The Cognitive Structure of Scientific Revolutions by Hanne Andersen, Peter Barker, and Xiang Chen

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When Thomas Kuhn wrote *The Structure of Scientific Revolutions*, he identified both cognitive and social influences on scientific change. For him, 'naturalizing' the understanding of the history of science required attention to both domains. Yet Kuhn's large legacy, in spite of the many disciplines affected, left few scholars pursuing the specifically cognitive side of his approach. Further, as the authors of *The Cognitive Structure of Scientific Revolutions* note, there is today no 'Kuhnian School' among historians of science, nor do most philosophers of science regard his approach with more than a measured scepticism.

Andersen, Barker, and Chen (ABC, hereafter) seek to change this situation by presenting a cognitive formalization of some aspects of Kuhn's theory of scientific change. They argue that such an approach can revive Kuhn's utility for work in the history of science and in the philosophy of science. The formalization is based on recent work in cognitive science dealing with concepts and conceptual change in thinking. ABC's larger goal, Kuhn aside, is to argue that, as they state summarizing one of their case studies, 'cognitive factors are ineliminable in reaching a historical understanding' [98]. The book describes the approach in some detail, applying it to three case histories of scientific change to show its power. Two of the cases (19th century reclassifications of birds and the discovery of nuclear fission) are treated relatively briefly, and one, the Copernican Revolution, is examined in more depth.

All three authors are known for their substantial contributions to the history of science, and for cognitive-historical accounts of particle physics [e.g., Andersen 1996], the Copernican revolution [Barker

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1999], and the wave theory of light [Chen 2000]. All three have previously written about Kuhn [Barker, Chen, and Andersen 2003]. The present book unifies their overall argument and provides an extended rationale for their post-Kuhnian cognitive-historical approach.

The history of cognitive formulations of concepts is briefly treated in the beginning of the book. The traditional 'feature theory', that concepts consist of lists of defining attributes, was attacked beginning in the 1960s, first *via* Eleanor Rosch's 'prototype theory', in which concepts are organized psychologically by prototypes that abstract away less important detail (a robin is a 'better' bird than a penguin, hence closer to the prototype). Later extensions by experimental psychologists were based on Wittgensteinian family resemblances. The approach was similar to that originally used by Kuhn in *Structures* and developed explicitly in his later writings. Recently, one of these extensions, known as frame theory and exemplified in the work of the cognitive psychologist Lawrence Barsalou, has dominated discussion in cognitive science; this is the approach used by ABC to ground their formalization.

According to Barsalou, conceptual structures are organized by frames, organized layers of nodes that include attributes at one level, with attribute values at a subordinate level. Inter-node relationships are included (for example, a value of one attribute may constrain the values of another attribute), as well as levels of nodes that are not attributes. For example, a blueberry is a 'non-red fruit', but one must then recognize that 'blue' and 'non-red' are possible values of 'color', with 'blue'' being a subset of 'non-red'. Thus 'blue' and 'non-red' cross conceptual boundaries, tying 'blueberry' to 'color' in a (nearly) unique fashion and constraining the possible attribute values. There is a dynamic aspect to Barsalou's use of frames: what is activated in any single instance of a recalled concept will be affected by the context of its recall. Thus, the tie to the color of blueberries may be less active when the context involves discussion of the vitamin content of fruit, but more active in discussion of a graphic design for cereal boxes. Following Barsalou, ABC use a graphical way of describing frames; this permits an economy of description highlighting similarities and differences among multiple frame representations.

The presentation is initially organized in terms of the two briefer case histories, bird classification and nuclear fission. In each case, frame representations are used to resolve historical puzzles and to exemplify the use of frames in historical cognitive analysis. Thus, 17th century taxonomies of birds divided them into two classes, water birds and land birds, the former having webbed feet and rounded beaks, the latter clawed feet and pointed beaks. By the early 19th century, many species had been found that did not fit these categories. Thus, South American Screamers had webbed feet but pointed beaks. These anomalies were accommodated in the 1830s by Carl Sundevall, who added a new category, Grallatores, for birds like the Screamers. He also replaced water birds by Natatores (Swimmers) and land birds by Gallinae (Chicken-like). All three categories were distinguished by anatomical attributes which preserved some of the distinctions of the old taxonomy, but added others. Thus, Natatores were still distinguished from the Gallinae by the presence of webbed or clawed feet (as in the old taxonomy), but absence of a fifth secondary feather (a feature present in both of the other two categories) was now required for the Grallatores, to distinguish them from the other two categories. Note that the new taxonomy is consistent with the old, in that while new attributes and values are added, the overall structure of the frame is unchanged.

The nuclear fission case invokes frame differences at a more complex level. The discovery of nuclear fission in 1939 is generally attributed to Lise Meitner and Otto Frisch, who argued that anomalous results first observed by Otto Hahn and Fritz Strassmann in 1938 were due to the splitting of a uranium atom into fragments that corresponded to lighter elements. The chemist Ida Noddack had argued essentially the same thing in 1934 following similar observations, although her discovery never generated discussion and was ignored by the scientific community. ABC argue that their cognitive account can explain 'why the same community that rejected fission in 1934 accepted it in 1939' [4]. The explanation rests on the fact that two different frame representations were involved, one, familiar to chemists and used by Noddack, was based on the dynamics of chemical elements. The other, accepted by most physicists, was based on nuclear disintegrations and made no reference to chemical processes. Physicists would have had to revise their entire conceptual structure dramatically to accept Noddack's account and there was simply no way for physicists to make sense of her proposal, given that the framework for nuclear disintegration allowed no room for the chemical motivations that inspired Noddack's frame representation. By contrast, Hahn and Strassmann's finding [1938] that lighter elements seemed to be present as a result of uranium disintegration, provided a reason for attacking an otherwise acceptable attribute constraint without reworking the entire conceptual structure. Meitner and Frisch were then able to explain the changes in structure by using a theoretical model (the liquid drop model of the nucleus) which had developed in the meantime. From a cognitive point of view, it was the unique nature of Noddack's frame representation that allowed her to propose something which, however, physicists saw as incomprehensible because their frame for the same processes was structurally different. While a community can understand and accept additions to an older frame (as in the bird taxonomy example), adopting a new frame is much more difficult.

So far, there is not a huge difference between the frame analyses offered by ABC and a comparable analysis based solely on ideas laid out in Kuhn's later writings: 'The structures Kuhn described appear in real historical situations and operate in very much the manner he proposed' [41]. Specifically, Kuhn described concepts in terms of contrast sets in which similarities and dissimilarities constitute the objects of categorization (hence implying family resemblances among instances of the same concept). The newly discovered Screamers simply added to the set of such contrasts. In the case of nuclear fission, Noddack's proposal was in effect calling for

> a revolutionary change in the paradigm without providing an anomaly competent to create a crisis state in which a new alternative could mature.. . . Cases like these are evidence of the creation and elimination of opportunities to categorize entities that we have already suggested as the main characteristic of revolutionary change. [103]

With this grounding, and the demonstration that recent cognitive theories are roughly consistent with Kuhn's account of conceptual change (at least in some cases), ABC turn to a knottier issue, that of incommensurability among concepts. Using their formal frame analyses, they

> draw out various conclusions that Kuhn suggested but did not elaborate, for example that incommensurability varies in

> degree or importance and that the degree correlates with the position of a concept in a hierarchy. [104]

Here, the case study used is the Copernican revolution, 'an episode that Kuhn never treated satisfactorily' [104].

As is well known, Kuhn spent decades after the 1962 publication of *Structures* in modifying and changing his original statements about scientific change. In the cognitive domain, he dropped his initial reliance upon gestalt switches to account for such change in favor of more nuanced claims, ones that could bridge the gap between the social and the cognitive. By the end of his life, he adopted a language-based mechanism that could generate incommensurability among the scientific terms that designate 'natural kinds' (like 'gold' or 'poison'). Such terms form natural hierarchies, with the lowest levels consisting of items that are described by learned similarity and difference relationships. For this reason, a change in one of the lowest level concepts could generate a 'local' incommensurability, and hence there could be partial or total failures of communication among scientific communities trying to talk about the subject matter. For changes at higher levels of the hierarchy, even broader problems of incommensurability could occur—hence his implied notion of 'degrees of incommensurability'.

To contrast Kuhn's account with their frame account, ABC use the example of the concept of physical object. Before Copernicus, the two natural kinds, celestial object and terrestrial object, differed in that the former were changeless and endowed with perfect circular motion. No such bifurcation was possible after Newton, however, for whom both natural kinds were now physical objects. Celestial objects were described by a frame in terms of orbit center (possible values: star, planet, other), orbit shape (ellipse, hyperbola, other), distance, luminance, size, and so on. By contrast, the equivalent pre-Copernican frame specifies attributes such as path (with possible values of daily, proper, and retrograde), distance, luminance, and size. There is no way to map the values of orbit center onto those of path; the structure of the frames is simply different. Thus, the frames for physical objects pre- and post- Copernicus were different in structure and, hence, incommensurable. So far this is not much different from Kuhn's description. ABC note, however, that their frame account allows for more precise specification of where the problems arise. Kuhn's degree of incommensurability now becomes a set of questions about the nature of the structural difference: Is it among attribute values at a low level? Does it redistribute objects across category boundaries? Do the categories differ? At what level of the frame?

Much of chapter 5 of ABC's book is devoted to exploring such differences across various aspects of the Copernican episode. For example, before Kepler, most astronomical theory concentrated on a celestial object's angular position, in accord with the importance of the components of the path attribute of celestial objects (whether, in short, the path was a manifestation of daily, proper, or retrograde motion). After Kepler, angular position was no longer part of the frame; a Keplerian *orbit* (not an *orb*) was characterized by its shape and center, not its angular position in the sky. This generated incommensurability because it introduced new attributes *and* new values, rather than new attributes with the same set of values or new values for existing attributes. As a result, the Keplerian approach violated a fundamental principle, the 'no overlap principle', which states that no concepts divided by a superordinate in a hierarchy may overlap. The degree of incommensurability caused by such a change is measured by how high in the hierarchy it occurs. The incommensurability between Keplerian and pre-Copernican astronomy was, therefore, not so severe as that which occurred after Newton, which brought further change in the concept of physical object, a higher-level frame than that of orbit.

Chapter 6 explores the consequences of incommensurability within a single tradition. Here, ABC compare Ptolemaic conceptions with Copernican conceptions, with a focus upon the way equants were accommodated within each tradition. Ptolemy introduced epicycles and deferent points, primarily to account for retrograde motion while preserving perfect circular motion. Even so, the fit to the observed phenomena was not perfect; so he introduced equant points on a line connecting the Earth and the center of the deferent, and then postulated that the planets moved with changing velocity such that, as seen from the equant point, the motion appeared uniform. Equants had been a sticking point for Ptolemaic astronomers because, by positing a change in the angular velocity of a planet along a path, it proposed something that was increasingly seen as physically awkward. One advantage of the Copernican system was that it removed the necessity for equant points. Thus, by removing equants, Copernicus ironically made it easier to retain the Ptolemaic system—

> Copernican astronomy is not incommensurable with either the conceptual structure favored by the Averroists or the Ptolemaic alternative apart from the difficulties with the equant, which the Ptolemaic astronomers regarded Copernicus as having resolved. [146]

From this perspective, Copernicus did not represent a revolutionary change from Ptolemaic astronomy. Instead of Copernicus, the real revolutionary change occurred as a result of Kepler's work,

> the first sustained defense of Copernicanism in the modern sense: the sun played a real physical and geometrical role, and planets moved around it on paths that could be calculated from Kepler's new principles.... the first major incommensurability with earlier astronomy. [162].

The authors note also that Kepler's concept of an orbit was an *event* concept, not an *object* concept, and that this also may mark a major incommensurability. Event concepts, so recent research in cognitive science suggests, are organized very differently than object concepts, since they embody values that can vary over time. This creates difficulties for the frame representation approach, since events seem to require multiple frames for each successive time interval. And, experimental evidence from the cognitive laboratory (partly due to Barsalou and his colleagues) suggests that different processes are involved when event concepts are memorized, retrieved, and communicated. ABC suggest that some cognitive scientists regard these as 'mental models', although the discussion of this alternate view from cognitive science is too brief to be of use to the reader.[1] For some reason, ABC do not discuss the excellent paper by the third author, Xiang Chen [2003]. He showed that John Herschel's partial understanding of the wave theory of light could be explained by the fact that Herschel had an object concept of waves, one that was incommensurable with the prevailing event concept of waves that emerged after the work of Fresnel and others. While the difference mattered little in how the wave account of refraction and reflection were understood by Herschel, his grasp of the theory of polarization, which required an event concept, was partially erroneous. This example, if included in the book, would have gone far to show the reader that

[1] Those interested could start by reading the papers in Magnani and Nersessian 2002.

the frame theory approach can work as a cognitive historical explanation, even in an area where the cognitive processes involved are still not fully understood.

In the final, seventh, chapter, ABC provide a brief summary, followed by brief discussion of the implications of their approach for several controversial issues. In particular, they note that the problem of incommensurability has been softened or denied by realist philosophers of science such as Putnam. Under this view, incommensurability is not absolute, so long as referential stability is maintained. Roughly speaking, you and I do not have to have the same concept of cat to communicate about cats, as long as we agree on what specific instances correspond to the named term. In science, later theories can be better descriptions of entities, as long as the entities under discussion are the same. ABC, by contrast, argue that concepts refer to phenomenal realities, not to realities in the real world. While there are constraints on what concepts may be posited under their view, the constraints do not guarantee that there are corresponding entities in the world. Incommensurability can not always be resolved, nor can science be taken as resting on realist interpretations.

The issue is related to ABC's extended discussion of a proposal made by the sociologist of science David Bloor, whose famous manifesto for the 'Strong Programme' in the sociology of science dismissed the need for cognitive understandings of science. Instead, Bloor argued for a sociological approach to a causal theory of science. ABC argue that their approach refutes Bloor's thesis by showing that a cognitive account preserves all the requirements set by Bloor for an account of science, one that is truly causal, impartial, reflexive, and symmetric (i.e., with the same kinds of explanations accounting for both true and false beliefs). Further, their account, but not Bloor's, preserves a role for historicity in accounts of science. Thus, according to Bloor, ahistorical social factors ('interests', and the like) trump contextual factors—for him, history of science is a secondary part of a comprehensive theory of science, whereas, on the framework approach, it is only through the historical context that one can begin to understand the nature of conceptual change in science. In this way, the original intent of Kuhn is preserved and his relevance for the history of science can be re-established.

This is a short but very rich book, one that must be seen as a major contribution to historiography of science. Still, this reviewer was left wishing for more substance on some issues, and for broader reviews of other work that has taken a cognitive historical view of the history of science (much of this work is cited but only briefly discussed, if at all). For example, Howard Margolis [2002] has used 'habits of mind' as a construct to explain aspects of the Copernican revolution. There should have been discussion of how his proposal differs from that of ABC. One of ABC's occasional collaborators, Nancy Nersessian [2005] has explicitly used mental models to account for scientific change, and has related her work to Kuhn's. Again, discussion and comparison would have been welcome.

In the first chapter, ABC note that Kuhn's use of gestalt psychological principles to illustrate conceptual change was problematic. No number of 'duck-rabbit' perceptual reversals can actually bridge the gap between the cognitive and the social—conceptual change at the social level is not a matter of accumulating such instantaneous perceptual phenomena. Instead, it occurs across long periods of time and represents the outcome of extended processing within and among individuals. By rooting their approach in recent cognitive science work, ABC claim to have bridged this gap. That is, they argue that conceptual change at the frame level can be seen to occur among different individuals across long periods of time.

Still, a gap remains, but this time at a different level. All of the examples used in the book represent change at group levels, or, for individuals, as the result of analysis of finished work. Kepler, for example, changed his concept of orbit between writing the *Astronomia Nova* of 1609 and the *Epitome of Copernican Astronomy* of 1618–1622. The results of these changes are nicely described by ABC's use of frame representations, but a full account would need to examine more closely the reasons why Kepler himself changed his views and the processes that led up to them. A complete cognitive account thus requires a more detailed analysis of the 'microstructure' of thinking. Barring such completeness for at least some cases, the gap between the cognitive and the social is still not resolved. Kuhn was sensitive to this need, especially in his last writings, and previous papers by the authors of the present book show similar sensitivity. Including discussion of this large issue in the present book would have strengthened it.

In the end, does the proposal for a newly cognitivized Kuhnian approach work? Can it offer the historian of science a useful set of tools? For this reviewer (already among the 'cognitively converted') the answer is clearly 'Yes', though much remains to be done. Still, given its richness and the clarity with which the case is argued, this is a work which will have to be dealt with. Cognitive science does offer historians tools for a new approach to the history of science, one that would have pleased Kuhn himself.

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