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One of the greatest monuments to the human intellect is the Babylonian Talmud, compiled, we are told, about the year 500. In it the two chief compilers congratulate themselves, with understandable pride, saying, we are no mere woodcutters in the swamp. The chief compiler is characterized elsewhere in that vast work as a master dialectician—evidently with his consent. He was, it is said, one who would raise a whole palm-tree and then take an axe to it. The question, what good does it do to put all the labor required into the act, what is gained by raising a palm-tree and the cutting it down, this question could not possibly occur to him. He knew that preoccupation with the law is the highest form of activity, the best form of life. The idea of progress was alien to him, if not actually distasteful. Echoing Plato, the Talmud says, if the former generations were angels we, the later ones, are merely human; if the former were human, we the later ones are asses. Yet, clearly, these humans or asses saw in the corpus that was compiled an achievement. How does one reconcile the idea of progress with a static system of thought? I do not know. I do not think it possible.

Let me stress, the question is not, is the compilation of this or that scholarly work progress? The question is, does a certain philosophy permit progress at all? This question is a bit vague. Clearly, the Talmudists denied the possibility of progress in some sense yet affirmed the possibility of progress in another sense. Moreover, both views are stated quite emphatically. Nor is there a need to declare that a contradiction is present in this. Clearly, before one could judge matters one needs to be told in what sense the Talmudists saw progress as impossible and in what sense they saw progress as possible and even present. Yet it is not easy to say; at least I find it hard to say. Clearly, in the sense in which I can elucidate matters somehow, I may employ the jargon popular today among both philosophers of science and historians of science, a jargon usually ascribed to Thomas S. Kuhn. But I wish to stress, that I am using here a popular jargon, not Kuhn's own views. I have heard Kuhn often say he is misrepresented, and I would rather not represent him at all just because of that recurrent complaint of his. And I also wish to stress that I am using the jargon loosely since it has but a loose meaning—which fully accords with Kuhn's recurrent complaint.

Well, then. The Talmudic scholars were operating within a paradigm. They denied that progress is possible in the sense that they deemed the paradigm absolute, contrary to contemporary views of paradigms as changing from epoch to epoch. The reason the Talmudists saw the paradigm as unalterable is, of course, their attribution of it to divine origin. They felt, quite rightly in my opinion, that without such divine attribution, adherence to a paradigm becomes a frivolous affair. One may, of course, adhere to a paradigm for pragmatic purposes. This possibility is, historically, the most popular second option. The two options, incidentally, are known in the philosophic literature by a number of catch-phrases, such as nature and convention, absolutism and relativism, dogmatism and pragmatism.

What is common to absolutism and relativism is the idea that progress remains within a paradigm. What they differ about is the number of paradigms available, whether it is one or many, to use anothher catch-phrase.

As historians we may wonder, what offers a historian more scope, the one or the many. The attraction of the many is what comes forth first: many paradigms present many epochs; from the very start the historical dimension is introduced. Yet this is the historian's view. The philosopher's view is quite the contrary: the one is the resting place, the home our spirit yearns for; the many only disturb us by placing before us an insoluble problem of choice: which of the many should I choose? The historian does not choose. For the historian, as a fact, Tom was a mechanist and Dick a vitalist, Reuben a plenist and Simon a vacuist. It is given; it is no problem of choice.

Some great thinkers have tried to make use of this fact, to approach philosophy historically. Prominent among them is Martin Buber who claimed that the choice of a religious paradigm is a matter of historical fact. Following him Michael Polanyi declared that the choice of a scientific paradigm is also a matter of historical fact. Polanyi did not mention Buber, as far as I know, but he used Buber's unmistakable idiom. Anyway, their idea is that we can choose only those conventions or paradigms which exist, which exist as cultures or as cultural communities. Moreover, they said, you need not like any existing option, since you can alter it, but you can alter it only from within, i.e., by endorsing its paradigm to begin with.

When comparing the Talmudic scholars with Martin Buber we may see a remarkable shift in the very concept of the paradigm itself. For, the Talmudists declared as central an unchangeable core doctrine, the divine word, and change and progress as occurring only in its application, interpretation, and further application. For Buber, and for Polanyi, as long as the socio-cultural base maintains continuity, all is well; the doctrinal changes are matters of the community in question, and no outside philosopher, certainly not a Buber or a Polanyi, has the authority to dictate any rule on that matter.

This is, briefly, a cop-out. The social background is considered primary and the culture it carries is used as a mere excuse for our study of it, or a rationale for our study. And this erroneous order of priorities limits one's very ability to understand the culture whose social background one carefully analyses. For, living societies face problems, problems rooted in the social background but not soluble by looking at the background. Problems beset any active member of any living community, be that community the Catholic Church, the local Jewish synagogue or the International Society for the History of Science. The problems are varied. Some are soluble within the paradigm in a resonable manner, some not; is reform then the order of the day? Neither Buber nor Polanyi has an answer. Neither has a criterion. They even explicitly declare that no criterion exists but that the leadership, the spiritual and intellectual leadership, take responsibility and act with no criterion to guide them. Polanyi knowns that leaders can err. They may then lose their position at the helm or, much worse, they might take the whole ship to a reef. All this is true, but quite unhelpful to the leaders. The leaders are not averse to being offered advice. They are, obviously, averse to having advice rammed down their throat, but, equally obviously, not amused when advised to act without the aid of advice.

Perhaps I am too rash to say they are willing to listen to advice before they make up their own mind. As a budding philosopher studying science and moving in the company of normal scientists I was told many a time, and in a blunt manner not meant to spare my feelings, that philosophers of science are individuals trading in advice that is both unasked for and inherently useless. But what they meant thereby to affirm, was their status as normal scientists, as individuals who take the paradigm of their scientific community as given, and as individuals happy to perform tasks which are valued by their community, no questions asked.

There is something terribly important shared by the ancient Talmudic scholars and today's scientists. Something which I consider the root of their peace with themselves and profound inner sense of well-being. It is the obviousness of their choice. The individuals in question know that they had a choice of a paradigm. Many Talmudic scholars were steeped in the local culture within which they lived in exile, yet they never stopped to consider their choice: the moral and intellectual superiority of their Jewish paradigm over the local culture hardly invited articulation. The same holds for my scientific friends. They could become religious thinkers or otherwise alter profession, but they knew they had a calling.

Yet, I must repeat, all this holds for the rank-and-file, for the normal scientist or the normal Talmudist. The leading ones faced more basic problems and sought criteria by which to face them. One of the very earliest Talmudist leaders, Raban Gamliel, decided to rely on scientifically calculated calendars in preference to traditional ways of determining dates by observing the new moon. He was challenged. He used his authority and demanded of his challenger to publicly accept his own-scientific-ruling, in preference to the challenger's traditional alternative. How did Raban Gamliel know that the heathen scientific calendar is reliable? How did St. Robert Cardinal Bellarmine know that the Copernican calendar is more reliable than the Julian calendar instituted by Raban Gamliel? (For, the Gregorian calendar is but a variant, well within the Julian paradigm.)

There are the obvious questions to ask anyone who is not quite content to be a normal member of a given intellectual community. Thomas S. Kuhn is right about one point in this context: he notices that at time this question is not pressing, at times it is. And when the pressing need for a change is felt and the leadership successfully effects the change, then the sooner the rank-and-file endorse it, the more easily they can pretend that nothing much has happened. That is to say, Kuhn views paradigm shifts from the viewpoint of the obedient rank-and-file. For the obedient rank-and-file are content to leave the big tough questions to others and merely follow in their wake. They want no advice from outsiders, they want instructions from the leaders. But not all rank-and-file are as obedient as he describes. He wrote about the Copernican revolution and he wrote about the quantum revolution and in neither case did he notice the enormous qualms and perplexities which even simple and very normal researchers may suffer. When you are perplexed, says the great *Guide to the Perplexed*, go and consult an acknowledged wise person. How do I know whether the acknowledged wise is wise? Once you ask this question, you are obedient normal rank-and-file no longer. The obedient normal rank-and-file have no trouble knowing this. You go to a convention and snoop around. In no time you find out. I think this is true. I think Maimonides' *Guide to the Perplexed* and Thomas S. Kuhn's *The Essential Tension* are fundamentally in accord. And, to speak from personal experience, life is much harder for one who rejects their counsel—for one who is unable to accept as truly wise the person whom the community judges to be wise.

So much for background information. I apologize for my lengthy and round-about presentation, but only now do I feel comfortable—somewhat comfortable—coming to my point. Twenty odd years ago I wrote my *Towards an Historiography of Science.* It was published in 1963 and reissued in 1967. It is still popular—perhaps my biggest public success to date. I was and still am very happy with its reception. Yet now, having to look back on it, I feel I should say what was found wrong with it and what I find wrong with its reception and what I do not like about its message.

My brief work presented the two paradigms concerning the writing of the history of the physical sciences, rejected them and proposed that the views of Karl Popper and some of the work of Alexandre Koyré offer a better paradigm. Most readers have concluded that I endorse Popper's paradigm with no qualification. Yet in that work I hint that I think I have a better paradigm, one which weds some ideas of my teacher Popper with ideas of Emile Meyerson and other aspects of the work of his already mentioned august disciple Alexandre Koyré. I have expouned tis paradigm elsewhere—in my *Science in Flux*, for example.

Some historians of science dismissed my *Towards an Historiography of Science* as small fry, either because they followed a paradigm I was rejecting or because I was an outsider. Also some critics who said kind and appreciative words about it criticized it from the viewpoint of a given paradigm—for example Nicholas Rescher and Edwin G. Boring. I will not respond to this, since the quarrel between paradigms is an elaborate, wearisome matter. I should also mention that some of those who said kind and appreciative words about my book complained that I was an outsider, particularly Thomas S. Kuhn and Derek J. deSolla Price. And, I suppose, I am an outsider.

What characterizes an outsider, what is it to be an outsider, and why does it matter? These questions make sense. I shall show this, discuss these questions, apply them to the place of my work in the literature and leave it for you to draw any conclusions you think appropriate.

That the question makes sense I have already argued. Reforms, to repeat, are within a culture or a cultural community maintaining a certain social continuity. The social continuity, so goes the popular prejudice, is maintained by the society in question sharing a certain paradigm—sharing a certain prejudice, I will say. To remain both flexible and prejudiced the community needs an intelligent and a strong leadership. The leadership, then, and only the leadership, prescribes a paradigm-shift. Query: what if an outsider offers a paradigm shift? Without comparing myself to other outsiders, and for mere presentational purposes, it may be advisable to take the biggest examples around. Moses the law-giver is an example, as is Einstein the patent-tester. For more examples one can use Collin Wilson's *The Outsider*, though it is a cheap romantic essay, once extremely popular and now all but forgotten. Anyway, the usual way an outsider's views become incorporated is for them to be endorsed by the current leaders. Aaron endorsed Moses, Max Planck endorsed Albert Einstein. Smaller examples are more abundant but less useful as examples. Myself, I am no example at all. I was not endorsed by the leadership, and, when my dissatisfaction with my book is presented later on, my satisfaction with this fact will be apparent enough.

Why, then, the complaint that I am an outsider? If my views are useful and endorsed by the leadership or useless and rejected by the leadership, then there is no problem. Yet neither is the case. Why? I do not know, but I have a conjecture. The field of the history of science in general and of the history of the physical sciences in particular has no adequate leadership and no recognized leadership. True, there are the trappings of agreement about common and shared prejudices, trappings of a totem pole, of territorial claims respected by others, and so on. Yet all this is so unsatisfactory that it can be disregarded.

Unsatisfactory to the rank-and-file who need leaders, I mean; not to those who would rather get lost without a leader than follow one. It is a historical fact, easily ascertainable, that the field is still dominated by diverse paradigms, that a few authors are purists who stick consistently to the paradigm of their choice, and that a few write with disregard for all paradigms. Of course, those who follow one paradigm or no paradigm have the virtue of consistency, those who are eclectic may come up with exciting finds and thus invite others to clean their works of their inconsistency. And, on the whole, the Koyré-Popper paradigm I sided with is by now a reasonable contender in this multi-paradigm field, and I am prould of my little share in this development. But, not to lose my sense of proportion, let me observe that the Kuhnian paradigm, not discussed in my *Historiography*, since it was presented to the world when that work was written, is more popular though not dominant: there is no dominant paradigm in the field.

Well, then, to my question, what characterizes an outsider, what makes one an outsider, and why does it matter? I think now the answer gets clear. In particular, the fact that I have published some books and research papers in the field of the history of science proper need not qualify me as an insider. Again I should mention a venerable precedent. In his late years Albert Einstein counted as a physicist for sure, yet being neither rank-and-file nor a leader he counted as an outsider. I still remember the sense of shock I experienced when I learned that my physics teacher, Julio Raccah, had no wish to read Einstein's latest papers. An outsider, then, is one who is neither leader nor rank-and-file. Most outsiders, of course, simply play no roles in the field, but not all of them: those who have something to contribute not endorsed by the leadership may also be viewed as outsiders. What makes them outsiders is a different matter.

Of course, the interesting cases are the problematic ones. For example, a person may be a partial insider, as was Einstein. Michael Faraday's case was more complex: he was an insider experimentalist and an outsider theoretician. James Clerk Maxwell, his leading disciple, was surprised to learn that he was a theoretician. Yet this fact was kept secret, and historians of science had to rediscover it. Nevertheless, the fact that this fact was a secret is still not known. This is what is called, since Watergate, the cover-up of the cover-up. The cover-up includes the fact that reviews of my *Faraday as a Natural Philosopher* often failed to report this point, which is the book's message.

This brings me to the heart of the matter, namely to the significance of it all. Though I do not like intellectual leadership, I do recognize the existence of communities of scholars, and the need communities have for leadership. I also recognize the fact that communities are often identified by shared prejudices, by paradigms, but this fact is one I dislike and claim that it is no longer true even as a fact in any pluralist society.

I therefore propose, first and foremost, to distinguish community from beliefs. Admittedly, communities often play certain roles and the leadership needs to know the consensus (on the roles) of their communities. Kuhn says the role of the rank-and-file physicist is to solve certain technological problems which serve the community at large; the theoretical investigations of the leading physicists, then, serve the rank-and-file in their discharge of their recognized task. If this is wat Kuhn says, then he articulates a sentiment widely held by physicist in the period of large-scale United States Federal Gevernment Research and Development Funding, roughly between the A-bomb and the big budgetary cuts. If so, Kuhn is by now out-of-date. If this is not what Kuhn says, then I do not know what he thinks the rank-and-file scientist's task of puzzle-solving is. In any case, this is an example of a question, what is the current role of the community of physicists? Perhaps also of the subsequent or the derivative question, what is the priority order of today's agenda within physics?

But I am talking of the history of physics, not of physics. And I have argued in my *Historiography* that the self-selected role of the historian of physics is to orchestrate the process of hero worship which the community at large is supposed to perform with the scientist as the hero. I find this task distasteful. I admire Popper and Koryé so because they have, following Einstein, taken it as a central idea that an error can be an excellent intellectual adventure and a great contribution to the progress of science. Yet this view is still quite unpopular, and the ascription of an error to a thinker is quite often taken as a way to belittle the contribution of that thinker as a matter of course. This is something I find intolerable and intolerably common in the literature of the history of science. This, I think, brands me an outsider in a clear manner: those who perform the task of presenting scientists as if they had never made an error do not like to be told what it is. Nor are they wrong: many social tasks have an ambiguity about them which is essential to their proper functioning.

This is not to say that such social tasks are enjoyable. They seldom are. And what I tried to show in my *Historiography* is that historians of science have better and more enjoyable tasks to perform, and some of them do. This put me in the position of an underground advisor for quite a few historians of science who, I know, have read my work with pleasure, and deem it beneficial for their work, yet on the condition that they pass over this fact in silence. As historians they know that future historians may, if they put their mind to it at all, discover some unmentioned facts. But as members of their own community they know what they could do and what not.

There is little doubt that a radical change took place in the writing of the history of the exact sciences in the last two decades. No doubt, Koryé and Popper are the chief sources of inspiration here. Even historians of science who reject Popper's ideas, such as Gerald Holton, admit this, not to mention Kuhn, whom I heard to say both that he has no quarrel with Popper and the contrary. Philosophers and historians of science, notably Dudley Schapere, have taken the accord on matters of principle between Popper and Kuhn despite surface difference, as the emergence of the new paradigm.

This is the time for me to quit. When I wrote my *Historiography* I defended Popper since he was an outsider. Now he is the insider and I want to disengage from him as sharply as I can. Here, briefly, is my central point of dissent written from the viewpoint of particular interest in the history of the exact sciences.

What use is there in raising a palm tree and taking an axe to it? What is gained by the game of conjectures and refutations which Popper declares science to be?

My major critique of Popper is that some conjectures are worthless though highly refutable and at times, indeed, refuted; some conjectures are technologically useful though scientifically worthless and they concern only engineers or, at times, engineers and mathematicians, particularly ones concerned with complex differential equations, complex approximations and their theory, and more. But here I wish to stay a little with the useless, as the major point of criticism of Popper's views.

Popper opposes scholasticism, i.e., Talmudism, as do almost all philosophers of science. It is well-known that scholasticism dogmatically clings to a petrified paradigm and merely adds to the paradigm patchwork upon patchwork. Dogmatism was characterized by Sir Francis Bacon as disregard for refutation and as making light of it and as meeting it by minimal adjustments. Amazingly, W. V. Quine endorsed this view. I say amazingly because the Duhem-Quine thesis is the claim that one can always both be dogmatic and accept criticism. The Duhem-Quine thesis, incidentally, is logically true and was proven by Duhem with ease.

If Popper advocates conjectures and refutations but rejects scholasticism, he should explain how. He does. He says, scientific conjectures are highly refutable, scholastic ones are not. This is a very beautiful claim; it is false. For example, it is possible to amend the Newtonian law of gravity by adding to the inverse square an inverse cube and other factors and indeed this is practiced by every space-program. The amendment is very useful, highly refutable, yet, scientifically viewed, it is quite scholastic. Einstein said repeatedly, he never considered it a scientific option. Popper, therefore, is in error.

I have argued elsewhere, that in science conjectures are chosen within a paradigm, until it gets tired and exhausted: it is no longer a source of inspiration for new conjectures that may explain older ones. The existence of hierarhies of theories as series of approximations is the reason we may consider them both as series of conjectures and refutations and as progress, both progress in explanation, and as progress in shifts from paradigm to better paradigm. The historians of science should not conceal refutation, but they may also ask, what ground was gained in our heroes having first raised a whole grove of palm trees and then having taken axes to them? That science progresses, that it progresses in diverse ways, is unquestionable at present. That this progress is one, but only one, factor making the history of science exciting, is, I think, quite obvious. My *Historiography* was more an attempt to free the field from some past constraints. It is far from adequate as a guide. Yet if science is, among other things, a force liberating our thinking from past constraints, and if some individuals are helped by it to become autonomous, then the future autonomous historians of science will have enough exciting work on their hands.

Perhaps the point from my *Historiography* which I like best is that of asserting autonomy—of both the historians and their heroes, and by the clear contrast of the opinions they happen to represent. And, to my regret, till today too many historians—especially the large group of historians who are still under the pernicious influence of Henry Guerlac—identify with their heroes instead of sympathizing with them, worship them and beautify their results in violation of historical truth while pretending to be scholars. We have to know that our heroes have viewed science quite differently from us, and present their work both from their viewpoints and ours. In writing my study of Faraday I attempted to do this, critically presenting his social background, his inner conflicts, his methodology and his paradigm—constantly reminding my reader that all of Faraday's discoveries are by now superseded. The ideal history of science today, I still think, is the very opposite of identifying one's ideas with the ideas of one's hero. We have to be problem-oriented and record that most past theories are refuted, and do so without being apologetic in the least. Yet this is not enough, and I wish to supplement my earlier work. We may want to contrast our own way of choosing problems with how past thinkers chose their problems and how they approached them. At times this would lead us to consider social history, at times the spirit of the age, including the popular prejudices shared by our heroes, including, particularly, their metaphysical and methodological views, and always in contrast with today's case. We may wish to offer integrated portaits—intellectual portaits—of our heroes, but we have to stay on guard: no person is fully integrated and portraits should not include more than history invites.

Some studies approach this idea. The current concerted study of Newton, in particular, comes close to it. But as yet too few historians notice that he was influenced by conflicting philosophies—by mystic doctrines, by inductive philosophy, by Cartesianism and more. To ascribe these views to him and ask how much they helped and how much they hindered his progress is still regrettably uncommon. The history of science is as open and challenging as ever.