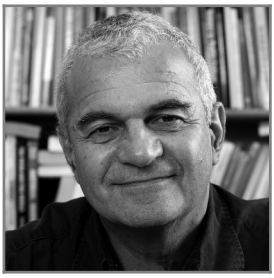
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THE BATH SCHOOL AND THE STRONG PROGRAMME

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I describe the origins of the 'Bath School' with its Empirical Programme of Relativism (EPOR) and its relationship to the Edinburgh Science Studies Unit and its Strong Programme. The Bath work arose independently and did not make contact with the Edinburgh Unit until around 1973. But it then benefitted enormously from the interaction. It is likely that without the Strong Program to 'bounce off' the Bath work would have remained an eccentric curiosity. Nevertheless, the two approaches remained different driven by a strong emphasis on empirical work at Bath, albeit set in a philosophical context, and, in the first instance, a more purely philosophy directed approach from Edinburgh. It is suggested that the apparently coincidental alignment of the approaches of the two groups arose because of the way British sociology had developed. The expansion of sociology in the 1960s drew heavily on the pioneering of the subject at the London School of Economics and that gave rise to a strong philosophical flavour in British sociology, not found in, say, the US. That is probably how both groups found themselves drawing on the later philosophy of Wittgenstein with his theory of meaning depending on use, with 'forms of life' being a precursor to Kuhnian paradigms. The differences between the two groups became less important as Latourian Actor/ant Network Theory came to dominate Science and Technology Studies followed by the determined democratisation of science associated with Jasanoff's 'coproduction' idea, which brought ordinary citizens into the heartland of science. Neither Edinburgh nor Bath ever wanted to doubt the existence of elite scientific expertise even if they did remove science from its claims to certainty, allowing a modicum of social values into the truth finding process. The paper regrets the current state of STS, which appears to support a populism which makes science, not a continuation of politics by other means, but simply politics. The paper invites speculation about how things could have turned out differently.

Keywords: Strong Programme, Bath School, sociology of scientific knowledge, experimenter's regress, Third Wave of Science Studies

БАТСКАЯ ШКОЛА И «СИЛЬНАЯ ПРОГРАММА»

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В статье рассматривается генезис «Батской школы» и ее Эмпирической программы релятивизма (ЭПР), а также ее соотношение с Эдинбургским отделением исследований науки и его Сильной программой. Батские исследования возникли независимо от эдинбургских; их непосредственный контакт установился лишь около 1973 г. Однако последующее взаимодействие оказалось для батской группы исключительно плодотворным. Можно предположить, что при отсутствии Сильной программы, служившей своего рода «точкой опоры» для критической рефлексии, батская работа так и осталась бы маргинальным интеллектуальным курьезом. При всем при том два подхода сохраняли существенные различия: в Бате

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приоритет отдавался эмпирическим исследованиям – пусть и в философской рамке, – тогда как эдинбургская линия изначально тяготела к чисто философской проблематике. Выдвигается гипотеза, что наблюдаемое совпадение траекторий обеих групп не было случайным, а коренилось в особенностях развития британской социологии. Ее экспансия в 1960-е гг. во многом опиралась на традицию, заложенную в Лондонской школе экономики, что сообщило британской социологии выраженный философский компонент, не свойственный, например, американской социологической традиции. Этим, по всей вероятности, объясняется обращение обеих групп к позднему Витгенштейну с его концепцией значения как употребления, где «формы жизни» выступают своего рода предшественником куновских парадигм. Расхождения между двумя школами постепенно утрачивали остроту по мере того, как в исследованиях науки и технологий (STS) все большее влияние приобретала акторно-сетевая теория Латура, а вслед за ней – концепция «совместного производства» (co-production) Ясанофф, ознаменовавшая решительную демократизацию науки и включение рядовых граждан в ее ключевые области. При этом ни Эдинбург, ни Бат никогда не ставили под сомнение само существование элитного научного знания, даже если они и лишали науку притязаний на безусловную достоверность, допуская определенную меру социальных ценностей в процесс установления истины. В заключительной части статьи высказывается сожаление по поводу нынешнего состояния STS, которое, судя по всему, смыкается с популизмом, превращающим науку не в продолжение политики иными средствами, а просто в политику. В связи с этим автором ставится вопрос о том, как могла бы сложиться альтернативная траектория развития данной области, и предлагается поразмышлять над этим читателю.

Ключевые слова: Сильная программа, Батская школа, социология научного знания, регресс экспериментатора, Третья волна исследований науки

I always date the start of the strong programme to David Bloor's 1973 paper, 'Wittgenstein, Mannheim and the Sociology of Mathematics' [Bloor, 1973]. That was the paper that impressed me. But I was doing strong-programme stuff *avant la lettre*, from, according to how you count it, 1967, 1971, or 1972. Actually, I didn't even know there was a field called the 'sociology of science' with Robert Merton as its figurehead until late in 1971. I knew about Robert Merton but as the promulgator of 'Merton's Theory of Anomie', not the science stuff so I was bound to be inventing the subject for myself. That was until I wound up at the University of Bath to do a PhD, with a Mertonian as a supervisor. My origins as a sociologist were very modest – a 'London External Degree' at a local college in Liverpool, with no sociology at a 'proper' university until I did a one-year taught master's in sociology at Essex in 1970–1971.

Going back to those 'according to how you count it' dates, 1967 was when I read, with enormous concentration, Peter Winch's book, *The Idea of a Social Science* – published in 1958 [Winch, 1958]. The reason for the enormous concentration was that, at first, I could not understand it, but when I did understand it, it changed my life. I would eventually realise when, sometime later, I read Kuhn's *Structure of Scientific Revolutions* [Kuhn, 1962], that Winch had prefigured Kuhn's idea of 'paradigm' by four years. Winch described the way surgery worked before and after the establishment of the germ theory of disease: the concept of 'germ' was given meaning by the changed actions of surgeons – shifting from operating in dirty waistcoats to pedantic disinfecting in spotless surgical gowns and gloves etc. This was a change in the social life of surgery. Winch took the idea from the later (eg 1953) philosophy of Wittgenstein [Wittgenstein, 1953] and referred to 'forms of life' and how, if you wanted the meaning of a concept you should ask, not for the meaning, but for the use. Winch thought he was attacking sociology using his exposition of Wittgenstein to say that sociology was 'misbegotten philosophy', but he didn't understand his own book. What he was really doing, and doing it quite brilliantly, was showing how concepts and actions were two sides of the same coin. He could just as easily have said that philosophy was misbegotten sociology. And, of course, Bloor would spend many years trying to get philosophers to accept that Wittgenstein was really a sociologist – it was delightful for someone like me to discover, some years later, that Bloor was doing this [Bloor, 1983]. But this was after I had stumbled upon Kuhn and realised that his 'scientific paradigms' were Wittgensteinian forms of life applied to science.

1971 was when I had to find a little research project to finish off my one-year taught master's degree in sociology at Essex University and wandered round the science labs until I found a likely candidate. This was people trying to build a Transversely Excited Atmospheric Pressure CO_2 laser (TEA-laser), and largely failing. I got in my car and visited all the locations in the UK where the attempt was being made to build one of these recalcitrant devices. I decided I would use my Winch/Wittgenstein/Kuhn perspective to treat the building of such a laser as like trying to learn a language rather than trying to follow a circuit diagram or list of instructions. I called this the 'enculturational' as opposed to the 'algorithmical' model. This was a great idea because I found that no-one could make a TEA-laser work unless someone from their laboratory had spent time in another laboratory where they had a working model (inventor aside, of course). I still knew nothing about the sociology of science nor the Edinburgh group – this was just my obligatory master's research project. I then went to Bath University to do a PhD and that is where I began to find out about the community of which it could be said I was becoming a part because my Bath supervisor was Stephen Cotgrove, who had co-authored a book about how industrial scientists could work within



the Mertonian norms. Of course, the job of a British sociology student in those days – not long after 'the sixties' – was to explain to his or her supervisor how thoroughly wrong they were about almost everything.

1972 was when I found myself driving across America in an old car interviewing scientists. I was completing my TEA-laser study by interviewing the Americans who had tried to build one; I was trying to interview some theoreticians in the domain of the theory of amorphous semiconductors, but that went nowhere except in terms of learning what it meant to understand and not understand a field of science; I was interviewing parapsychologists, which was its own kind of experience and would lead to some papers and a book; and I was interviewing scientists in the controversial field of the detection of gravitational waves, a project that I would stick with for another 45 years.

If I've ever had a 'eureka moment' it was towards the end of this journey across America. Driving through the endless scrub of Nevada I realised that I could not write up the gravitational wave study in the way I had written about the TEA-laser study. In the case of the TEA-laser scientists I knew whether they had succeeded in building a working device because if they had it would produce a powerful beam of infra-red radiation that would make the wall of the laboratory smoke. In the case of gravity wave detectors, however, scientists were still arguing about whether a device of the sensitivity of those currently being built should or should not detect gravitational waves. And if they didn't know that, they didn't know whether their gravitational wave detector was like a TEA-laser that worked or a TEA-laser that didn't work. For about 20-minutes I was devastated as I realised my PhD couldn't be written up in the way I had intended when I started my trans-American journey so I had wasted my time and money. Then it struck me that I had 'discovered' that experimenters in frontier fields never knew if their experiments were working or not and that is what they had to argue about. Frontier science was not a matter of aggregating the results of a series of repeated experiments but a matter of arguing about which set of experiments with competing results were the ones that were working properly. Later it became clear that the same problem beset the fields of parapsychology that I was looking at. This problem would eventually come to be called 'the experimenter's regress' and unknowingly, I had tripped over a vital foundation stone for the 'social constructivist' approach to science. But all this was still before I knew there was a strong program.

It was around this time or in the next year or two that I found out about the existence of the Edinburgh Science Studies Unit and something about the work going on there and found out more about the field of which I was becoming a part – the sociology of science. I remember bumping into Richard Whitley who was in the sociology of science group located in Manchester university and he asked me where I was going to publish my TEA-laser study. Up to that point I had just thought of myself as

a student not someone who publishes stuff. I think it might have been him who told me about the journal *Science Studies* and its editor, David Edge, who was also the head of the Science Studies Centre at Edinburgh. I suppose it must have been 1973 when I sent my TEA-laser paper, now including the American data, to Edge, as a submission for publication in his new journal. Edge wrote back very encouragingly, telling me how good it was that someone was doing empirical work in this domain not just theorising about it. He sent the paper out for refereeing and one referee wrote back saying that what I was writing about was Michael Polanyi's concept of tacit knowledge (which I'd never heard of). So that is how the paper (and a lot of my subsequent career) has come to be about tacit knowledge.

The TEA-set paper was published in 1974 [Collins, 1974] and the paper about experimentation in frontier physics supported by the gravitational wave study was published in 1975 [Collins, 1975] and this led to me being one of most salient researchers in the new version of sociology of science that was building fast.[1] I also won a grant to study the parapsychology that turned on the Israeli showman, Uri Geller, who was puzzling scientists, and that was how I wound up with a job at Bath University. With the grant I was able to recruit Trevor Pinch to be my research assistant. This, along with a graduate student, and a lot of subsequent publications, led to us being referred to as 'The Bath School', which had by now acquired an international reputation. We had our 'Empirical Programme of Relativism' (EPOR) while the Edinburgh Unit had its much better known 'Strong Programme'.

What I was doing and what Edinburgh were doing had common roots in the later philosophy of Wittgenstein. I think this grew out of the way sociology in the UK developed. At the time I was becoming a sociologist, there was a lot of philosophy in UK sociology. The London External degree that I had done was driven by the syllabus of the London School of Economics and that LSE syllabus fed strongly into most British universities. Philosophy of science was a central concern, with Karl Popper being the figurehead, but the LSE also felt it had to teach Winch's philosophical attack on sociology (philosophical attack as Winch saw it) and that was why I found myself reading it with burning intensity. Various syllabuses would also include discussion of what was called 'the rationality debate'. This questioned how anthropologists should treat 'strange' societies such as that of the Azande. The Azande believed that a good test for whether someone was a witch was to use the 'poison oracle'. They asked the question and made-up some poison called 'benge' and fed it to a chicken. If the chicken died the person was a witch, if it lived, they were innocent. But they repeated the test to make sure. This time,

[1] The ideas in these papers were consolidated in my 1985 book *Changing Order*, which would have been written a few years earlier were I not working on parapsychology fieldwork.



if the chicken lived the person was a witch and if it died, they were innocent. The question for the anthropologist was whether the Azande should be treated as 'irrational'. When I was at Essex, I wrote an essay about this arguing that the Azande weren't irrational because if you were inhabiting a magical rather than a scientific paradigm/form of life it made perfect sense that the magic should work different ways on repeated tests. So, this kind of paradigm/form of life kind of thinking was in the air in British sociology in those years. I think that's why I wound up doing something the consequences of which were not unlike the strong programme even though I had never heard of it. But one might say, I had the 'advantage' of not having heard of it because it meant my approach started from empirical studies. I loved getting in a car and interviewing scientists but I also loved putting the results into a philosophical framework because I started with Winch and Wittgenstein.

I had come from 'nowhere' and stumbled into this stuff and without the strong programme I think I would have just disappeared as just another eccentric.[2] New ways of thinking have to be recognised and build alliances if they are to flourish. Ludwik Fleck's analysis of science [Fleck, 1979] published in the 1930s was pretty much the same as Thomas Kuhn's except for the notion of paradigm-revolution but he was not recognised, at least not in the English-speaking world, until long after Kuhn's book caused a splash. The existence of the Edinburgh Science Studies Unit and the controversy associated with the Strong Programme gave me a social identity and a social group to join up with, rather than just being classed as an odd individual. This had a disadvantage too. The downside was that people who came across my work thought I was a member of the Edinburgh team and had come out of the Strong Program. Therefore, I had to spend some time doing 'product differentiation' in respect of the Edinburgh group even as a number of Edinburgh students were taking up my strongly empirical approach. Nowadays, that careful picking apart of differences seems odd because from the 1990s onward Bruno Latour's Actor Network Theory and hyper-symmetry made Bath and Edinburgh seem very close in term of our overall approach and we were also very close in not thinking that our job was to attack science and level it down, but to understand it better while still respecting it. The similarity of Edinburgh and Bath was even more apparent with the growing popularity of full-scale attempts to democratise science associated with Jasanoff's 'co-production' idea and support for the parent rebellion against Measles, Mumps and Rubella vaccine, which transported ordinary citizens into the heartland of science.[3] Neither Edinburgh nor Bath ever wanted to doubt

[2] Though the little TEA-laser study is still being cited and even discussed in the newspapers 50 years on.

[3] Though it has to be said that Wynne, who was central to this democratisation movement, was originally an Edinburgh student.

the existence of elite scientific expertise even if they did remove science from its claims to certainty, allowing a modicum of social values into the truth finding process. This paper regrets the current state of STS, which appears to support a populism which makes science, not a continuation of politics by other means, but simply politics.

Though David Edge, an ex-radio astronomer, was the founder of the Edinburgh Science Studies Unit, the theoretical drive came from David Bloor and Barry Barnes. It was Bloor's concept of 'symmetry' [Bloor, 1976] that delighted me most in so far as it fitted my 'relativistic' approach to things and, of course, I loved the way that Bloor was interpreting Wittgenstein. Though I admired Barry Barnes, I did not get the same warm feelings from his work. But Bloor was a philosopher and wanted to do philosophy. Only later in his career, after he had written two books about the interpretation of Wittgenstein, did he turn to case studies of airplane flight, circling round to his personal enthusiasm, flying – he piloted an old Spitfire at one point. But neither Bloor nor Barnes had any enthusiasm for empirical investigations that took them out of the library and that's why several of the Edinburgh PhD students turned to my work as a model.

In terms of building a community, however, it was David Edge, founding the Unit and the journal and engaging us in foreign conferences who made the growth of the approach possible.

In immediate concrete terms the person who made all the difference was Steven Shapin, a biologist turned historian of science, also employed in the Science Studies Unit. Shapin knew a whole network of historians of science – he would co-author a prize-winning book about Robert Boyle and the airpump with Simon Schaffer, a historian of science at Cambridge – and introduce non-entities like me to these people, and be the driving force behind a series of conferences we organised at Bath. They were great days and for a few years we thought we might succeed in reinventing social studies of science as a genuinely cross-disciplinary field – a true combination of history, philosophy and sociology of science. But the professional roots of history of science and philosophy of science went too deep into the soil for them to join in with the shallow rooted sociology of science. And it is true that history had professional standards that sociology could not match and, according to me anyway, sociology, and sociology of science in particular, have misunderstood their role in the world – we'll come to that – so I doubt it would ever have worked.

Meantime, I went from being a nonentity to an 'entity', becoming the President of the international Society for Social Studies of Science from 1991 to 1993. David Bloor continued doing his individualistic thing, while Steve Shapin did stand for the presidency after my time was over but, scandalously in my view, failed to be elected. I wonder if the three academic professions would have held together longer if that election had worked out differently.



Since then, a lot of blood has flowed under the bridge. From about the 1990s onwards, Latourianism came to dominate the science studies domain to a staggering extent. To me it has never made any sense. The speed with which it 'caught-on' must have had something to do with its polysemic quality. A polysemic slogan is useful in political campaigning because it allows voters to impute to it whichever meaning they prefer. 'Make America Great Again' is like this: it can mean invade Venezuela, Greenland and Canada, or pull out of 'forever wars' such as that in Ukraine and dump your allies; it can mean 'make our streets safe again' or send ICE into Minnesota to murder citizens and strengthen the gun lobby; it can mean bolster employment by imposing tariffs on foreign goods or do away with welfare and support 'the wealth-pump' while ruining the climate for poor nations. With Actor(ant) Network Theory you could not even tell if was the most radical application of social constructivism there had ever been, or a bugle call for collaborationism, or a recipe for returning to old fashioned history of technology with the non-humans regaining their traditional agency as I argued in a 1992 article [Collins, Yearley, 1992, especially pps 316ff]. I also offered a narrower interpretation of the success of Latourianism: that Bruno's brilliance was in giving arts and humanities scholars a way of criticising science without having to understand any science and thus coming out on top of the 'two cultures' debate without any 'heavy lifting'. You can see how confused I am about it because I've been complaining that Latour offered no explanation for his notorious *volte face* of 2004 when we all found him back on the side of science again (leaving the science warriors sprawling). But then I realised after some nudging that in the earlier 'Epistemological Chicken' I had argued that ANT was scientifically conservative. So, it wasn't really a *volte face* after all but just another small step in the usual conservative direction dressed up as *volte-face* so as to be a judo-type sidestep 'performance' with everyone, including me, tumbled over the outstretched leg for dramatic effect.[4]

Anyway, going back to the blood flowing under the bridge, there's been a tsunami's-worth in the last decade. I wonder how much of the STS domain's reaction to Trumpism can be traced back to those early differences. Perhaps the unpredictability of what has happened to science in America suggests there is nothing to say that could link the early days to today's happenings. Or maybe it was not so unpredictable. Remember, there was a period on the 1990s when the 'science warriors' emerged and started warning about the dark ages that would follow from the levelling down of science. And they were right – the dark ages for science have arrived! And they have arrived in the wider realm of national politics, with

[4] Thanks to Nicolas Langlitz for pointing out to me that this wasn't a *volte-face*.

post-modernism serving as a workshop manual for the erosion of truth in entire populations.

Unfortunately, the science warriors, though they saw what was coming, had no idea what to do next. All they could do was put their energy into sniping stupidly at the really good work that was being done in the post-1960s studies of science toward understanding science better.[5] In 2002, prior to Trumpism, Collins and Evans (C+E) tried to do something that they thought was more sensible by inventing the Third Wave of Science Studies; they were really reacting to ambiguity over vaccination. C+E said the First Wave was the high point of science before and after WWII. They said the Second Wave was the much richer analysis of science, probably triggered by Kuhn's book but based more in the philosophy of Wittgenstein which began in the early 1970s which had the effect of eroding the mountain of science down toward the plain of ordinary knowledge; unfortunately this philosophical wave metamorphosed into Latourianism and also into the democratisation of science movement, led by Sheila Jasanaoff and Brian Wynne. C+E said the Third Wave had to be designed to raise the level of science back up again, if not so far as the first wave, at least far enough so that we could still make sensible, science-based, decisions about vaccination and so forth.

The Third Wave was to be based on the analysis of scientific expertise, which could be recognised and analysed in real time even if scientific consensus formation was much slower. The more recent iterations of the Third Wave programme declare that science is 'craftwork with integrity' with aspiration that goes with 'integrity' being the crucial feature: as an institution, science is obsessed with truth in the way that no other institution is obsessed and can therefore act a check and balance. Its political importance is not so different as that of law, or the parliamentary opposition and, unlike these, it could also be an object lesson in how to use truth to make decisions of every type. And then along came Trump, revealing in cartoon form how vital the Third Wave was if STS was to contribute to the preservation of democracy.

But the unexpected thing is the way STS has responded. First, though the Third Wave paper is hugely cited, the term 'Third Wave' is almost never mentioned in STS circles. The citations are mostly coming from Philosophy, Psychology and other disciplines, with those coming from within STS nearly all having to do with the analysis of expertise. STS it seems, is determined not to 'look up' at the huge lump of political rock about to collide with the Earth and destroy all scientific life.[6] Instead, STS is either stuck with the criticisms of the Third Wave paper that

5 Richard Dawkins and others have recently reiterated their tired old stuff in a new book which I imagine they wrote wearing red baseball caps sporting the 'Make Science Great Again'.

6 I am referring to the film, *Don't Look Up*.



greeted its initial publication or with a determination to 'democratise' science and make it the property of the ordinary people or of disadvantaged or marginal groups.[7] There seems to be no realisation that the disadvantaged groups will be destroyed by the rock just as much as the rest of us. The problem has changed: the problem is no longer rescuing the disadvantaged from the power of elite scientists but rescuing elite (expert) scientists from the power of politicians. Truth is stranger than fiction so it is in this case. Sheila Jasanoff who, along with her ally in supporting parents in their opposition to Measles, Mumps and Rubella (MMR) vaccine, have never withdrawn or reconsidered their position on this matter but, on the contrary, Jasanoff has 'doubled down', arguing (supported by Kant so she claims though this is disputable), that citizens have to make their own judgements. Her words are a perfect match for those of Robert F. Kennedy Junior and Donald Trump is uttering the same stuff, attacking not only climate change science, which attack is at least comprehensible in terms of oil interests, but vaccination too, right down to the particular case of MMR.

I am more and more astonished at what the STS community is doing, and not doing, and cannot but wonder if it would have turned out differently if things had been different in the early days.

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[7] The opposite approach is represented in, for example, [Collins, Evans, 2026].

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