

The dusk of incommensurability

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Kuhn's era is nearing its end. This is a time of reflections. Perhaps, the fresh young Turks are here, but we do not know who they are, and what their new rule will be like. What would remain of Kuhn's teaching? Will it have an enduring effect? What should replace it?

Science aims at replacing divine inspiration with rational thinking. How can human thinking, however rational, compete with the authority of divine revelation? Attempts to answer this question constitute the history of methodology, first of philosophy and then of science. Induction, deduction, and transcendental deduction, were offered as possible candidates for filling the void that was left by the forsaken God. Their failure is the proper starting point of the current discussion about science. The starting point of our discussion is that no known method provides us with a guarantee for truth.

But we can still try to be rational, and make rational predictions. Even without guarantees, science is the greatest of human achievements. This needs no justification. But how can we *explain* it? Popper presented this interesting problem, thereby inaugurating a new era in the philosophy of science. His outlook soon became pregnant with interesting new problems forming a new agenda. In a nutshell, it was this: assuming that there is no guarantee for the truth of scientific theories and the truth of the predictions that rest on them, why do we value science? What makes a theory scientific? And, how should we rank theories within science?

Fuller maintains that Kuhn had no alternative agenda to offer. He offered no real problems to be solved. Instead Kuhn (or rather Conant and Kuhn) had a hidden political platform, possibly a means for keeping intellectuals away from the practical problems that might call for political activism. He bribed them with the hope of becoming the new establishment. This is interesting, since obviously intellectuals are no fools. How, then, did he appeal to them?

There is standard answer to this question. It is discussed in the next paragraph. But let us first note that whatever the hidden political platform of Kuhn's philosophy, it nearly annihilated the carefully planned, promising task of explaining the fragile uniqueness of science as a progressive enterprise in the age of uncertainty. Kuhn offered a powerful romantic myth, which putatively explained that science is not at all unique, nor quite progressive. According to this myth science is a Dionysian-Christian cycle of waning and waxing fashions.

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Why was this myth so appealing? The standard answer is that its appeal is the irrationalism which it harbours. Indeed, yielding to some inevitable, blind, indefatigable forces (in our cases socio-scientific fashion changes) is found tempting by many people. Why should it appeal? Intellectuals, to repeat, are no fools. Why should they be tempted by irrationalism? One possible answer is this: irrationalism is tempting since it glitters with a promise for a safe return to the pastures of divine certainty, to an infantile security that in itself is a promise for relief from moral responsibility. Since the indefatigable laws of socio-scientific fashions are the driving force of science, whether we like it or not, we need not worry about doing justice to theories and to their originators, we need not concern ourselves with choosing right from wrong. To be led by a strong force (divine, Kuhnian or other) suggests a possible break away from the Sisyphian life called autonomy. Nathaniel Laor and Joseph Agassi develop such a line of thought in their sharp and revealing ‘How ignoring repeatability leads to magic’ (*Philosophy of the Social Sciences*, 2000): they convincingly analyse such irrationalism as the dominant trend in analytic philosophy as well as in present day philosophy of mind (which is mostly analytic).

Kuhn, then, is a champion of modern dogmatism. Yet explaining his success by ascribing immorality and/or moral laziness, latent or not, to intellectuals can be dangerous. It is itself a myth as old as the one used by Kuhn (and intimately connected with it, since it is moral laziness or its opposite—the hubris of being truly moral—that brings the king-for-a-day to his inevitable fall). It is too easy an explanation and so must only be resorted to, if at all, with great prudence. If I resort to it here it is because I find no better. I will explain Kuhn’s ‘incommensurability thesis’ as the relic of a tradition that has yet to grow out of its yearning for certainty.

My thesis is this: The secret of the success of Kuhn’s philosophy in general and of the incommensurability thesis in particular is the license it provides for bypassing modern formulations of the problem of induction while preventing philosophy from advancing to its post-inductive stage.

Traditionally, induction was supposed to prevent the possibility of controversy (Bacon). The ‘problem’ of induction was the discovery that it does not do that: empirical observations do not impose (the correct) theory upon us (Hume). In other words, empirical observations can be explained by conflicting theories (Duhem, Quine). This is why they should facilitate controversy instead of agreement (Popper). Kuhn tried to explain away controversies. He did so by means of his incommensurability thesis. He said that controversy is impossible or else negligible. (He was a neo-Baconian.) Agassi suggests that controversies are not only unavoidable, but also desirable. His suggestion is a vital improvement to Popper’s since it admits metaphysical outlooks as prominent in the development of science, and he does not attempt to explain away such outlooks, as Popper does, by reducing them to refutable theories on the one hand, or to mere psychological eccentricities on the other.

Kuhn presents his discovery of paradigms as a last piece in a giant puzzle. Everything falls into place, once it is admitted that natural scientists work under the shadow of super examples—universally recognized scientific achievements—that are taken as models for science-making. Paradigms explain, he says, why natural scientists are rarely in dispute over fundamental matters, and why natural scientists refuse to admit ‘anomalies’ (i.e. refutations) until they are forced to do so by socio-

scientific change. In short, paradigms explain why rational discourse cannot occur in science: fundamental matters are either trivially accepted, or they are trivially rejected. If they are trivially accepted, no questions are asked about them, and all non-fundamental criticism is hushed. And if they are trivially rejected, all non-fundamental criticism linked to them is ignored. This is Kuhn's incommensurability thesis in a nutshell.

It is a transcendental proof for the impossibility of rational disputes. It is reminiscent of Wittgenstein's fideistic *On certainty* (another neo-Baconian creation). Both are valid but unsound: they are refuted by the observation that rational disputes exist. Rational disputes arise when observations do not impose (the correct) theory upon us. We then try to consider competing theories, we devise new experiments and suggest new theories that possibly explain our observations and the failure of previous predictions. Admitting rational disputes, then, is a prerequisite for participating in post-inductivist philosophy. These may arise, in principle, when contradictory explanations, which were not refuted, exist. They can be suggested, in principle, when observations (or any other form of data), do not impose (the correct) theory upon us. This is the core of Hume's so called 'problem' of induction. Kant tried to bypass it by maintaining that our *a priori* apparatus imposes (the correct) theories upon our observations. Disputes in science, he maintained, are (transcendentally) impossible. Kuhn tried to bypass disputes by maintaining that paradigms impose solidarity upon us, at least to the extent that rational disputes are (socially) impossible. But why should cases in which data do not impose (the correct) theory upon us be deemed a problem?

I do not know. What still surprises me is that the problem of induction has proved to be so enduring and popular, that we are still encouraged to chew the cud of induction. Let me explain this in some more detail.

Kuhn mentioned Quine as his central inspirations for his ideas about incommensurability. Quine's version of the incommensurability thesis is usually referred as a two-fold thesis: 'the indeterminacy of translation thesis' and 'the under-determinacy of translation thesis' (both previously noted by Duhem). Both are results of Quine's attempts to inspect the analogy between the study of nature and the study of language. Indeterminacy of translation is the claim that knowledge of intentions of other speakers can never be certain. It is normally obtained in under-determined circumstances. The circumstances in which we learn that a given sentence expresses a given intention or even that a given word expresses a given concept allow just as well for competing conjectures to arise regarding the intention or concept expressed, and no procedure can guarantee that a given conjecture is better than the next. So we can never be certain that what hearer has understood is what speaker had intended.

We cannot infer intentions out of the circumstances of their expression. This formulation helps reveal the fact already noted above, that the problem of induction lies in the heart of the indeterminacy thesis: the latter is a special case of the former. We cannot infer intentions out of the circumstances of their expression *because* we cannot infer necessary empirical connections out of collected observations.

Quine's under-determinacy of translation thesis is the claim that any experience can be accounted for by competing theories. Observations cannot determine the status of scientific theories, because they merely under-determine them. Thus, mutually exclusive theories can (each) account for what has been observed. It is

interesting to note that the under-determinacy claim is a generalization of the indeterminacy claim; we cannot infer intentions out of the circumstances of their expression, this is a special case of the fact that we cannot infer an informative theory out of observed facts. Thus, the under-determinacy claim is a rather straight forward re-formulation of the problem of induction. (Quine students may observe that the analogy between the study of language and the study of nature is thus complete, and that Quine's denial of this is an inconsistency on his part.)

This brings us back to Popper and to Kuhn. Both denied that translation is problematic. Kuhn had done so by means of his incommensurability thesis, i.e. by rejecting the existence of translation problems within the natural sciences: either you are within our paradigm and understand us perfectly (ignoring anomalies) or you are within a different paradigm and you do not understand us at all (the theories we produce and discuss are thus, incommensurable). This option carried an impossible price: the denial of real progress in science, and the annihilation of the task of explaining it, explaining the fragile uniqueness of scientific knowledge in the age of uncertainty. The price is too heavy and the gain is very small. Popper said translations are not problematic since they are conjectures, as any other theory, and as such may be criticised and possibly improved. The problem, he said, was not how to bypass the possibility of rational dispute but how to explain it. It is not how to bypass the fact that science is progressing but how to explain it.

Examples, even those super-examples called paradigms, are cases given to study and observation. They are never conclusive with respect to the theory that aims at explaining them, but rather they are open to interpretation. Any such case can be taken to support contradicting assertions and be explained by conflicting theories. Take any of Kuhn's paradigms for paradigm-shifts (Newton's revolution or Einstein's, for instance). Popper uses them as exemplifying a tolerably coherent process of conjectures and refutations, i.e. as exemplifying the relatively rational progress in science. That examples may exemplify conflicting explanations is no anomaly. It is the core of the so called 'problem' of induction.

The livelihood of Kuhnian historians is based on cases in which scientists have decided to accept some theory over another despite the fact that observations at the time were inconclusive. They celebrate such cases as exemplifying the irrationalism of scientists: their choice is explained as paradigm guided. As if observations ever really impose (the truth of a) theory upon us, so as to make its choice unavoidable. Kuhn's success lies then in his ability to exploit the tension that is felt only by those who still wish to view science as inductive and certain (and are amazed when they discover it is not). Once we give up the demand for a final and absolute verdict regarding the truth value of a theory the tension is lost: the scientific activity need not be explained as a paradigm guided gamble. It is explained as a critical study of conjectures.

The success of Kuhn's philosophy, and the incommensurability thesis is (or was) a symptom of decay. It is not without hope, then, that we notice that Kuhn's era has indeed reached its end. Perhaps some fresh young Turks are here. Hopefully they will avoid hidden political platforms altogether. Hopefully, they will leave the incommensurability thesis behind and choose to transcend inductivism altogether. At least as an opening gambit.