Are there 'Kuhnian' revolutions in biology?

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The recent death, on 17 June 1996, of the noted philosopher of science, Thomas Kuhn, at age 73, provides a suitable occasion to remember and commemorate his contributions to the philosophy of science. It also provides an appropriate moment to ask how well the Kuhnian idea of scientific revolutions, which was developed principally from study of the physical sciences, applies to biology.

Kuhn, a professor emeritus at MIT in recent years, had written or coauthored five books and numerous scholarly articles, but he is undoubtedly best known, and will be best remembered, for *The Structure of Scientific Revolutions*[1], first published in 1962. In this seminal work, Kuhn argued persuasively against the traditional idea of 'scientific progress', the notion that scientific knowledge involves the steady growth of understanding through the application of something called 'The Scientific Method'. He argued that, in reality, science involves two distinctly different processes. For the most part, scientists work within certain conceptual frameworks or models, 'paradigms'. This work serves to embellish and strengthen the central paradigm at the heart of each field and is essentially conservative in nature. Kuhn termed such activities 'normal science'. Yet, the continued practice of normal science within a field often shows up weaknesses in the central paradigm. When these weaknesses can no longer be ignored, the stage is set for dramatic conceptual change. Such radical changes of thought are usually brought about by young individuals, not established figures, and their new concepts replace the old, failing paradigm with a new and stronger one that explains phenomena that the previous one could not. Kuhn termed such radical conceptual breaks 'scientific revolutions'. A successful scientific revolution is then followed by a period of normal science dedicated to the new paradigm. In effect, Kuhn was proposing a form of conceptual 'punctuated equilibrium', 10 years before that term was coined by Niles Eldredge and Stephen Jay Gould in a different context[2].

Kuhn's idea certainly had an immediate, and largely favourable, reception in large sectors of the scientific community. Within two years, his book was on the reading list of courses in the philosophy and history of science in American universities. More significantly, his terminology, in particular the concept of 'scientific revolutions' and the term 'paradigm', became part of the everyday discourse of scientists everywhere.

Yet, there was some resistance, even amongst those scientists who found themselves referring to 'reigning paradigms' and the like. The notion that what scientists believe at any one time is determined in part by group consensus – in some corridors, there were mutterings that the idea involved little more than 'mob rule' in deciding scientific truth, a notion vehemently denied by Kuhn himself[3] – was unsettling. Furthermore, the neurological implications – that young brains are much more likely to generate and be receptive to major conceptual breakthroughs – though not new, could not have been comforting to those past their first youth. Nevertheless, the impact of Kuhn's idea was immediate and pervasive. It would not be inappropriate to refer to the 'Kuhnian revolution' in the philosophy of science.

The question of generality, however, still nags. In contrast to many earlier, *a priori*, philosophical theories of knowledge, Kuhn built his case from examples, in effect inductively. (Kuhn's ideas co-exist uneasily today with those of Karl Popper, an arch-foe of argument from induction; it is, in fact, impossible to be both a Kuhnian and a Popperian, at least at the same instant.) Kuhn's primary examples were all drawn from physics and chemistry – Kuhn had taken his bachelor's degree in physics – and involved some of the classic discoveries in those sciences: the Copernican, Newtonian and Einsteinian revolutions and Lavoisier's disproof of the phlogiston theory.

But, what about biology? Have there been proper Kuhnian revolutions in biology? One's immediate response is likely to be 'of course', as one instantly thinks of the 'Darwinian revolution', triggered by the publication of Charles Darwin's *The Origin of Species* in 1859, and the molecular biological revolution, ushered in by the Watson-Crick model of DNA in 1953. Yet, closer examination of both these episodes shows that neither truly fits Kuhn's model.

In its immediate impact, Darwin's work was, at best, half a revolution. The evidence compiled in *The Origin of Species* certainly convinced a majority of scientists that evolution was a fact, but it signally failed to convince most that natural selection, Darwin's proposed mechanism of evolutionary change, was its chief agent[4,5]. Furthermore, when the converts are compared to the non-converts, there was no dramatic division by age[4], *contra* Kuhn. Nor was there a steady, let alone rapid, increase in the number of the believers in natural selection as the main motor of evolutionary change. (Many believed that natural selection could take place but assigned it a minor role in evolution.) By the end of the 19th century, Darwinism, defined in terms of its emphasis on natural selection, was, to use Julian

Huxley's word, in 'eclipse'[6]. In fact, Darwinism, in its refurbished form of Neodarwinism, was only to triumph in the 1930s and '40s, with the advent of what is known as 'the evolutionary synthesis' or 'the Modern Synthesis'[7]. In its dynamics, if not in its eventual impact, Darwin's revolution does not fit the Kuhnian picture.

In contrast, the molecular biological revolution that began in 1953 would seem to be a clear-cut case of Kuhn's model, both in terms of its immediate effects and its reception. There was rapid and nearly universal acceptance of the idea that the Watson-Crick model could explain what a gene was. Furthermore, it was clear that the model effectively dictated a whole new, general research programme, focussing on the nature of the genetic code, the mechanism of protein synthesis, and the manner of gene replication. Yet, what was the old paradigm that the new one had relegated to the dustbin of history? The truth is that there wasn't any. The Watson-Crick idea replaced not a former paradigm but a conceptual vacuum. True, if pressed to a wall at a cocktail party and asked what a gene was, the average genetically minded biologist before 1953 would have mumbled something about DNA or 'nucleoprotein' and a gene acting 'like an enzyme' but there were, in reality, no clear ideas about gene structure or action. Furthermore, after 1953, there was no obvious division by age in those who accepted and those who rejected the model, if only because nearly everyone, regardless of age, accepted it. There is no record of an 'Old Guard' fighting to the death to defend the previous paradigm because there was no previous paradigm to defend.

While the Darwinian and molecular biological revolutions do not comfortably fit the Kuhnian model of a scientific revolution, there is a third episode in the history of modern biology that may come closer. That is the Mendelian revolution of the early 1900s, even though it is not usually tagged as such. It involved an immediate conceptual breakthrough that challenged previous ideas; it acquired adherents quickly; it certainly had enemies; and eventually it triumphed, although it took nearly a generation to do so[8]. Yet, in two respects, it also does not quite fit Kuhn's model of scientific revolutions. First, it is not clear that it was more readily embraced by younger scientists than older ones: William Bateson, its chief proponent, was only 4 years younger than its chief antagonist, Karl Pearson, and both were middle-aged men in 1900 (Bateson was 39, Pearson was 43). Second, and more disconcertingly, the Mendelian revolution, right as it was about heredity, was simultaneously part of the counter-revolution against Darwinism[9,10] and, in this, the champions of Mendelian heredity were clearly rather wrong-headed. In consequence, early 20th century Mendelism is something of an embarrassment as a scientific revolution; it conspicuously lacks the clarity of the conceptual revolutions discussed by Kuhn.

Scientific 'revolutions' in biology have certainly taken place – as recognized even before Kuhn[11] – but do not seem to match the Kuhnian paradigm particularly well. (Perhaps, however, that is appropriate. Much of biology itself is focussed on matters of diversity and why should not conceptual change also exhibit diversity?) Yet, to say as much is not to disparage Thomas Kuhn's achievement. *The Structure of Scientific Revolutions* swept away an outmoded, untenable theory of scientific progress and alerted scientists to the fact that there is more to scientific belief than simply rigorous proof structures. If some of the implications of his ideas about why we believe what we do in science still seem uncomfortable, that can only be a further stimulus to thinking about the nature of scientific knowledge and belief. And, in the meantime, science itself has been both enriched by Kuhn's idea and enlivened by talk about 'scientific revolutions' and 'paradigm shifts'. Kuhn, along with Popper, is only one of two philosophers of science whom most scientists know anything about and that in itself is a measure of his achievement.

References

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