Thomas Kuhn: A Historian's Personal Recollections

NERVOUS BREAKDOWN

Immediately after Thomas S. Kuhn (1922–1996) published the first edition of his *The Structure of Scientific Revolutions* (hereafter abbreviated as *Structure*) in 1962, philosophers of science attacked him. Karl R. Popper, for example, blamed Kuhn with his famous comment, ‘down with the normal science’.

Kuhn’s pride was so damaged by such ridicule from philosophers (especially on the points of his misuse of philosophical jargon) that he suffered from a sort of nervous breakdown for some years. Its symptom appeared in his oft-spoken expression: ‘Please do not quote me. Everybody misunderstands me. Unless you have my previous permission, please do not refer to my saying!’

As a former student of Kuhn, I witnessed how he coped with philosophers’ criticisms. In the late 1960s, I advised Kuhn to add a section ‘postscript 1969’ for a Japanese edition (on which I was working) and thereby end the controversy.

I even said to him, ‘You know, philosophers are professional debaters, who found in your thesis an attractive target that provides them the opportunities for endless attack.’

From the outset of his quarrel with science philosophers, it was quite clear to me that the difference of their views (or images) of science might never be resolved. While Kuhn primarily wrote the *Structure* as a historiography of science for historians of science, philosophers of science, like the Popperians, took it to be a study on how scientists ought to be. While Kuhn described the way that ordinary scientists tend to proceed in their problem-solving activities, philosophers argue from the naively idealistic image of science that they have constructed with a logical consistency but without consideration of the manner in which scientists actually perform. Kuhn often complained that no philosopher has ever read a technical paper of a scientist to know what he is normally doing. Though Kuhn spent the rest of his life trying to explain and persuade philosophers concerning the actual mechanism of scientific research, it was a series of fruitless argumentation.

There was an even more deep-rooted difference between Popper and Kuhn. During the interwar period, regardless of specialty there appeared a generation that was eager to defend the tradition of modern Western rational science as opposed to Nazi and Stalinist sciences. Popper, the Vienna school, Robert Merton, and even Marxists like Joseph Needham and J. D. Bernal shared such a
consensus view. Professionally, the philosophy of science was grounded as an academic discipline that aimed to teach the philosophical basis of Western science at colleges and universities. This perspective lasted well into the post-war period. They took Kuhn’s attitude towards science as a challenge to their established authority and criticized him as a ‘relativist’ because he admitted the existence of plural ways of scientific development rather than extolling the absolute value of modern science. I remember that even Kuhn’s contemporaries, like Robert Cohen, accused him of doing serious damage to the philosophy of science, though the latter changed his attitude later.

On the other hand, Kuhn could not understand what philosophers meant by ‘relativism’. What’s wrong with relativism? Kuhn belongs to the wartime generation, and was mobilized in war as a rank-and-file scientist and also saw the rise of Cold-War big sciences during the post-war period. Thus, Kuhn could not maintain such an ideal and absolutely valid picture of science. A little later, by the post-war generation of Jerry Ravetz and me, conventional science had been completely relativised.

KUHN BACK TO PHYSICS

Kuhn himself was by no means a conservative, but an ambitious liberal as his youthful days testified. When he and I were together at Harvard in 1955–56, we discussed Marxist works with Marxist terminology, though he could never be an orthodox dogmatic follower. Under the McCarthy purge that was still in the air, he once told me, ‘In this country if I were to say that I was “working on a social history of science”’, I should lose my university job.’

With his inclination for social history, I had expected him to move from philosophical debate (or rather quarrel) to the sociology of science. He did follow my advice with thanks on the point of not repeating interminable philosophical arguments, which in my view did not lead anywhere. Everybody seemed to have expected Kuhn to work in the sociology of science to become the founder of the then-called Kuhnian sociology of science. But he said to me, ‘I was not trained in sociology and so I shall go back to my old field of physics.’

Then, for nearly a decade, Kuhn devoted himself to the completion of his earlier study of the Copenhagen group of physics, although he occasionally talked about philosophical problems upon request. When I told people like Barry Barnes (who wanted to follow the line of Kuhnian sociology of science), they were deeply disappointed and resented his decision that he had moved away from sociological problems.

Why and how has Kuhn made such a thematic change in his late forties? Due to his earlier experience with philosophers of science, he was so frightened to step into the new field of sociology without mastering customary use of sociological jargon. As a physics PhD, the language of physics was the only one familiar to him.

KUHN BACK TO PHILOSOPHY

In the late sixties, along with the rise of the student movement, the Kuhnian paradigmatic shift gave support to their anti-establishment mentality in their