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EXPLANATION, REDUCTION AND THE SOCIOLOGICAL TURN IN THE PHILOSOPHY OF SCIENCE OR KUHN AS IDEOLOGUE FOR MERTON'S THEORY OF SCIENCE

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EXPLANATION, REDUCTION AND THE SOCIOLOGICAL TURN IN THE PHILOSOPHY OF SCIENCE or KUHN AS IDEOLOGUE FOR MERTON'S THEORY OF SCIENCE

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Sociological attempts to explain the success of science try to derive that success from characteristics of the social formations in which science is embodied. Not all derivations are reductions; and not all reductions are objectionable. The recent emergence of the science of the sociology of science provides an opportunity to analyse one kind of explanation typically used in sociology and, in particular, to explore whether, in explaining scientific success, sociology thereby 'reduces' science in some objectionable way. My philosophical thesis will be that sociology illicitly reduces only if it tries to explain socially a crucial component in the success of science, namely the truth or otherwise of scientific ideas.

To display this thesis about reduction Robert K. Merton's case study (1977) of the emergence of the sociology of science and of the 'presence' of Karl Popper and Thomas Kuhn to that emergence will be examined. While not being found reductionist, it will be noted that ideas--neither Popper's nor Kuhn's--are given much role. A second, material sociological, thesis will be advanced to the effect that each man's ideas are needed to explain their 'presence'. Kuhn's ideas, it will transpire, legitimise the social formations in which the science of his time is temporarily housed. Popper, more ambitious, offers an explanation of the success of science that not only transcends the particular social formations of his time, but which happens also to be inimical to and critical of them and of Kuhn's ideas as their legitimation. Kuhn's ideas legitimate science's current social form, Popper's undermine it. These no doubt unintended consequences of each man's ideas are integrally necessary for explaining the trajectory of their careers. The serendipity has to do with the establishment, both academic and scientific, sensing these consequences and hence embracing Kuhn while holding Popper at a distance.

Reduction of science to society

At least two very different things can be meant by the success of science: organizational success and intellectual success. Organizational success includes such things as the rise of scientific societies, the introduction of science into the curriculum, growth in the absolute and percentage numbers of people who are scientists, rise in the status of the scientific profession, and so on. Intellectual success is the growth of scientific knowledge, whether in quantity, or in the rate of increase. The crucial question about reduction is whether organizational success is regarded as independent of intellectual success.

Robert K. Merton's pioneering 1938 study of Science, Technology and Society in Seventeenth Century England was primarily a study of organizational success, but it did not wholly discount ideas. That monograph set out to investigate the idea that the growth of science was not unconnected with other processes and events in the society. It was an implicit criticism of those historians of science who looked only at scientific ideas and their succession to one another. There can be no doubt that it was incomparably superior to the earlier style of history of science, which was largely dominated by the Great Men, Great Ideas approach. As we shall see, however, when considering the rise of the sociology of science in the mid-twentieth century Merton concentrates on organizational success at the expense of ideas.

Innovators are not responsible for their followers, so we should stress that the radical attempt to reduce the ideas to the organization, while tipping its hat to Merton, betrays him in spirit and in letter. Taking their cue from Scheler and Mannheim's sociology of knowledge, there has come to be centered at Edinburgh the radical programme in the sociology of knowledge'. This argues that scientific ideas are a kind of epiphenomenon of social formations, and will appear on cue as they are needed. This school takes for granted science citation and science indicators. Intellectual success can be measured by the production of units of knowledge, such as published papers; such papers can be weighted by the prestige of their outlet and their length; their influence gauged by the number of citations they receive in other papers. This reduction can be complicated by adding to papers, books and technical reports, and by discriminating, within citations, between mentions, uses and negative citations. It can then be extended beyond the production of intellectual material to the assessment of training sites, such as graduate schools. The intellectual success of graduate schools can be 'objectively' measured by such indicators as the intellectual output of the faculty, the output of graduate students, the 'placement' of graduates in other institutions, and the amount of research money attracted. The intellectual success of graduate schools can also be measured 'subjectively' by asking scholars, chairmen or deans to rank other graduate schools on an ordinal scale.

Citations and indicators are sometimes held to be measures of intellectual success. If they are so held, an element of circularity seems inescapable. Sometimes they are held merely to indicate, that is to correlate well with, true intellectual success. True intellectual success is, however, hard to specify. Hence citation, indicators; and so on.

So far we have discussed success. If success is a complex notion, science is no less complex. Much of the stir caused by T. S. Kuhn's 1962 monograph The Structure of Scientific Revolutions had, I conjecture, to do with his effort to specify what science was, not in terms of intellectual success but, in terms of organizational features. His thesis was that science is produced within a scientific community, and that a scientific community comes into being only when individuals and institutions subordinate themselves to a paradigm (or--in his later preferred usage--'disciplinary matrix'). A paradigm is not merely an intellectual construct such as a theory:

> "paradigms" . . . I take to be universally recognised scientific achievements that for a time provide model problems and solutions to a community of practitioners . . . (p. 11). In learning a paradigm the scientist acquires theory, methods, and standards together, usually in an inextricable mixture (p. 108).

Kuhn, no doubt, would deny any reductionist intent. He sought to specify science as he found it. As Merton suggests, he might say, 'je ne suis pas kuhniste' (1977, p. 109). The fact remains that the Edinburgh heresy combines Merton's pioneer work, with science indicators (consider the photograph in Latour and Woolgar's Laboratory Life of a disordered desk of papers--p. 102) and Kuhn's paradigms into an overarching attempt to reduce science to sociology. What are these reductions reducing? Answer: the traditional intellectualist account of science as a body of knowledge. Properties traditionally ascribed to this body of knowledge include: truth, certainty, based in experience, reliability, predicatability, authoritativeness. With the exception of truth and certainty, Kuhn's social reduction preserves these features as distinctive of science. Truth and certainty he abandons in the course of placing revolutionary change at the centre of his characterization of science. The Edinburgh school go one further, throwing into doubt all the other traditional properties of science also. Science for them becomes identical with its social formation; one having, like all such, some unique features; and also having some typical features. For them the sociology of science is a totalizing discipline. Merton takes it as a case of the emergence of a science, organizationally, biographically and, although he does not specify, intellectually. The Edinburgh school, by contrast, takes the sociology of science to be the master science: that which explains all the others, and itself.

Explanation Without Reduction

In contrast to these social reductions of science, there is a long intellectualist tradition that places truth and certainty at the centre, where they can be used to explain the reliability, predictions and authoritativeness of science, as well as the rise of scientific organizations, the trajectory of scientific careers, and other matters like science citations or science indicators. It's not just that a piece of work is good because it is cited; it is cited because it is good; and, moreover, it could be good but not get cited and vice versa. One major reason that this intellectualist tradition has been challenged by the reductionist programmes is that it broke down. During the extraordinary period when Newton's physics ruled virtually unchallenged as a paradigm of science all epistemological discussion took it and its established place for granted. Once, however, Newton's physics was overthrown, its truth, certainty, basedness in experience, reliability, predictions and authoritativeness were seen as deceptive. Whatever replaced it, there was no reason that that should not in its turn to replaced.

Intellectualism need not be seen as in opposition to such efforts as Merton's, to study the manner in which the success of science connects to other events in the surrounding society, to features of the organization of science, and so on. It is opposed only to the claim that science is no more than another, special, social formation. Acknowledging that science is another, special, social formation, intellectualism makes the non-reducible claim that what explains its specialness is its product. Science is the social formation that produces scientific knowledge. The success of science is then to be assessed neither by its organizational features alone, nor by measures of output or citation alone, but by both of these plus the nonoperationalizable concept of truthlikeness.

Thirty years before Kuhn, a young Viennese philosopher, Karl R. Popper, was bringing this consequence to the attention of the then ruling philosophy of science establishment, called the logical positivists. His ideas were not taken very seriously, if one is to judge by the paucity of citations of his work in the literature, the relative isolation and low profile of his career trajectory--of which more below--and the difficulty he experienced in publishing. Furthermore, despite his criticism, the logical positivist movement continued to flourish, reaching its apogee--in science indicator terms--about 1950.

Like Kuhn, Popper wanted to spell out what scientific success was, unlike Kuhn he wanted to do it without abandoning the tradition of intellectualism. Not unmindful of the fact that science was a social institution, he wanted to claim that its very special aim (namely, truth about nature) was pursued within a very special social structure. Success lay not in the social formations, nor in the indicators, prizes, or what not, and certainly not in the dogmatic imposition of ruling ideas; rather did it consist in refutable assertions about the world. Scientific success in Popper's model consisted in an increase in the quantity or in the scope of our knowledge of the world.

Merton's Study of Popper and Kuhn

In a nice reflexive twist, Merton offers a study of the emergence of the sociology of science as a case study in the emergence of science and hence a case study in tracing the connections between scientific success and other changes in society. Merton's own strategically placed position as a facilitator of the emergence both of the sociology of science and of Kuhn makes his participant observation report a fine-grained and invaluable document.

The first monograph Merton ever published (1938) inaugurated a field in which, he reports, there was little activity for ten years, little more for twenty, and which then began a rapid emergence. What was lacking was a theory of the sociology of science.

Two philosophers of science, Popper and Kuhn, offered theories of a connection between the science and society. Although both loom large Popper, despite publishing nearly thirty years before Kuhn, is less often cited. Popper's 1934 book, while influential among philosophers of science, was little remarked in the literature of sociology and the history of science. Only thirty years later, as the sociology of science was growing, were his 1963 and 1972 works to become the route for the filtration of his ideas over from the philosophy of science to sociology, via the sociology of science. This

> suggests that delayed cognitive interests wait upon appropriate cognitive and institutional developments in neighboring disciplines before they actually become operative (71).

This sentence of Merton's is a trifle opaque. It seems to say that Popper's lack of influence was because he was ahead of his time. Being ahead of his time means that there was a lag in the development of cognate disciplines which made them unable to take up his ideas. Philosophers, by contrast, did take up those ideas.

Merton seems to be arguing that a field of study had to be identified, isolated and mapped out prior to it beginning the search for a theory. Thus his own (1938) work seeks out patterns or connections which then become data to be explained by theory. Theory that arrives too soon will thus having nothing to explain.

In criticism of Merton it should be said that Popper's ideas were not taken up by the philosophers of science except in the Pickwickian sense that they were more or less systematically ignored. Secondly, Popper's ideas do not propose that data precedes theory. The puzzle thus is why Popper's ideas did not stimulate the growth of the social study of science.

Merton is on surer ground with his study of Kuhn. Kuhn came along at the right time, indeed was brought along to be in the right place at the about-to-arrive right time. Merton wants to show how Kuhn stood at an intersection where his own developing interests met those of others, and that his positioning for such serendipity was a sort of Hidden Hand wisdom built into the commanding institutions of American academic life. These institutions filter out a person like Kuhn, offer him opportunities to follow new directions of thought, and, as his work is recognised first at the local and later at the cosmopolitan level, slot him into a process of 'cumulative advantage'. Hence while the intellectual public was dazzled by the appearance of The Structure of Scientific Revolutions in 1962, the inner circles of the relevant academic élites were not. They had looked on Kuhn as a coming man since late in the second world war and had in anticipation heaped upon him many of the privileges and advantages available in American academic life.

Merton holds that the emergence of the science of the sociology of science is a self-exemplifying case of its own findings about science. The problem of the sociology of science as tackled in Merton 1938 was to explain the rise of science in seventeenth century society. His thesis was that 'the socially patterned interests, motivations and behaviour established in one institutional sphere--say, that of religion or economy--are interdependent with the socially patterned interests, motivations and behavior obtaining in other institutional spheres--say, that of science' (ix). The interdependences Merton explores are religion (especially the Puritan value system), economic (especially mining and transporation), and military.

> Before it became widely accepted as a value in its own right, science was required to justify itself to men in terms of values other than that of knowledge itself (xix).

Science for its own sake, pure science, came later:

> The automonous case for pure science evolved out of the derivative case for applied science (xii).

This seems obvious enough. When the problems are applied ones there is a wide constituency able to judge a scientist's work. If boats miss their landfall then owner, passengers, crew, and those awaiting their arrival can all judge either the navigation or the navigator to be faulty.

But when the problems are pure, the need for a specialist reference group is felt. So a scientist's

> claim resides only in the recognition accorded his work by peers in the social system of science through reference to his work (48).

Thus, concludes Merton in 1970, 'science is public not private knowledge'

The invisible college of peers is partly housed, nowadays, in élite academic institutions that attract to them and reward talent. The effect of this is to enable those on the frontiers of knowledge to engage in interactions far beyond their speciality. If thereby they enrich their field then it has been serendipitous. This is an expectation, but not a demand. Kuhn, in Merton's example, was both slow and reluctant to publish (91). It seems that in the end, however,

> if one's work is not being noticed and used by others in the system of science, doubts about its value are apt to arise (5).

So, the gatekeepers in the networks of academic privilege in the USA early identified T. S. Kuhn, a 'not yet widely identifiable young scholar' (101), 'doubly marginal', as a suitable recipient 'for valued opportunities in fields widely defined as alien to his own' (96). From graduate study in physics he was inducted into the Harvard Society of Fellows, gave the Lowell Lectures, taught at Harvard, was offered both a Guggenheim and a Fellowship at the Center for Advanced Study in the Behavioral Sciences in the same year--all before he had published his first book (1957).

Merton scrutinises Kuhn's footnotes and acknowledgements to chart the various people and publications Kuhn was enabled to bump into over the years which fed the ideas of his magnum opus and across which he might not have come were it not for his positioning in this system of elite academic institutions.

Unlike Popper's work of 1934 Kuhn's was timely. A growing constituency in the sociology of science was emerging; his career and this development intersect, as literature and personalities came into contact with him. Thus his crowning achievement is, in a way, the capstone of the sociology of science, offering as it does a major theoretical system for understanding science in social terms.

Critique of the Kuhn Case

Before offering some critical comments on Merton, it is only fair to stress how he, unlike some of his erstwhile followers, nowhere suggests that the ideas scientists produce can be explained away or reduced to the study of social interactions in which they engage. Indeed, that will be one of my criticisms, for it seems to me that Kuhn's ideas eerlily serve the very social formations in which they emerged. To speak sociologically, Kuhn's ideas legitimate the system from which he benefited so much. Although Merton does not reduce ideas he slights them by ommission and misses their explanatory power in the cases of Popper and Kuhn.

First a minor criticism. Although he mentions Popper's autobiography, Merton refrains from analysing it into a detailed study of Popper's career trajectory. It is after all as interesting to study what happens to someone for whom the time is not ripe, as to study he for whom it is.

Second a criticism that is not so minor. Merton has much reference to <u>first class minds</u>, <u>talent</u> and <u>brilliance</u>, words which name, presumably, properties possessed by individuals who will produce work of merit. But since such persons are identified within a system, what this actually cashes out to is their capacity to go through certain hoops in the system: such as getting high marks in university courses, or impressing senior colleagues. The system, so to speak, takes itself for granted. Merton however stresses that sooner or later recipients of its rewards must produce or the rewards process will dry up. This is contentious in itself, since the institution of tenure is sometimes awarded on promise, promise that is never fulfilled. Be that as it may, the question becomes, what is 'production'; that is to say, what is it that we expect of those talents swept into the system? Merton seems to allow that what must be produced are intellectual products: science; what the peer-review system of science considers to be a fulfillment of the expectations. What he fails to consider is that the vested interests of the system are such that whatever its creatures produce will be hailed as the major new acheivements. And here is where the self-exemplifying character of the sociology of science breaks down. Science offers something that might be naively described as conceptions of nature. These conceptions, moreover, can be reproductively tested against evidence other than that considered by their creators. What, however, can play that role of independent check on the sociology of science?

Merton is very interested in devising numerical devices to assist in independent checking. But it seems to me no accident that his radical followers claim there is no such check on their views, that scientific activity is a self-reinforcing system no more subject to the independent checks of some 'external nature' than are the speculations of the sociology of science. Why, then, were both Merton and the Edinburgh heretics stimulated by Kuhn? Not, surely, because he was a product of the system of science, anointed as it were? If that were so, then the question would be, how was Young Kuhn recognised, what were the signs? Merton's answer is to dispense with such specific signs, Kuhn is not treated as a sort of Dalai

Lama. Rather does the social system delegate to its elite institutions the task of seeking out talent, selecting its own future membership in a manner that might variously be called sleepwalking or guidance by the Hidden Hand. But what if Kuhn had not fulfilled expectations? And what if he had produced different ideas? Would a Stalinist theory of science have been accepted from him? Or suppose Kuhn, having been in contact with Popper (see below), had produced a Popperian theory. What then? This smacks of playing with those bogeyman of philosophers, counter-factual conditionals. That, however, is not the case. These are traditional sociological questions, namely, why did <u>these</u> ideas find a suitable home among <u>these</u> persons at <u>this</u> time. To neglect this question is to leave a big hole in the explanations of the lack of acceptance of Kuhn and the acceptance of Popper.

And here is the final criticism: Merton does not explore Kuhn's ideas. He mentions there is a literature critical of Kuhn and notes that Kuhn might well want to dissociate himself from some of the things done in his name. But he does not notice the most alarming fact: Kuhn explicitly abandons the notion of an external nature against which science is checked, and in the understanding of which science claims to make progress. Rather does he operate with a model of scientists organising themselves into communities around paradigms (or disciplinary matrices), serving its ends and defending it to the death. Once it is overthrown in a revolution no comparisons with the past are possible in terms of progress or depth, the scientists in effect inhabit a new world. Kuhn's theory is very odd. It postulates science as a series of hegemonies or establishments that perpetuate themselves by indoctrinating students and systematically rewriting the past in textbooks. This is justified in terms of the necessity of training people to do certain jobs called 'puzzle-solving normal science'. It is, he says, explicitly dogmatic and intolerant of questioning of fundamentals.

One cannot help seeing elements of autobiography in this. Is Kuhn describing how he, as a young man with a mind that tended to wander, was forced to discipline himself (no questioning fundamentals) to get his degrees in physics? Having done that and earned the opportunity to wander, he leaves science, enters history of science and then, lo and behold, produces a theory of science that blends many currents of fashionable ideas (Merton completely misses the Wittgensteinian influence) and offers a rationalizing legitimation of the system from which he emerged. There is no suggestion here that he refrained from biting the hand that stroked him. Quite the contrary. Kuhn is a believer in the system Merton describes. His theory, and the system it describes are, however, on Popper's principles, incoherent. Merton needs to explain the establishment's patronage and recruitment of Kuhn, and its initial failure to recruit Popper, by some such general property as their being or not being "talented". To be more precise, Merton's case study is too thin on Popper to really take up his outsider status and its transformation, which is why I will make a few remarks about it later myself. At several places in his paper Merton holds that it is Kuhn's first-class performance and talent and whatnot that was the reason senior people at Harvard discovered him, promoted his "early visibility" and strongly backed his accumulation of advantage. This is certainly the manner in which the establishment and institutions within it like Harvard see themselves, as part of a meritocracy not as merely a self-perpetuating set of institutions. Indeed, they can stoutly defend themselves against such a charge by pointing to the scope of their recruitment effort, the objectivity of their procedures and results--that member institutions of the establishment time and again gain prestigious awards, research monies, and high rankings of their graduate programmes. There is a concordance between their local forms of self-evaluation and the cosmopolitan forms of evaluation that are taken to be somewhat more objective. Merton is not unaware of the possible circularity and self-fulfilling prophesy character of such an argument, but he does not address it as the serious deficiency I think it is.

Subsequent to the publication of Henry Fairlie's journalistic article "The Establishment", and Hugh Thomas' anthologising of it, any such self-defence by an intellectual establishment has to be disingenuous. An establishment is a network of self-perpetuating institutions dedicated to maintaining their hold on power on behalf of the current and future membership, and in full conviction that, as the best and the brightest, it is to the benefit of the society as a whole for them to do so.

It need not be claimed that everyone deserving recuritment has been recruited, hence some dissatisfaction from those with talent as well as ambition without talent, is inevitable. It need not be claimed that the establishment embodies the interests of any class or party or system of ideas. On the contrary, a healthy establishment will be as flexible about ideas as it is about recruitment; it is disinterested. It is even possible for the establishment to connive at piece-meal attempts to alter its own structure. All that is required is that there is <u>some</u> hierarchical structure of leaders and followers, experts and laymen. An acknowledgement, in other words, that there must be an establishment, is sufficient for the establishment to justify itself.

Since such a minimum requirement will still result in disaffection and envy by those who want to lead and by those who hate being led, a benefit to an establishment would be an ideology that legitimates the hierarchy of expertise as a hierarchy of power. It is a striking feature of our modern age that there has grown up in the liberal tolerant democracies professions that are structured in an authoritarian and illiberal manner. Oldest among these is the law, which was a profession before medicine was even a guild. There followed medicine, certainly a guild pretending to be a profession. Just as law courts insist on laymen employing their officers, so physicians insist that the State legalise their monopoly on drugs and surgery. Last of the modern professions to evolve are scientists and teachers, which are partially overlapping. Other professions are in the process of formation.

All these professions claim to impart through their training and licensing procedures a form of expertise so essential to society that its acquisition must be supervised and restricted. Its power then is accumulated to 'protect' society. We see then that forms of social organisation have been growing that, if challenged, may require some form of general ideological legitimation if they are to explain and defend the power they wield in an egalitarian and democratic society. Kuhn supplied that for science.

Popper as a Threat

In the nineteen twenties a young man was maturing in Vienna who was to subject science and democratic society to unprecedented scrutiny--offering the thought that our fundamental understanding of each was in error and needed rectification. A deep admirer both of science and democratic society, Popper nevertheless detected authoritarian dangers in the excessive respect for expertise in both. By 1934 Popper published his scrutiny of science (in German) in a manner that insured that it would get limited attention but not be taken as the swingeing attack it was. It was published in a manner that identified him as a bright and independent member of the iconoclastic European philosophical group called the logical positivists. On this basis he was offered, as a refugee, a temporary job at Oxford. He declined this and took off for New Zealand in 1937 where he was to work out his ideas for democracy, published in 1945 to great acclaim, as The Open Society and its Enemies. The acclaim did not turn into establishment recognition, but rather into establishment disbelief and disapproval by establishment radicals whose social, political and academic credentials it challenged. It was almost twenty years more before Popper began to accumulate the academic and political honours that a Mertonian recognition of talent should have been bestowing. It is notable that Popper was invited to the Stanford Center in 1955, the same year as Kuhn, who was then 32; Popper was at this time 54.

Popper's career as outsider, by no means unknown outsider, but outsider kept at a distance from the establishment and its networks of power and influence, followed by his absorption in the establishment networks only at the very end of his career, can be explained by Merton either by the suggestion that he had less or less obvious talent than Kuhn and hence was not quickly inducted into the system of benefits and rewards; or that his talent was harder to discern; or by the accident of being a Viennese rather than an American, and of being of the generation in Europe whose academic lives were disrupted by the second European war. None of these explanations seem very convincing. Nor does a kind of randomising sample in which one simply allows establishment recruitment and succession procedures to have a certain amount of error, the capacity to miss a certain percentage of good people.

All these explanations are very week, and could be resorted to only were a better one not available. But Kuhn's career, of rapid induction at a very early age to the establishment, which made available to him all its perks long in advance of him publishing anything of any particular significance, suggests a line of explanation much more powerful than those weak ones. This explanation is that Kuhn was one of the small group of scholars being recruited to the emerging subject of the sociology of science, under the auspices of elite institutions and scientists within them, in recognition of the need of the greying science establishment to find a legitimating ideology. Both sociology of science itself, which legitimated scientific organization as science; and the specific model devised by Kuhn, functioned well to buttress the claims of the scientific profession to very much more money and power. These were not by any means the only signs. A quite systematic muddling of science with technology (both the atomic bomb and the moon landing being taken as vindicating 'science'), and aggressive campaigns against cranks (Velikovsky), superstition (astrology) and religion (creationism) are other signs. We might see these as a customs union with the powerful (technology), plus strict boundary policing to keep benefits away from non-citizens.

It may not therefore be quite as "serendipitous"--to use Merton's word--as it seems that when Kuhn began to publish his ideas they served very well to legitimate the hierarchy of expertise and its hegemony of power claimed by science in a manner that explains and legitimates Kuhn's own recruitment as described by Merton. Before expanding on this let me take up the question of why, in the nineteen fifties, the scientific establishment was looking for a new ideology? The answer is two-fold: the positivist ideology which had previously legitimated science was in intellectual ruins, although this was known only to a small circle. Secondly, the principal instrument of this ruination was in fact the work of Karl Popper, who also in his positive views threatened the establishment because of his total opposition to expertise. Kuhn was aware of both of these matters. Already in Harvard in 1950 he had attended Popper's William James lectures and seminars. He was subsequently recruited by the positivist group who edited the International Encyclopedia of Unified Science, to write a volume for them, which is the origins of his <u>The Structure of Scientific Revolutions</u>. One might then conjecture that Kuhn's theory of science serves to describe and legitimate the hegemony of those with expertise, an expertise demanded by a paradigm, and enforced by those who accept and impose it on scientific training. Thus Kuhn's theory allows that scientists develop a faculty of judgement that warrants their dismissing the works of outsiders with at best a cursory examination. He allows that science systematically rewrites its textbooks, falsifying history in the cause of training in the paradigm and engendering the puzzle-solving capacity in budding scientists. No matter how inegalitarian, illiberal, or anti-democratic the behaviour of scientific elites, Kuhn is able to show how to legitimate them by appeal to scientific success and technological success and hence to social benefit.

<u>Kuhn's Function for the Establishment</u>

We now have a crucial missing link. Kuhn was recruited as part of a pool of talent whose job was to reinforce the shaky ideology of an establishment under threat. Hence Kuhn's rapid elevation from author of an obscure monograph to a major figure in American academic life. Although the main theses of his book were refuted or severely criticised within a couple of years of its appearance, if anything attention to it grows as the criticism mounts. Its value as a legitimation charter becomes ever clearer.

That being so, how has the boat-rocking Popper been recruited to the establishment? The answer is that he has not. What has been recruited to the establishment is the Popper Legend, a Legend whose contents Popper has himself delineated, and about whose growth and influence he has been greatly exercised. When Popper says he doesn't believe in experts; that Kuhn's normal science is for him a disaster; that what is important is to be critical; it is as though no one hears, certainly not the establishment which turns its attention rather to the gadfly Feyerabend, or the vulgariser Lakatos, both easily brushed off <u>and</u> incorporated. Kuhn meanwhile goes from strength to strength, redefining his terms, acting as though his work is intact and that most criticism of it is a matter of either misunderstanding or disagreement. It is in fact neither, it is rather the detection of vagueness or inconsistencies in his ideas.

The sociology of science, then, is a self-exemplifying development, as is this paper. Kuhn's theory is a theory of science in which the role of ideas is minimised. Most of what he calls normal science is devoid of them. Scientific revolutions are rare, unwelcome, and as much changes of generation and of pedagogy as they are of ideas. Scientific education wants stability in order to know what to teach and to develop standards; it also wants success--rewards for the effort. Only upon education and standards can a prosperous profession be built. Without quite reducing science to society, Kuhn stresses that paradigms or disciplinary matrices and exemplars have only a small component of ideas. His own work is thus the ideal paradigm for the emerging sociology of science.

Ideas, however, will not go away. Moreover, despite the best efforts of both the philosophy and of the sociology of science to ignore Popper's centering of ideas, criticism, the overthrow of establishments, the democratising of society, severe checks on power, and critique of expertise, he has found, surprisingly, as Merton notes, quite a following among scientists. On Merton's model this is inexplicable. On Popper's, in which science is not a monolith, but a battleground fought over by shifting coalitions of friendly-hostile groups, it is. For if Kuhn's theory is correct the neglect of Popper is to be expected to continue. If Popper's theory is correct things are not near so bad.

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