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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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The general problem that this paper addresses is whether sociological explanations of science can dispense with the independent causal factor of ideas.[1] It will argue that these sociological explanations themselves employ ideas the development of which cannot be explained away.

INTRODUCTION

The scientific ethos was always universalistic: everything has a scientific explanation. Slowly it transpired that scientific explanation was causal, in some sense (and, of course, valid and true or else not explanation or not science). Hence, every event has a cause. The events that look particularly uncaused are inspirations, so those inspired by the scientific ethos took the bull by the horns and declared inspirations to be caused. At first it looked as if religious inspirations were the hardest to explain causally; they turned out to be the easiest, being explicable as hoaxes, or psychologically, or sociologically (functionally). What turned out to be genuinely difficult to explain were scientific inspirations. It is for this reason that the philosopher who attacked the scientific ethos in recent times—Bergson—used scientific inspiration as the paradigm that violates the scientific ethos.

More recently, the scientific ethos has been studied sociologically.

Scientific ideas themselves were by way of a crucial test case. Was the fount of determinism itself determined? Faced with this rather daunting gulp, the founding father of modern sociology of science, Robert K. Merton in his *Science and Society in the Seventeenth Century* followed his mentor Mannheim in not offering any reduction of scientific ideas to social, economic, and military-technological factors. These factors, Merton allows, can make the time ripe for an idea to be taken up or even searched out, but an idea has some sort of separate existence. However, neither in that work nor in his subsequent publications (1973, 1977) has Merton been very explicit about ideas. This may explain why there has arisen at Edinburgh a group devoted to the Strong Programme in the Sociology of Knowledge, *i.e.*, the program of going beyond Mannheim and of condemning anything less than full determinism as idealism:

> because received concepts and beliefs were routinely used to explain the actions of individual scientists, there was a tendency to idealism in the history of science, just as there is always such a tendency in the history of ideas generally. Concepts, beliefs, principles were credited with inherent potency; they were thought of as autonomous entities with power or influence over men's minds. Cultural change was even, on occasion, conceptualized as the unfolding of the inherent implications of ideas. Such a one-sided conception, which ignored the power men possess to extend, adapt, modify or reflect received ideas, was not acceptable in sociology (Barnes, p. 8).

Who is and who is not an idealist need not detain us. But clearly Merton's ambiguity about ideas has left a big hole in his theory that needs to be closed if it is not to collapse towards the Edinburgh reduction.

To close this gap, I shall proceed by way of Merton's 1977 study of the new science of the sociology of science; a text in which, once more, he says very little about the role of ideas, and, within that general topic, concentrates on his case study of the diverging careers of two men whose ideas have influenced the science of the sociology of science although, again, Merton scarcely discusses their ideas. The two men are Karl R. Popper and Thomas S. Kuhn, the latter more influential than the former.

Merton's original study of 1938 looked at the rise of natural science in the 17th Century. His new study concerns the rise of the science of the sociology of science in the period since 1938. In each case his approach is the same. He attempts to explain the rise of science by studying the social formations in which it is embedded. Such an explanation is not necessarily an objectionable reduction.

My *philosophical* thesis will be that sociology engages in illicit reduction if it tries to explain socially the truth or untruth of scientific ideas. My second, *material or sociological,* thesis will be that the ideas Kuhn and Popper put forward are essential to explain what Merton calls their differential "presence" to the sociology of science. Kuhn's ideas, it will transpire, legitimize 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 also happens to be inimical to and critical of these formations, and thereby of those ideas of Kuhn which legitimate these formations. Kuhn's ideas legitimate science's current social embodiment; Popper's undermine it. These no doubt unintended consequences of each man's *ideas* are, I will argue, integrally necessary for explaining the trajectory of their careers. The American academic and scientific Establishment, sensing these unintended consequences, has naturally preferred to embrace Kuhn and to hold Popper at a distance. An interesting question is whether this "fit" between Kuhn's ideas and Establishment needs was, as Merton has it, a serendipitous mutual discovery, or whether Kuhn sensed and articulated Establishment needs. Popper, by contrast to Kuhn, has always been aware of a normative element in his work, a normative element suspicious, and hence critical of Establishments as such. His increasing popularity with the natural science Establishment of Europe will be touched on later.

REDUCTION OF SCIENCE TO SOCIETY

At least two very different things can be meant by the rise of science: organizational success and intellectual success. Organizational success includes the founding 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, in the percentage of the GNP spent on research, in Nobel Prizes won, and so on. Intellectual success is the growth of scientific knowledge, whether measured by quantity, quality, or rate of increase. The question of reduction amounts to asking whether organizational success can be explained independently of intellectual success.

Before discussing Merton's specific study of Kuhn and Popper, we need to look at his general ideas on science and on success. Only then will we be prepared for his explanation of Kuhn's influence on, and Popper's relative neglect by, social scientists.

Merton's pioneering 1938 monograph, *Science, Technology and*

Society in Seventeenth Century England did not wholly discount ideas but was primarily a study of organizational success. He set out to test the idea that the growth of science was significantly and interestingly connected with social, political, military and, ideological matters. There was in it an implicit criticism of those historians of science who looked only at scientific ideas in their temporal succession. And one need not be a connoisseur to see immediately that it was incomparably superior to the traditional style of history of science, largely dominated, as it was, by the Great Men, Great Ideas approach. Between 1938 and 1977 the emphasis in Merton's work on organization as opposed to ideas grew stronger, so that when in 1977 he studied the rise of the science of the sociology of science in the mid-twentieth century ideas played very little role.

So far we have discussed success. But it could be argued that, both socially and intellectually, there is more to science than success. Science also embraces failure, routine, hard work. Any reduction of science to social factors that does not comprehend its quotidien and unsuccessful sides as well as its brilliant and successful sides is scarcely adequate. This, I conjecture, is where Kuhn comes in: he makes room for the mundane in the house of science.[2] Indeed, I think he makes scientific mediocrities, to change the metaphor, the salt of the scientific earth. Kuhn is like one of those war-time politicians boosting the morale of face-workers in the coal mines by telling them they are essential to the war effort because their coal heats the blast furnaces that produce the metals that go into the guns, ships and aeroplanes that are destroying the enemy. Much of the stir caused by Kuhn's 1962 monograph *The Structure of Scientific Revolutions* stemmed, I conjecture, from his effort to specify what science was, not in terms of intellectual success, but in terms of current social organization. His thesis was that science is and can only be produced within a scientific community, there is no Robinson Crusoe science. Furthermore, a scientific community comes into being only when individuals and institutions subordinate themselves to the authority of 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 inextricable mixture (p. 108).

Popper also, as we shall see, proposed a view that excludes

Robinson Crusoe science and gave a crucial role to the scientific community. But his was not a view that gave comfort either to mediocrities or to the established élites who presided over the system. Hobbesian sociologists see the fundamental problem of sociology as "How is social order possible?" In Kuhn's scheme the paradigm and the guardians of the paradigm are what the community depends upon for its very existence. Popper's social and political philosophy is rather different. Communities are not created solely by subordination to authority, to a Hobbesian sovereign paradigm, which must be obeyed until it is overthrown.[3] Instead, Popper sees the fundamental sociological problem as, "How can the social order be reformed?" He urges all scientists to be Trotskyites, apostles of permanent revolution, critical of intellectual as well as social authority. The aim that unites the scientific community is not fear of social and intellectual chaos but devotion to the ideal of truth. Instead of society explaining science, intellectual values explain social formation.

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, and Kuhn's paradigms into an over-arching attempt to reduce science to society. What are these reductions reducing? Answer: the traditional intellectualist account of science as a body of ideas, inspirations. Other values traditionally ascribed to this scientific body of knowledge besides truth include: certain, based in experience, reliable, predictability, cumulative, authoritative. Kuhn's social reduction preserves only some of these values as distinctive of science. Truth and certainty he abandons in the course of fitting revolutionary change into his characterization of science. The Edinburgh school go one further: all the traditional intellectual values of science are mere epiphenomena: not they but the social functions of science make it what it is. Science becomes identical with its social embodiment. True, they allow, the social formations of science, like all social formations, have some unique features—and also some features typical of social formations in general. Both unique and typical features are social; ideas, values are epiphenomena. Thus Merton's modest program for examining the social aspects of science can, partly as a result of the ambiguities of its own emphasis, be grotesquely transformed. The sociology of science, which for Merton is a branch of the sociology of knowledge, which is a branch of sociology, can be transmogrified by the Strong Programme into a totalising science—the master science that catches everything in its embrace, including itself (it is declared to be "reflexive").

EXPLANATION WITHOUT REDUCTION

Contrasted to social reductions of science is the long intellectualist tradition that places truth and certainty at the center, 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 such as 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 programs is that it seems to have broken 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 claims to truth, certainty, basedness in experience, reliability, cumulativeness, predictions and authoritativeness were taken as deceptive. Whatever replaced it, there was no reason that that should not in its turn to be replaced. And if science is identified with Newton, then science comes to seem impermanent.

Intellectualism need not oppose efforts, such 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. What intellectualism must oppose is the claim that science is *no more than* another, special, social formation. Without denying that science *is* another, special, social formation, intellectualism makes the claim that what explains its specialness is its product. That is to say, science is the social formation that produces scientific knowledge (ideas, inspirations). 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 or proximity to truth of the ideas.

More than thirty years before Kuhn's book was published, a young Viennese was trying to fathom the problems of the intellectualist tradition. Reflecting on Kant's view of scientific knowledge and his implicit identification of it with Newton, Popper sought a way in which he could solve two problems.[4] The first was to recast the intellectualist tradition to make sense of Einstein's overthrow of Newton. This he did by making falsification, or the overthrow of ideas, the motor of that tradition. The second problem was to discriminate the progressive character of Einstein's achievement from all the many pseudo-achievements, which filled the Austro-Hungarian intellectual atmosphere. This he also did with falsification, by making it a criterion of genuine achievement, and hence in principle desirable.

A substitute teacher and outsider, Popper tried hard to get the attention of the then ruling philosophy of science establishment, called the *logical positivists.*. If one is to judge by such fashionable measures as 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, then it looks as though his ideas were not taken very seriously. Indeed, despite his criticism, which was later judged deadly, the logical positivist movement continued to flourish, reaching its apogee—in science indicator terms—about 1960 or perhaps a bit earlier.

Like Kuhn, Popper wanted to explain scientific success; unlike Kuhn he wanted to do it without abandoning the intellectualist tradition. Not unmindful of the fact that science was a social institution—indeed, he was the first philosopher of science to integrate this point into his philosophy of science, a couple of years before Merton—he yet wanted to claim that its very special aim (namely, the truth about nature) was pursued within a very special social structure. Success was to be measured not by social formations, nor by indicators, prizes, or what not, and certainly not by dogmatic imposition of any ruling idea; rather, it consisted in refutable assertions about the world and such social formations as would foster their production and refutation. Scientific success in Popper's view 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 in 1977 ("The Sociology of Science: An Episodic Memoir") made the emergence of the science of the sociology of science a case study for the emergence of science in general and hence a case study in tracing the connections between scientific success and social setting. Merton's own strategic position as a facilitator of the emergence both of the sociology of science and of Kuhn makes his participant observer's report a fine-grained and invaluable document.

Merton's 1938 monograph 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 had been lacking was a theory to undergird the sociology of science. To be more precise what was lacking was the coincidence of a theory and the time being ripe.

Two philosophers of science, Merton says, Popper and Kuhn, offered theories of a connection between science and society. Although the presence of both looms large today, Popper, despite publishing nearly thirty years before Kuhn, is less often cited. Popper's 1934 book was widely reviewed but noticed mainly by philosophers of science, being little remarked in the literature of sociology and the history of science.[5]

> In (forty-five journals of) philosophy, Popper is cited some one-and-a-half times as often as Kuhn while Kuhn is cited some one-and-a-half times as often as Popper in the ninety-nine journals of sociology and the seven journals in the history of science covered in the 1973 and 1974 *Social Science Citation Index* (Merton 1977, p. 69)

Thirty years after his first book, with the sociology of science already growing, Popper's 1963 and 1972 works become the route for the eventual osmosis of his ideas 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 would seem 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 have nothing to explain, and no audience to appreciate it.

In criticism of Merton it should be said that Popper's ideas were in fact not taken up by philosophers of science except in the Pickwickian sense that they were more or less systematically ignored. Secondly, Popper's ideas stipulate that data cannot precede theory. Since Popper's theory denies the time-is-ripe doctrine the problem of why his ideas did not stimulate the growth of the social study of science much earlier remains. To this day he is the inspiration of very few sociologists of science.

Merton is on surer ground with his study of Kuhn, which is based on his insider knowledge. The story according to Merton is

that Kuhn came along at the right time, indeed was brought along by the Establishment 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 single out a person like Kuhn, offer him opportunities to follow new directions of thought, and, as his work is recognized 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 already heaped upon him many of the privileges and advantages available in American academic life.

Merton's story explains the emergence of a key figure in the science of the sociology of science. The science of social explanations is socially explained. Back in 1938 the problem Merton had tackled was to explain the rise of science in 17th Century society. His thesis was that "the socially patterned interests, motivations and behavior 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 interdependencies Merton explored were religious (especially the Puritan value system), economic (especially mining and transportation), 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 autonomous case for pure science evolved out of the derivative case for applied science" (xii).

This seems obvious enough. There is a wide constituency able to judge an applied 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, which attract and reward talent. This enables those on the frontiers of knowledge to engage in interactions far beyond their specialty. If thereby they enrich their field, then it has been serendipitous. This is an expectation, not a demand. Kuhn, in Merton's example, was both slow and reluctant to publish (91). For his legitimation, however, results and reactions were needed: "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 United States 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, and in the same year was offered both a Guggenheim Fellowship and a Fellowship at the Center for Advanced Study in the Behavioral Sciences—all before he had published his first book (1957).

Merton scrutinizes Kuhn's footnotes and acknowledgements to chart the various people and publications Kuhn's "cumulative advantages" enabled him to bump into, and which fed the ideas of his magnum opus; people and publications across which he might not have come were it not for his positioning in the system of elite academic institutions.

Unlike Popper's work of 1934 Kuhn's of 1962 was timely: a constituency was awaiting it, a growing constituency in the sociology of science. Kuhn's career and this developing constituency intersected as the growing literature and numbers of practitioners came into contact with him. Thus, his crowning achievement was, in a way, the capstone of the sociology of science, offering as it did a major theoretical system for understanding science in social terms.

CRITIQUE OF THE KUHN CASE STUDY

Before offering some critical comments on Merton, it is only fair to stress again that he, unlike some of his intellectual heirs, nowhere actually says that the ideas scientists produce can be explained away or reduced to the study of the social formations to which they connect. Indeed, that will be one of my criticisms, for it seems to me that Kuhn's ideas eerily serve the very social formations amongst which they emerged. I do not say that the social formation caused Kuhn to think the ideas, as Durkheim would. Rather, I suggest that we recall the functionalist insight that the reception of ideas may have to do with their fitting or not fitting into pre-existing social formations.

To speak sociologically, Kuhn's ideas legitimate the system from which he benefited so much; a system moreover, that was relatively new and sorely in need of a legitimating ideology. Although Merton does not reduce ideas to social factors, he does slight ideas by ommission, by not stating his valuation of ideas as ideas. He thus overlooks the explanatory power of Popper and Kuhn's ideas for his problem: the congeniality and uncongeniality of Popper and Kuhn to sections of the American academic élite. I expand this in detailed criticism of Merton.

First, a minor point. Although he mentions Popper's autobiography (1976), Merton refrains from analysing it into a detailed study of Popper's career trajectory. It is, after all, no less sociologically interesting to study what happens to someone for whom the time is not ripe, than to study someone for whom it is.

Next, a criticism that is not so minor. Merton makes much reference to *first class minds, talent* and *brilliance.* These are words which name, presumably, properties possessed by individuals that cause them to produce work of merit. They are independent variables. But since individuals possessed of these properties are identified within a system, what this actually cashes out to is that these individuals have the capacity to jump through certain hoops set up by the system: such as getting high marks in university courses, or impressing senior colleagues. They are dependent variables. The system, so to speak, takes itself for granted; is itself the independent variable. Merton however stresses that sooner or later recipients of the rewards of the system must "produce" or the rewards process will dry up. This is contentious in itself, since tenure is sometimes awarded on promise, particularly Oxbridge and the Ivy League, promise that is never fulfilled—particularly in Oxbridge and the Ivy League. Be that as it may, the question becomes, what is "production," that is to say, what is it that we expect those talents swept into the system to produce? Merton seems to allow that what must be produced are intellectual products: science, i.e., what the peer-review system of science considers to be a fulfillment of its expectations. He fails to consider that the vested interests of the system are such that whatever its creatures produce will be hailed as major new achievements. 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 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 n independent checking; but Merton's radical strong program heirs laim there is no such check on their views, that scientific activity is self-reinforcing system no more subject to the independent checks f some "external nature" than are the speculations of the sociology f science. The institutions of science and of Azande witchcraft are hus on a sociological par.[6] Why, then, were both Merton and the trong Programme stimulated by Kuhn? Not, surely, because he was product of the system, science-anointed as it were? For, were that o, then the question would be, how was young Kuhn recognized, hat were the signs? Merton's answer is to dispense with such specific igns: Kuhn was not selected like a Dalai Lama, where the transmi- rated soul is sought out in a child. Rather, the social system delegates o its élite institutions the task of seeking out talent, selecting its own uture leadership in a manner that might variously be called sleep- valking or guidance by the Hidden Hand. But what if Kuhn had not ulfilled expectations? And what if he had produced different ideas? Vould a Stalinist theory of science have been accepted from him? r suppose Kuhn, having once been contaminated by contact with opper (see below), had produced a Popperian anti-Establishment heory. What then? This smacks of playing with those bogeymen of hilosophers, subjunctive and counter-factual conditionals. That, how- ver, is not the case. These are traditional sociological questions, amely, why did *these* ideas find a suitable home among *these* persons nd institutions at *this* time. To neglect these questions is to leave a ig hole in the explanations of the lack of acceptance of Popper and he acceptance of Kuhn.

Moreover, neglect of these questions also casts doubt on some f Merton's detail. Kuhn's success was not unclouded. His quest for espectability among philosophers did not go smoothly either at Berke- ey, or in the reception of his book, some reactions to which were vithering. His second thoughts and replies to critics were widely held o be weak and defensive. His output of graduate students has been mall, and he seems recently to have thrown in his lot with the hilosophy of language.

Parallel to his slightly too rosy account of Kuhn's career, Merton lso quite underestimates "the Popperian presence," which is required o explain a great deal of what is happening in philosophy and the ocial sciences—even though Popper is rarely mentioned.

And now to the meatiest criticism: Merton does not care to xplore Kuhn's ideas. He mentions there is a literature critical of uhn and notes that Kuhn might well want to dissociate himself from ome of the things done in his name. But he overlooks the most larming and conspicuous 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, Kuhn operates with a model of scientists organizing themselves into communities of experts rallying round a paradigm (or disciplinary matrix), serving its ends and defending it to the death (theirs or its). Once a paradigm 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, a world governed, in another echo of Hobbesian sovereign power, by new laws and procedures. Kuhn's theory is very odd. It postulates science as a series of hegemonies or establishments that perpetuate themselves by indoctrinating students and systematically obliterating the past in textbooks that offer students puzzle-solving tasks. This 1984-type exercise is justified in terms of the (social?) necessity of training people to perform these tasks; this is called "puzzle-solving normal science." It is, he explicitly says, dogmatic and intolerant of questioning of fundamentals. Society rewards scientists with high status and money for this blinkered performance.

The social philosophy is hard to fathom. Hobbes feared the absence of authority, anarchy. But he was concerned with the whole social Leviathan, not with the particular social formations within it. Kuhn gives us no reason to think he is not a liberal and a democrat, a believer, in general, in freedom of thought. Yet his argument is that when it comes to science if we want to make progress (note his contradiction) we have to suppress anarchy and impose intellectual discipline. The power of discipline gets things done, whether it is the Manhattan Project or the trains running on time.

Dare one see elements of autobiography in all this? Is Kuhn describing how he, as a young man with a mind that tended to wander, was forced to discipline himself (no questioning of 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 also completely misses the Wittgensteinian veneer) and offers a rationalizing legitimation of the system from which he emerged which simultaneously explains how and why it works and why a free spirit like Kuhn left it. There is no suggestion in Merton that Kuhn carefully refrained from biting the hand that had stroked him. Quite the contrary: Merton treats Kuhn as a naive believer in the system Merton describes. His theory and the system it describes are, however, quite incoherent. It is easy to see on Popper's principles how obviously incoherent it is; but its incoherence is there—one way or another.

Merton needs to explain the establishment's patronage and recruitment of Kuhn, and its initial failure to recruit or even take Popper seriously, by some such general property as their being or not being "talented." To be more precise, Popper may serve as a test case to evaluate Merton's test case as well as his theory. And, clearly, Merton's case study is too thin on Popper to explain Popper's outsider status and his later transformation into the leading philosopher of science he now is. This is why I will make a few remarks about all this later. Here let me round off my critique of Merton's case study of Kuhn and Popper.

At several places in his 1977 paper Merton holds that it is Kuhn's first-class performance and talent that led senior people at Harvard to discover him, promote his "early visibility" and strongly back his accumulation of advantage. This is certainly the manner of self-perception of the establishment and institutions within it; Harvard and other élite institutions see themselves as part of a meritocracy not as merely a self-perpetuating set of institutions. Indeed, they can stoutly defend themselves against a charge of mere self-perpetuation by pointing to the scope of their recruiting 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 programs. 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 fact that such an argument is possibly circular and thus a self-fulfilling prophecy; but he does not address it as the possibly serious deficiency I think it is.

Subsequent to the publication of Henry Fairlie's journalistic article "The Establishment," and Hugh Thomas' anthologizing of it, any such self-defense by an intellectual establishment has to be disingenuous. An establishment is a network of self-perpetuating institutions dedicated to maintaining its hold on power on behalf of the current and future membership, yet in full conviction that, as the best and the brightest, it is to the benefit of the society as a whole for it to do so. So strong is the rationale that many members of the establishment are unaware of themselves as such, and even deny its existence.

The Harvard defense need not claim that everyone deserving recruitment has been recruited; so it can allow for some dissatisfaction from those with talent as well as that from those with ambition but without talent. 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 will be disinterested. It is even possible for the establishment to connive at piece-meal attempts to alter its own structure. All that is required for its legitimation is the admission that there has to be *some* hierarchical structure of leaders and followers, experts and laymen, if society is to gain the benefits it wants. An acknowledgement, in other words, that there must be an establishment, is sufficient for the establishment to justify itself by more than its own self-interest. For all of this Merton has to draw on Kuhn who argued that in the absence of an established paradigm there is no science.

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, it would benefit an establishment to have 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 outspokenly authoritarian and illiberal manner. Oldest among these is the law, which was a profession before medicine was even a guild. There followed medicine, obviously still a guild, though pretending to be a profession. Just as law courts insist on laymen employing their officers, so physicians insist that the State legalize their monopoly on drugs and on surgery. Last of the modern professions to evolve are scientists and teachers, which are partially overlapping. Other professions have formed into guilds, notably engineers and accountants, and still others 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 organization 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 and in so doing papered over the cracks in Merton's theory.

POPPER AS A THREAT TO THE ESTABLISHMENT

Although Merton tells us the citation ratio of Popper to Kuhn is over the range of 1.5:1 positive and negative, in his 1977 paper he devotes three pages to Popper and forty-two to Kuhn, a ratio of 1:14. Such are the rewards of not being ahead of your time. Let me then redress the balance of objective indicators somewhat by offering a few more pages on Popper and his career as part of the process of showing ow important his idea is that ideas (including, for my money, his deas) are important.

In the nineteen twenties a young man was maturing in Vienna who was to subject science and democratic society to unprecedented crutiny—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 had published his scrutiny of science (in German) in a manner that insured that it would get limited attention but not be understood as the swingeing attack it was. *Logik der Forschung* was published in a series edited by the positivist Schlick. This falsely identified Popper to the intellectual public 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 Cambridge. He declined, and took a post in New Zealand in 1937 where he was to work out his ideas for democracy, published in 1945 to great if delayed 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 political radicals whose social, political, and academic credentials it challenged. It was almost twenty years more before Popper began to accumulate the academic and political "advantage" that a Mertonian recognition of "talent" should have been bestowing. It is notable that Popper was invited to the new Stanford Center for Advanced Study in the Behavioral Sciences in 1956–57, a year later than Kuhn, who was then 32; Popper was at this time 54. Kuhn declined, Popper accepted.

At the end of the war Popper came to The London School of Economics where he stayed until his retirement. Popper's career as outsider, by no means unknown outsider, but outsider kept at a distance from the intellectual establishment and its networks of power and influence, followed by his absorption in the British establishment networks only at the very end of his career, and his continuing exclusion from American establishment networks can be explained by Merton either by the suggestion that Popper had less 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.[7] None of these explanations works, especially when the careers of other Viennese are looked at. Equally poor is explanation by randomizing 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. Popper's exclusion continued after he was acknowledged as good.

All these explanations are very weak, 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 these 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 élite institutions and scientists within them, in recognition of the need of the greying science establishment to find a legitimating ideology. Indeed, the very absence from the social formations of a sociology of science group, yet the strongly felt need for a justification to the public of science's prestige and wealth, facilitated Kuhn's recruitment to take place outside of and prior to the growth of sociology of science. Both sociology of science itself, which legitimized scientific organization as science; and the specific model of science devised by Kuhn; functioned well to buttress the claims of the scientific profession to ever more money and power. These were not by any means the only signs that the establishment was defending itself. 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 "serendipitous," to use Merton's word, since it seems that when Kuhn began to publish his ideas, they served very well to legitimize 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. Had Kuhn published his *Structure* in 1938, and had it been a standard text in 1950, there would have been no need to recruit anyone. Fleck had in fact published in 1935 and Polanyi in 1958, the one too soon, the other too late, to become the standard text book.

To explain this, we have to explain why it was that in the post-war period the scientific Establishment, especially in America, felt an increasingly urgent need for an ideology.

The first thing to note is that this establishment itself, as well as its money and power, were new phenomena. The growth of R and D during and after the war had catapulted a new class of cientists—normal scientists Kuhn calls them, though their normality ; very recent—into the structure of western society. This class had n interest in perpetuating and increasing its power when the Cold Var created a situation in which further heavy investment in research nd development for military purposes became imperative. The Rusian atomic and hydrogen bombs, and their launch of Sputnik acentuated the already strong tendency for the richest nation in the vorld to tackle problems by throwing money at them. Thus was the ituation created for the mammoth growth of the military-industrialcientific complex, a complex that faithfully obeys the theories of ureaucracy of both Max Weber and C. Northcote Parkinson.[8]

This sheer growth in scale itself made the previously available deologies for science inadequate. Very little is needed to legitimize he paper and pencils required by a Swiss patent clerk. In truth, the prevalent ideology of the inter-war years—logical positivism—fitted Einstein's work rather poorly. It fitted the mixed technical and scientific efforts of the Manhattan Project much better, and the behaviorist psychology applied to troop morale, home and overseas propaganda, better still. Unfortunately, the cognoscenti knew, although none of them would consciously allow, that logical positivism's intellectual foundations were in ruins, and the collapse of the whole building was only a matter of time. A darker secret, one still not acknowledged in public, was that the one person primarily responsible for this ruin more than any other was Karl Popper.[9]

Any thought that Popper could be the needed ideologist of the new situation was scotched by two considerations. The first was that he had committed the thoughtcrime of attacking logical positivism, a crime not mitigated by the attack being successful. The scientific establishment and its captive audience of normal scientists had all been raised in the heady atmosphere of logical positivism and were not happy to lose their toys (some have yet to admit that they are lost). Bad enough to have to seek a replacement, but to go for it to the very instrument of this fate, Popper, was altogether asking too much. Secondly, Popper's own ideas were quite unsuitable, since, so far from legitimizing an establishment and its perquisites, Popper threw doubt on all expertise and made challenging establishments integral to the scientific endeavor.

It is my hypothesis that Kuhn was aware of all these matters although not perhaps consciously.

Already at 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 *The Structure of Scientific Revolutions*. One might then conjecture that Kuhn's theory of science serves to describe and legitimize 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, ignoring and 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 antidemocratic the behavior of scientific elites, Kuhn is able to show openly and in good conscience how to legitimize them by appeal to scientific success and technological success and hence to the benefit of society at large.

KUHN'S FUNCTION FOR THE ESTABLISHMENT

We still need a crucial missing link. Consider the facts. Kuhn was recruited as part of a pool of talent whose job was to reinforce the collapsing ideology of an insecure establishment. Hence Kuhn's rapid elevation from author of an obscure (and, some thought, iconoclastic) monograph to a major figure in American academic life. Although the main theses of his book were severely criticized not to say refuted 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.

The facts being these, the missing link needed is this: 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.[10] 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 vulgarizer Lakatos, both easily brushed off *and* 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 minor disagreement. It is in fact neither, it is rather the detection of vagueness or inconsistencies in his basic ideas.

The sociology of science, then, is a self-exemplifying developnent, as in the present paper. Kuhn's theory is a theory of science n which the role of ideas is minimized. Most of what he calls normal cience is devoid of them. Scientific revolutions are rare, unwelcome, nd as much changes of generation and of pedagogy as they are of deas. Scientific education wants stability in order to know what to each and to develop standards; it also wants success—rewards for he 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 both of the philosophy and of the sociology of science to ignore Popper's centering of ideas, criticism, the overthrow of establishments, the democratizing of society, severe checks on power, and critique of expertise, he has found, surprisingly, as Merton notes, quite a following among *bona fide* professional scientists, especially in Europe. 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 need not be, and perhaps are not, near so bad.

NOTES

1. Many thanks to J. Agassi, W. W. Bartley, III, Donald Campbell and Gerard Radnitzky for their critical comments on an earlier version of this paper. The usual warning that this in no way spreads to them responsibility for the upshot, should be taken with unusual seriousness.
2. The metaphor of the house of science comes from Oppenheimer (1954).
3. The Hobbes parallel comes from Geller (1982).
4. K. R. Popper (1976).
5. Popper's claim, that Merton repeats, that *Logik der Forschung* gained a modest amount of attention could, as far as philosophers of science are concerned, be shown, by citation measure, to be false. Popper contrasts this early reception with the paucity and poor quality of the reviews of the translation, published in 1959. Reviews may be poor indicators.

Citation of *The Logic of Scientific Discovery* shows it to be very well known indeed.

6. Evans-Pritchard (1937) showed us how any system of ideas can be self-critical, provided the criticism is internal to the system. It is criticism *of the system* that marks science.
7. This is close to the view of my commentator, Michael Cavanaugh, whom I thank for his critique.
8. Weber (1968); Parkinson (1958). Not to mention "Murphy."
9. See "Who Killed Logical Positivism" in Popper (1976).
10. The great bulk of Popper citations, for example, refer not to Popper but to the Popper Legend.

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