely, no one is qualified to speak. The activity of training and collaborating with students is endlessly analytic and open-ended.

12. Being Jewish

Being Jewish, there is the sense of observing things from the side; of being absolutely assimilated without being a member; of thinking from a slightly different perspective and noticing a little more quickly what is being taken for granted. There is a culture that can be entirely observant in religious terms without having belief. My family life was absolutely absorbed by the Synagogue and rituals without it ever seeming to have any theological or spiritual aspect.

Maintaining the culture of Judaism is maintaining the faith. There was an expectation that I was to be the incompetent but clever member of the family. Literary and scientific life is seen as indispensable to the culture and the family. There is a strange mixture of pride and resigned tolerance; not a culture that heroizes the intellectual and learning. Much humour dwells on the complete uselessness of such persons; but also an acceptance that it is potentially a way up and out of the ghetto.

Historically Jews were limited by what they could do; could not go into the army for example. There are tensions around Zionism. My parents were always ambivalent even when the support for Israel by Jewish intellectuals was unquestioned. I went to Israel, to the Hebrew University, in my gap year. It was eye-opening both in terms of becoming better informed about what was going on in the Middle East but also I became permanently disillusioned about the Zionist project.

Zionists wanted to break a certain stereotype of the Jew and replace it with a more physical, athletic, militant, virile image. The idea of working on a Kibbutz never appealed to me, but not for ideological reasons. Many non-Jews did work there in the early seventies, but now all that has gone.

13. Reflections



Svante Lindqvist had the job of setting up and administering a museum of the Nobel Prize which opened just a few years ago. Svante is an impressive technology historian who wrote on the introduction of the steam engine into Sweden and an article on glass-blowing as the key technology of the modern world. The challenge was how do you show the Nobel Prize, what is there to show.

He had a clever idea which was to note that certain places have been peculiarly good at winning it, and Cambridge is certainly one of them. He commissioned a group of Swedish film makers to make fifteen minute films about these special places.

The film on Cambridge followed a carrot from a fenland farm to high table at St John's College. Around this were interviews with Cambridge people about the networked quality of the city.



I was interviewed in my home and said that Cambridge had been astonishingly rewarding for me because it is sufficiently withdrawn and hybrid to provide mixtures of resources when one needs them. It provides a certain kind of retreat but is very porous. My work on Newton which I am about to publish, which is called 'Newton on the Beach', indicates that he may seem to have been isolated and cloistered but if one looks at the data he was able to command from Cambridge

it is very well linked with global information order. Nevertheless, the capacity to withdraw has always been indispensable.

The juxtaposition of unanticipated skills has been absolutely crucial for me, that relatively effortlessly one can assemble teams of people with heterogeneous interests and skills. That is the key to being creative.

Gerry Martin's description of the ideal state, bounded but leaky; visitors passing through, technicians, the unknown collections, the strangely motivated student, the bizarre shelving system of the University Library, can be friendly without being collegial. There is also the opposite; this is a fragile system; one danger is the confusing of defence of autonomy with defence of elitism, and the demand for outreach with that of abandonment in the sense of the intellectual work we are trying to do. Were we not involved in multifarious activities it would be very hard to defend what is going on here, and if we were not productive it would be indefensible; that balance is increasingly hard to strike.



In 1898 Britain's imperial power was peaking and British experimental psychology was just getting into its stride. Led by anthropologist A.C. Haddon, the Cambridge Anthropological Expedition to the Torres Straits included, among others, Britain's leading experimental psychologist, W.H.R. Rivers, ambitious William McDougall (still in his twenties), and young C.S. Myers, all canonical pioneer psychologists.

I got interested in W.H. Rivers, a physiologist, anthropologist and psycho-analyst. I learnt about him from Anita because Rivers was a member of the Cambridge Torres Strait expedition in 1898; his work was fascinating to me because of what one might call the extra-mural laboratory - how can the exterior world be changed so that one can learn about it. A major conference was co-organized by Anita Herle on the relation between anthropology and psychology in the wake of his work.

Anita took us to the Torres Strait in 1996 as part of her work on the subject and I have been privileged to visit it twice and to meet Torres Strait islanders both there and on their many visits to Cambridge.

It raised many questions for someone with no anthropological training. A second strand was reading the work of George Stocking on Franz Boas. Boas had trained as

a physicist and had moved to ethnography and anthropology and the traces of his scientific training marked his later work. I wondered whether this was also true of Rivers given that he had been doing work in optical physics and physiology. I wondered whether it was possible to use his work to analyse laboratory science. I gave a paper on the subject to a Department of Social Anthropology seminar which Keith Hart published in a Prickly Pear pamphlet. Rivers is a ghost who still haunts this town, still fascinates and provokes thought.



I think we are facing a real political and economic crisis within the academy around the notion of discipline. On the one hand, we are told, there were once rigid discipline structures but now we live in an epoch of fluidity, hybridity and multi-disciplinarity.

However, the rigid system did not exist before the Industrial Revolution. There is a constant struggle between amnesia and nostalgia. For most of my career it was assumed that the arrow of history pointed West. The Euro-centrism and Anglo-American-centrism of our discipline is the consequence of that great delusion.

I think the single most exciting thing that is happening to my discipline is completely and seriously to abandon that intellectual map so that one no longer assumes that arrow of history. On the contrary, one goes back to the eighteenth-century confident in the knowledge that the loss of economic and political leadership by the Qing was probably a blip.

This would be to re-ask the Needham question but with a completely different premise. Needham assumed that the question was why only in Europe and not in China. Inverting the question, one should ask why this temporary and possibly aberrant change in the intellectual and technical map of the world. I think would change our whole field because of fluid disciplinarity, almost all other fields as well. I think the conversations one could have in the future would start cognitively in a different place. The premise of our work must change completely in the next generation, and it will.
