



HAL
open science

Is the New Paradigm a New Paradigm? Comments on Knauff and Gazzo Castañeda

Igor Douven

► **To cite this version:**

Igor Douven. Is the New Paradigm a New Paradigm? Comments on Knauff and Gazzo Castañeda. Thinking & Reasoning, inPress. hal-03508558

HAL Id: hal-03508558

<https://hal.science/hal-03508558>

Submitted on 3 Jan 2022

HAL is a multi-disciplinary open access archive for the deposit and dissemination of scientific research documents, whether they are published or not. The documents may come from teaching and research institutions in France or abroad, or from public or private research centers.

L'archive ouverte pluridisciplinaire **HAL**, est destinée au dépôt et à la diffusion de documents scientifiques de niveau recherche, publiés ou non, émanant des établissements d'enseignement et de recherche français ou étrangers, des laboratoires publics ou privés.

Is the New Paradigm a New Paradigm?

Comments on Knauff and Gazzo Castañeda

Igor Douven
IHPST / CNRS
igor.douven@univ-paris1.fr

Abstract

Many cognitive psychologists have come to regard graded belief as fundamental to our understanding of how humans reason and many have also come to think of probability theory as providing at least part of the norms of correct reasoning. David Over has characterized this development as the emergence of a new paradigm in the Kuhnian sense. The target article argues that the choice of this term was unwarranted and also that it has done more harm than good. This commentary argues that there is nothing in Thomas Kuhn's work to suggest that he would object to Over's terminological choice and that there is no evidence that the choice has caused any harm.

Keywords: formal epistemology; logic; mental model theory; New Paradigm psychology of reasoning; probability; theory change.

Over the past thirty years or so, an increasing number of cognitive psychologists have become convinced that the notion of graded belief is crucial to our understanding of human reasoning. Concomitantly, they have come to regard probability theory rather than classical logic as providing the norms of reasoning.¹ David Over (2009) has described this development as a paradigm shift and has dubbed the probabilistic approach to reasoning the *New Paradigm psychology of reasoning*. In their target article, Markus Knauff and Estefania Gazzo Castañeda argue that this was an unfortunate terminological choice. According to them, it not only suggests that a development took place that was much more consequential and disruptive than the shift that actually happened, but it also hampered progress and needlessly antagonized workers in what Over would regard as the Old Paradigm.

Whereas Knauff and Gazzo Castañeda nowhere specify how big a shift in scientific theorizing should be to warrant regarding the result a new paradigm, from their discussion it does become clear that they think the New Paradigm is not a new paradigm because, first, it is *continuous* with what went before, and second, it has not *replaced* that “old” paradigm. In addition to this, they complain that we still lack a clear definition of the New Paradigm and as a result lack a clear understanding of what exactly sets it apart from supposedly more traditional approaches in the psychology of reasoning. They even claim that “[i]f it is just the abandonment of deduction [that is to distinguish the New Paradigm], then almost all current research . . . belongs to the new paradigm” (p. 35 of manuscript).

¹To be more precise: they have come to regard probability theory as providing the *static* norms of reasoning. The debate about the *dynamic* norms (i.e., the norms governing how to respond to the acquisition of new information) is still very much ongoing (Oaksford & Chater, 2013; Douven, 2021, in press).

How dramatic should a shift be to warrant talk of a new paradigm? Over used the term “paradigm” in reference to Thomas Kuhn’s (1962) groundbreaking work on the evolution of scientific knowledge. In his early work, Kuhn meant the expression *new paradigm* to refer to a scientific development that is so revolutionary that there is bound to be a breakdown in communication between scientists working in the new paradigm and the old guard. Even if they use the same words (e.g., *mass*, *time*, *velocity*, *energy*, and so on), these words mean different things in the two paradigms—an alleged phenomenon Kuhn called *meaning incommensurability*—and as a consequence practitioners in the two paradigms inhabit different worlds, a thesis that is sometimes called *conceptual relativism* (Putnam, 1981).

That, as said, was the early Kuhn. Implicit in his later writings is a much less radical, much more sane (many have argued), conception of *paradigm shift*. Indeed, there was no way for Kuhn to stick to his earlier ideas about meaning incommensurability and conceptual relativism. Meaning incommensurability derived from a positivist account of the meaning of theoretical terms. According to this, the meaning of a theoretical term is determined by its place in the theory, in particular by its connections to other terms occurring in the theory. So if a theory changes, at least if the change is drastic enough (e.g., if it completely changes our conception of how *mass* and *energy* are connected), the meaning of the terms changes with it. This view has long been debunked, with most philosophers of language now following Hilary Putnam (1975) and Saul Kripke (1980) in holding that the meanings of theoretical terms are at least partly determined by the causal connections they bear to their real-world referents. And with the positivist theory of meaning out of the way, there is no longer any pressure on the commonsensical idea that those real-world referents remain fixed across changes in theory, however dramatic these changes may be.

But even if physicists working before and after the Einsteinian revolution do not (necessarily) talk past each other, nor live in different worlds, no one will dispute that the shift from Newtonian mechanics to relativistic mechanics is big enough to justify calling it a *paradigm shift*. Is the shift that occurred in the recent history of the psychology of reasoning as big? Not even close. (What is?) I still do not see any harm in Over’s use of the term *paradigm shift* in connection with the advent of Bayesian and more generally probabilistic approaches in the psychology of reasoning. In particular, I do not see why the fact (as it certainly is) that there is continuity between these approaches and the more traditional, logic-oriented approach should conflict with Over’s terminological proposal, even taking the intended reference to Kuhn into account. Indeed, there is even more continuity than Knauff and Gazzo Castañeda seem aware of. Not only did mental modelers already “consider” and “discuss” probabilities—as the authors state at various junctures—New Paradigmers never abandoned deduction, contrary to what Knauff and Gazzo Castañeda suggest. How *could* they have done so, given that probability theory *builds* on deductive logic? One could even argue that deductive logic is a limiting case of probability theory much in the way in which Newtonian physics can be conceived as a limiting case of relativity theory: logic is what remains of probability theory under the idealizing assumption that all propositions have an extreme probability (ideally, we assign probability 1 to all truths and probability 0 to all falsehoods). As to the point that the New Paradigm has not replaced the Old Paradigm, note that it would be wrong to say that relativistic physics has replaced Newtonian physics when engineers across the globe rely on the latter on a daily basis.

While there may be these continuities, there is still a big difference between considering or discussing probabilities and taking the concept of probability to be fundamental to understanding human reasoning. (The quote from Philip Johnson-Laird, 1983, in the target article nicely underlines that mental modelers do not do so.) Similarly, whereas New Paradigmers

are not opposed to logic, nor have—to my knowledge—ever claimed that it is immaterial to reasoning research, they do believe that it falls short of providing a general enough, adequate framework for modeling reasoning. It is thus easy to distinguish between two broad research programs, one taking binary concepts such as, most notably, truth and (categorical) belief as fundamental and classical logic as the main normative framework, and the other taking probability and graded belief as fundamental concepts and probability theory as the main normative framework. Of course, that still does not quite settle the question of whether Over was right to designate these programs as opposing paradigms.

How *are* we to settle this question? I do not know of anything in Kuhn's writings, or in those of Kuhn's many commentators, that suggests a negative answer to it. On the other hand, Kuhn has often been criticized for not bothering to state in a precise manner what he means by *paradigm*. In view of this, the best I can offer is my own "feeling," which is that in answering the said question much depends on one's perspective. I could well imagine that a future historian of science, looking back at the psychology of reasoning in the late twentieth and early twenty-first century, will find the differences between the truth-and-logic-centered approach and the graded-belief-and-probability-theory-centered approach unremarkable. And indeed, these differences may pale in comparison with how both approaches differ from what is yet to come. Even so, someone who experienced first hand the shift from the one to the other approach—who lived through it, so to speak—may well have a different view and may find the term *paradigm shift* exactly right to describe the development that took place.

To buttress this point, I briefly recount a parallel development in my own main field of research, which is analytic philosophy. Until around 1990, the central concepts in epistemology were knowledge, justification, and (categorical) belief. Logic had an important place in this epistemology because it was generally held that justification was closed under logical derivability, the idea being that if, for instance, you are justified in believing A, and B follows deductively from A, then you are justified in believing B as well. (Obviously, it need not hold that if you believe, or know, A, and B follows deductively from A, you believe or know B as well; you may fail to see that B follows deductively from A.) Mainstream epistemologists did sometimes mention the notion of graded belief, or made an appeal to probability theory, but they did not see graded belief as being central to understanding human epistemology, nor did they conceive of probability theory as an important analytical tool in epistemology.

I am referring to mainstream epistemologists here because there was, at the same time, an underground epistemology, as Bas van Fraassen (1989) called it. This underground epistemology went back to the writings of the 17-th century logicians from the Port-Royal school (including Antoine Arnauld and Blaise Pascal) and did see graded belief as being more fundamental to understanding human epistemology. It also saw probability theory as providing the norms for (static) rational reasoning. (For the reason mentioned previously, this did not amount to a rejection of logic as being somehow irrelevant to rationality: probability theory builds on logic. Rather the idea was that the laws of logic on their own were informative of rationality only in limiting cases.)

But the underground epistemology gained popularity almost overnight, and just a bit more than ten years after van Fraassen's writing, Luc Bovens and Stephan Hartmann (2003, p. iii) could rightfully claim that "Bayesian epistemology is all the rage." What they called *Bayesian epistemology* not much later came to be more generally called *formal epistemology*, and under that name it has been thriving for close to twenty years now.

I cannot remember whether any of my colleagues ever referred to this development as a paradigm shift. However, having lived through the development, I do not think using that

term would be out of place. When I started teaching epistemology in the beginning of the 1990s, it would have been absurd to spend a class or two explaining the students probability theory in an introductory epistemology course, or to devote an entire advanced course in epistemology to the use of probabilistic methods. By the end of the 1990s, that was exactly what I and many of my colleagues were doing. In my experience, this was an exciting development which however for those of us who were raised in mainstream epistemology also required a lot of rethinking as well as delving into probability theory. (High school knowledge of probability theory is in general not nearly sufficient if one wants to contribute to formal epistemology.) Thus, I would not necessarily have objected if someone had called formal epistemology *New Paradigm epistemology*.

To be clear about this, I do not think anything would be gained by a name change to this effect. But also, no harm would be done. The Old Paradigm (the epistemology that continues to focus on the notions of knowledge, justification, and categorical belief) would not suddenly vanish. Indeed, there are quite a number of formal epistemologists who contribute to that paradigm as well, even if perhaps only occasionally. And those who do not will still look to the work from Old Paradigmers for inspiration, not to mention all the work that is going into trying to build bridges between the two paradigms (I am thinking here in particular of the work on the lottery and preface paradoxes; see the papers in Douven, Ed., 2021).

Whatever one's terminological preferences, the important point is that it would fundamentally have made *no* difference if what is now known as *formal epistemology* had been called *New Paradigm epistemology* instead. I am equally confident that it would have made no difference had Over called his 2009 paper "The probabilistic turn in the psychology of reasoning," or "Bayesian approaches to the study of reasoning," or some such. Suppose he had omitted all reference to Kuhn. Would mental modelers and (what are now called) New Paradigmers have been best pals in that case? Would there have been more open discussion among members of those groups? Did the open discussion noticeably diminish after the publication of Over's 2009 paper? Knauff and Gazzo Castañeda are right to complain about the animosity in reasoning research as well as about the lack of direct discussion. But I cannot find anything in Knauff and Gazzo Castañeda's paper to support the thought that any of this has to do with Over's use of Kuhnian terminology.

References

- Bovens, L. & Hartmann, S. (2003). *Bayesian epistemology*, Oxford: Oxford University Press.
- Douven, I., Ed. (2020). *Lotteries, knowledge, and rational belief: Essays on the lottery paradox*. Cambridge: Cambridge University Press.
- Douven, I. (2021). How explanation guides belief change. *Trends in Cognitive Sciences* 25: 829–830.
- Douven, I. (in press). *The art of abduction*. Cambridge MA: MIT Press.
- Johnson-Laird, P. N. (1983). *Mental models*. Cambridge: Cambridge University Press.
- Kripke, S. A. (1975). *Naming and necessity*. Oxford: Oxford University Press.
- Kuhn, T. S. (1962). *The structure of scientific revolutions*. Chicago: Chicago University Press.
- Oaksford, M. & Chater, N. (2013). Dynamic inference and everyday conditional reasoning in the new paradigm. *Thinking & Reasoning* 19: 346–379.
- Over, D. E. (2009). New paradigm psychology of reasoning. *Thinking & Reasoning* 15: 431–438.
- Putnam, H. (1975). The meaning of "meaning." *Minnesota Studies in the Philosophy of Science* 7: 131–193.

Putnam, H. (1981). *Reason, truth and history*. Cambridge: Cambridge University Press.
van Fraassen, B. C. (1989). *Laws and symmetry*. Oxford: Oxford University Press.