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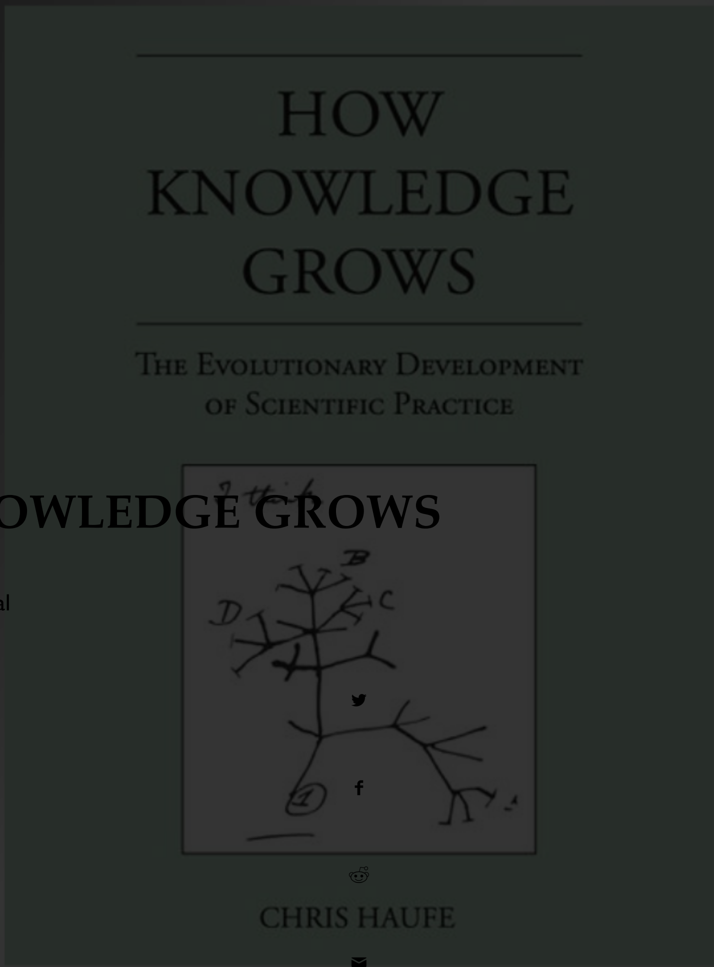
Reviewed by Juan V Mayoral



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In his paper 'The Road since Structure' ([1991]), the historian and philosopher of science Thomas Kuhn emphasized the parallels between his own theory of scientific development and biological evolution. His theory had been introduced almost thirty years before, in 1962, in the well-known book *The Structure of Scientific Revolutions*. Three decades later, Kuhn showed that scientific revolutions—those episodes in which a certain community of experts leaves behind a set of paradigmatic problems and problem-solutions to adopt a new one—split the scientific community into two or more sub-groups, each of which in turn devotes its efforts to developing new problem-solutions for new problems. Scientific development has thus the appearance of 'a layman's diagram for a biological evolutionary tree' (Kuhn [1991], p. 98). Each new branch in that tree usually deals with increasingly specialized issues with increasing success, and this is part of the evidence of scientific progress. Communication between these specialities tends to be limited. Kuhn's lexical theory helped to show why, and provided reason to think of this limitation as a welcome consequence.

This succinct account of Kuhn's evolutionary analogy serves as an apt preamble to Chris Haufe's *How Knowledge Grows*. Haufe intends to show how this 'layman's diagram' would turn into a sophisticated model of scientific development driven by the force of natural selection. He briefly examines other 'evolutionary predecessors' (p. 16) such as Karl Popper, Donald Campbell, Stephen Toulmin, and David Hull (pp. 16–29), but these authors are not mentioned in the rest of the book; the main reference for Haufe remains Kuhn's work. As Haufe says, 'I view my efforts in this book largely as an attempt to organize and clarify Thomas Kuhn's ideas about scientific knowledge under the evolutionary framework that he ultimately came to see as responsible for the epistemic phenomena that interested him' (p. 17). Though I am not certain that Kuhn was interested in anything more than a parallelism between the evolutionary theory and his own, Haufe's demonstration of how an evolutionary model of scientific development can work is nonetheless stimulating.

Haufe's book is divided into two parts: Part 1 (chapters 1–6) introduces and examines the model step-by-step. The model is compared to some of Kuhn's theses, which are considered evidence for the discontinuous nature of scientific knowledge. The result is an abstract model of scientific development that coincides with some of Kuhn's points at the appropriate moments. Part 2 (chapters 7–9) displays, in Haufe's words, 'an extended historical case study' on the 'emergence of evolutionary paleontology', in which his model—especially 'the branching process described in chapter 5'—is put to work (p. 9). This case study mainly describes Stephen Jay Gould's attempts to establish palaeontology as an autonomous discipline that independently contributes to evolutionary theory. There are other key characters in this historical episode, such as George Gaylord Simpson, Norman Newell, Thomas Schopf, and Niles Eldredge mentioned where relevant, but, as Haufe says, Gould plays the leading character (see p. 211). What would have been helpful for readers here was a concluding discussion of the efficacy of Haufe's evolutionary model for interpreting this case study.

In his model, Haufe shows how the progress of scientific knowledge can be analysed in terms more akin to an evolutionary picture based on the force of natural selection. Inspired by Gould (p. 29), Haufe translates our old mode of expression about progress into the more easily analysable notion of 'directionality'. Scientific knowledge, he shows, exhibits historical directionality in two recognizable senses: first, problem-solving abilities usually increase as time goes by; second, at some points in history, the scientific group divides itself into two or more sub-groups, thereby increasing the number of specialities (see pp. 29–36). So, our capacities to solve problems and understand phenomena scientifically increase in 'depth' as well as in 'breadth' (p. 35). Roughly, these two modes of directionality closely correspond to the outcome of those activities that Kuhn termed 'normal' and 'revolutionary' science.

Haufe understands scientific knowledge in terms of problem-solving abilities rather than in terms of propositional knowledge. Those abilities are the result of scientific practices. Scientific practices include all those elements that belong to a Kuhnian disciplinary matrix, and maybe more (Haufe (p. 39) credits Kitcher ([1993]) as the source of his perspective on practice). The significance of this way of understanding scientific knowledge is that it may acquire the features of a Darwinian population. The concept of 'Darwinian population' comes from Peter Godfrey-Smith ([2009]), and refers to any 'collection of causally connected individual things in which there is variation in character, which leads to differences in reproductive output […] and which is inherited to some extent' (as quoted by Haufe, p. 37). Scientific practices, Haufe says, can be considered Darwinian populations, and so they evolve under the pressures of natural selection just as biological populations do (pp. 37–41). As he says, 'scientific practice is—at least at times—a nearly paradigmatic Darwinian population and is thus highly susceptible to the influence of natural selection' (p. 163; see also p. 209).[1]

The pressures Haufe is interested in are those he identifies as epistemic. By definition, a pressure is epistemic if (and only if) the adaptation of scientific practice it promotes brings about progress of knowledge in any of the directions referred to above (p. 34). In his words, 'some factor is an epistemic pressure when it causes us to develop improvements in our knowledge' (p. 35; see also pp. 80, 91). He does not include 'sociocultural pressures' in that set because they do not necessarily cause a directional advancement of science (pp. 33–34). Even if that progress happens, he says, it would be an incidental achievement, because the corresponding goals are not epistemic—they do not aim for 'a better understanding of natural phenomena' (p. 33). This aim for a better understanding of natural phenomena, however, is an addition to the definition of epistemic pressure given above. Factors that he labels 'epistemic pressures' include only those that cause directional progress that are intended for this role. Accordingly, his initial definition is incomplete, and depends on a distinction between internal and external factors that is arbitrarily imposed. After all, some factors—for example, aesthetic concerns—contribute to directional progress even though they may be considered sociocultural in origin. As scientific values, they are invoked in order to better understand phenomena, but practitioners of the fine arts do not conjure them up with that purpose.[2]

Haufe argues for his internal–external distinction on the basis of a further argument. For a pressure to be internal—and therefore epistemic—the progress it promotes involves 'achievements that are recognizably important to most practitioners, but not to most nonpractitioners' (p. 35). Again, this quotation is part of a definition. As a counterexample, we may think of a pressure that is sociocultural in origin and that may end up being internal to science, particularly if only experts understand the significance of the achievement it helps to promote (its significance is not layman-friendly). The origin of aesthetic factors in science is again a good case in point. Though aesthetic concerns are shared by individuals both in and outside of science, these concerns can sometimes play a role in scientific achievement (for example, Kepler's) and scientific methodology (for example, Dirac's viewing the beauty of theories as an epistemic value) in terms that are difficult to understand for the uninitiated. Are they, then, internal or external? Haufe's perspective does not seem able to answer this question.

In short, Haufe shows that the historical development of science can be modelled on the assumption that scientific practices are Darwinian populations that evolve adaptively in response to epistemic pressures, so they fall under the scope of natural selection. The history of science may thus be considered just another example of evolutionary phenomena such as the history of life itself (p. 153). His division between internal and external—and epistemic and non-epistemic—pressures seems somewhat outdated, but the model details how both histories are linked by evolutionary theory. Haufe's model is not completely descriptive. Rather, it is an idealized model that presents the conditions for the efficient progress of scientific practice if it is subject to natural selection (see p. 103). If we believe that the conditions introduced here are representative of an idealized scientific behaviour, we can use the model to estimate the deviation and act accordingly. So, as in Kuhn's case, there is a serious normative message and challenge in this model.

Haufe's model is, at some points, similar to aspects of Kuhn's overall view. Yet, the evolutionary characteristics that Haufe adds help us to see some of Kuhn's key elements in a new light. Kuhn showed that a scientific community is not only a meeting of experts, but rather a collective epistemic agent whose members—despite the differences between them—are joined together by their ability to recognize relevant problems and solutions when they see them. Scientific practices are preserved on that basis.[3] Haufe shows that once we consider the scientific community in light of evolutionary theory, we see that it is the niche in which scientific practices either survive or die out. It all depends on the niche constructors, the scientists themselves, on the pressures they introduce on the basis of their preferences, and thus on the kind of environment they create for those practices. For Haufe, 'the population of scientific practices can be understood as occupying a constructed niche, where the selection pressures are the preferences of scientists' (p. 80; see also p. 103). 'A scientific community', he says later, 'qualifies as a cultural niche' (p.

99; see also p. 86). Of course, Haufe's proposal for these niche constructors is that they give priority to epistemic over non-epistemic pressures. The normative message that accompanies this model all along is that directional progress requires the preference for those pressures that he considers convenient—the epistemic ones (see p. 72). Despite the evolutionary view introduced here, this model is pretty traditional.

At other points, however, Haufe's model seems less well equipped to cope with Kuhn's model. Nevertheless, recall that Haufe's model is not just a re-telling of Kuhn's, but rather a clarification of the evolutionary analogy in philosophy of science that takes Kuhn's views as a source of inspiration and comparisons. There 'problems' merely reflect the differences between their views. After all, Haufe's preferences concerning pressures are only an intimation of his own perspective on science, which may or may not be coincidental with Kuhn's. Having said this, there are times when Haufe's views are scarcely coincidental with Kuhn's. His model captures normal science under a new evolutionary light, demonstrating some interesting features that have normative weight. Besides this, the so-called branching process in chapter 5 shows how a specific kind of progress—one that involves an increase in the number of specialties—is an effect of natural selection. Speciation is thus an outcome of his model. Scientific revolutions, on the other hand, present more problems.

Despite scientific revolutions—and crises—playing an important role in Kuhn's theory, Haufe does not fully integrate them into his model. Normatively, Kuhn's revolutions are not completely desirable. Haufe, in chapter 6, considers scientific revolutions as kinds of change located at one end of a spectrum on whose opposite extreme he locates branching. The rupture is more radical in the case of revolutions, and natural selection is only dimly active in such upheavals. Seen in this way, he contends, they resemble mass extinctions (pp. 177–78). In these cases, scientific practices lose their resemblance to Darwinian populations, and so are no longer progressive (see p. 195–98, esp. p. 198). Scientific revolutions are acceptable as long as they resemble episodes of speciation, with some divergence and some preservation of basic norms of inquiry. In contrast, insofar as they involve more drastically divergent views (still worse if revolutions are not motivated by the fight against, say, an anomaly) and radical change in norms, revolutions are not desirable: they do not contribute to directional progress in science (see p. 198). The normative message opposes their presence in the history of science. Crisis stages are undesirable too, since during this stage, decisions are made largely on a subjective and individual basis (p. 192). This involves the interruption in the activity of long-respected epistemic norms. To sum up, neither crises nor scientific revolutions seem to find their place in Haufe's model. Insofar as it is an attempt at shedding some light on Kuhn's perspective from an evolutionary point of view, some crucial aspects of the latter's theory still remain in the shadows.

Part 2 of this book includes, as noted, an episode in the history of palaeontology, demonstrating palaeontology's progressive emancipation from geology and integration into biological evolutionism. The case study is quite interesting and, just as with the biological theory in part 1, Haufe shows his expertise in the field. My only criticism of this part is that it seems too detached from the theoretical model in part 1. This case study mainly illustrates chapter 5—that is, the process of branching in scientific specialties. However, there are few references to the model itself in those pages (for some relevant passages, see pp. 265–67, 270–72). The main message of this case study—the process by which palaeontology gained autonomy—is perfectly understandable without the earlier parts of the book. To some extent, this is disappointing. Part 1 does not have many examples that illustrate its theses. I would have liked to see some historical examples for, say, the branching processes, directional advancement (in any of the two directions Haufe introduces), or the application of Orr diagrams in real cases from the history of science, but I found almost none.[4] I had hoped for part 2 to play this role, but it did not live up to that expectation.

In short, Haufe's book is interesting to read, either as a way to make the evolutionary analogy in philosophy of science much clearer, or as a discussion of the recent 'revolution in paleobiology' (to use David Sepkoski's ([2009]) expression). It is also a sensible discussion of the possibilities for an evolutionary interpretation of Kuhn's philosophy of science. However, Haufe's preferences for some pretty traditional divisions between what is internal and what is external to science, and his reticence in integrating some of Kuhn's theses into his own model due to these preferences, make it a less promising plan. At some points, the book is rather old-fashioned, and its reluctance to accept some of Kuhn's ideas and theories seem anchored in the 1970s. Yet, the benefits exceed the shortcomings, so I suggest a careful reading of this volume.

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Juan V. Mayoral
Universidad de Zaragoza
jmayoral@unizar.es

Notes

[1] Haufe's central thesis is often repeated; for example, see (pp. 190–91).

[2] Following Kuhn ([2012], pp. 154–55), Haufe mentions those periods of crisis when subjective and aesthetic values play some role in theory choice (p. 192). But what Haufe omits is that for Kuhn these values are important for some processes of revolutionary change: when a new candidate for a paradigm—and so for future directional advancement—cannot yet prove its problem-solving abilities.

[3] On the relevance of practices for Kuhn, see (Rouse [2003]).

[4] Chapter 9 includes a passing mention of Orr diagrams (p. 303).

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