



[THINKING ABOUT SCIENCE] MASSIMO PIGLIUCCI

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Popper vs. Kuhn: The Battle for Understanding How Science Works

Karl Popper and Thomas Kuhn are two of the towering figures of twentieth-century philosophy of science, and for good reason. Some of their ideas even made it into mainstream culture and are among the few concepts from philosophy of science that are somewhat known to practicing scientists. Popper, of course, gave us the idea of falsifiability as a way of distinguishing genuine scientific theories from pseudoscience. Kuhn introduced the concept of paradigm shift to indicate

those rare situations in the history of science in which a field abandons a framework in favor of another, as a result of a crisis induced by a mounting number of puzzles that cannot be resolved within the context of the old framework.

I recently read an interesting article by Darrell P. Rowbottom, to be published in *Studies in History and Philosophy of Science* (available online for download at <http://users.ox.ac.uk/~sfop0262/SHPS.pdf>), about a notorious controversy between Popper and Kuhn.

Rowbottom points out that the flashpoint of the controversy was the publication of *Criticism and the Growth of Knowledge: Proceedings of the International Colloquium in the Philosophy of Science*, in which the two heavyweights of philosophy of science went at each other with no punches pulled.

Essentially, Popper's position was that the main role of a good scientist is that of a critic of established or working hypotheses. As he put it: "It is the most characteristic feature of the scientific



Karl Popper (left) and Thomas Kuhn.

method that scientists will do everything they can in order to criticize and test the theory in question [i.e., any theory being considered]. Criticizing and testing go hand in hand: the theory is criticized from very many different standpoints in order to bring out those points which may be vulnerable." I have

not the theory. . . . It is a poor carpenter who blames his tools." That's certainly more like the reality of scientific practice, at least according to my experience.

Of course, Kuhn's view cannot be the entire story, because certainly Popper-type criticism of hypotheses does take place in science, and we need an

an individual scientist to pursue, but both Popper and Kuhn missed the obvious solution: both functions are performed at the group level, i.e., by the ensemble of scientists working within a particular discipline. Some scientists are more inclined or apt to engage in criticism and others in puzzle solving. Science works well precisely because there is a division of labor that facilitates the continuous interaction between the two functions.

Even so, Rowbottom raises but does not address the obvious crucial follow-up questions: What is the ideal balance between Popperian and Kuhnian functions in science? And how does that differ from the actual mix between critical and puzzle-solving scientists that we find on the ground? Are the answers to these questions different for different sciences? And if so, why? My experience is that there is a far greater number of puzzle solvers than critical scientists around, which is probably a good idea as long as enough critical scientists are funded and their work is taken seriously for the vital part it plays in the broader enterprise. It would be interesting for funding agencies and journal editors to actually address this question empirically and to adjust their priorities accordingly. To my knowledge, this has never been attempted.

There is one more crucial function that Rowbottom mentions but leaves as an unexplored black box: creativity. Both critical scientists and puzzle solvers have to get their ideas from somewhere, somehow. Yet what philosophers of science often refer to as the "context of discovery" (how new ideas come about) is simply not amenable to the kind of logical analysis that philosophers are good at, which makes them concentrate on the "context of justification" (how ideas are tested once they have been thought out). It strikes me that the context of discovery is a question for sociologists and psychologists of science that remains largely unexplored.

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no idea which scientific laboratories—if any—Popper hung around, but this description surely doesn't resonate with the kind of science that I have seen in action during a quarter century of practice as a biologist.

Kuhn's view, on the other hand, was that criticism is exceptional (it's what leads to the occasional paradigm shift) and that much of science is what he called "puzzled solving," or "normal science." To wit: "Trial attempts [to solve puzzles], whether by the chess player or by the scientist, are trials only of themselves, not of the rules of the game." Furthermore: "Failure to achieve a solution discredits only the scientist and

account of its role within the broader context of scientific progress. Kuhn may have been right that most of the time the rules don't get questioned, but if they were never questioned we would have no explanation for the occurrence of paradigm shifts to begin with, thus undermining Kuhn's own picture of how science works.

The idea that Rowbottom explores in his forthcoming paper is that Popper and Kuhn's disagreement amounted to a distinction between two functions within the practice of science, one of criticism (Popper) and one of puzzle solving (Kuhn). The debate hinged on which function was more important for