Review
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Another way of expressing the general concept is that "a symmetry is a transformation that leaves all relevant structure the same" (p. 262; emphasis from original is dropped), which can include what are typically called laws, for example, conservation of momentum. *Covariant equations* satisfy many of the criteria for lawlikeness introduced earlier, but they are not necessary in any theory-independent sense. A covariant expression is one whose "truth value must remain invariant under all admissible transformations" (p. 287). But what constitutes an "admissible transformation" or, correspondingly, what are the *fundamental* "relevant structures" that must remain unaltered by a transformation, for example, distance in Galilean transformations, is specified by the theory being used. So symmetry principles give only theory-relative generality. Even if such theoretical context dependence is ignored, covariant expressions cannot play *all* the traditional roles given to laws.

Combining this characterization of symmetry as theory-relative with his model-theoretic view of theories, van Fraassen comes to the somewhat surprising conclusion, "We have indeed found a significant notion of true generality, but not one of necessity. And that significant generality pertains to our description of the structure of models, not the structure of nature"(p. 289). The latter claim is one that needs significantly more discussion than is given in this book. It is certainly true that symmetry arguments are applied to a class of models of a theory, but once what R. Giere (1988, 80) calls a *theoretical hypothesis*—a claim that a natural system is itself a member of that class, or at least *is isomorphic to* a member of that class—has been made, the results of symmetry considerations would seem to be equally true about nature. There are physical actions which are similar to transformations, and at least one way to use the concept of invariance is to say that certain physical relations will be unchanged when those actions are carried out. A fuller account is needed of how symmetry considerations apply *only* to "models" and not to "nature" given that the concept of a natural model has long been used by those advocating the semantic view of theories.

My account of van Fraassen's treatment of laws and symmetry does not do justice to the wealth of other ideas discussed in the book, such as the logical and ontological status of possible worlds, the relation between objective chance and personal probability assessments, or the semantic view of theories. This breadth is both a strength and a weakness. Through these wide-ranging discussions, van Fraassen gives the general reader access to specialized topics and provides useful connections among seemingly disparate subjects. But often the details of the larger thesis of the work—that law in the classical sense of a necessary grounding of regularity is unacceptable as a foundation for or analysis of science—are left for the reader to fill in. Still, van Fraassen clearly identifies the areas from which these details might be completely mined. Most importantly, van Fraassen provides the reader with a fine philosophical and scientific introduction to symmetry as an area of extreme importance for further work in philosophy of science. *Joel M. Smith, Claremont Graduate School.*

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THEO C. MEYERING. ***Historical Roots of Cognitive Science: The Rise of a Cognitive Theory of Perception from Antiquity to the Nineteenth Century.*** Boston: Kluwer (1989), xix + 250 pp. $69.00 (cloth).

Meyering's book rationally reconstructs the history of theories of perception from Aristotle to Helmholtz with respect to two fundamental problems: the philosophical problem of how perception can provide knowledge of an external world, and the psychological problem of specifying the processes by which perception occurs. His most general aim is to explicate the conceptual developments that made possible, or even necessary, the rise of "modern cognitive science" (p. xiii).

Meyering's thesis is that the rise of cognitive science was made necessary when the Aristotelian "identity theory"—according to which knowledge of the external world is made unproblematic by a kind of identity between the knower's mind and the known object—and the attendant absorption model of perception were replaced by the Cartesian theory of mind and knowledge. Meyering contends that Descartes's rejection of the identity theory created an epistemological dualism between "appearances" and "things in themselves", and, in particular, that the further denial of a complete "resemblance" between sensations and external objects necessitated the postulation of information-processing operations of an inferential and interpretive nature to bridge the gap between sensations considered as mere cues for or signs of external objects and perceptions that adequately represent those objects (pp. 71, 78, 87). He maintains that, for a variety of reasons, Descartes, Locke, Berkeley, and Hume failed to see the need for or were unable to posit the unconscious interpretive processes that this theoretical situation demanded, and that therefore it was left to Helmholtz to develop a proper information theory of perception (pp. 8, 79, 108). Moreover, Meyering finds that his historical tale of interaction between philosophical theories of knowledge and scientific—especially physiological and psychological—theories of perception supports the "naturalistic" thesis that philosophical problems are not given and solved a priori, but arise in interaction with empirical knowledge (pp. xv, 64).

The book should be admired for its articulation of a strong and clearly stated thesis about the historical and philosophical development of theories of perception from Aristotle to Helmholtz. I am myself doubtful of each of the major assertions alleged to support this thesis. Meyering's first supportive assertion, in the form of a reconstruction of ancient and medieval Aristotelian identity theory, falters on two important counts. First, his claim that the Aristotelian theory of perception portrays the acquisition of knowledge as unproblematic by postulating a direct transfer (p. 3) of essences from external objects to the mind vastly oversimplifies that theory. By contrast with his extensive discussion of ancient and medieval theories of vision, Meyering simply asserts that according to the Aristotelian account of knowledge essences of objects are received through the senses and conveyed into the mind (pp. 14–15). But if we take Thomas Aquinas or later scholastics such as Suarez and the Coimbrans as paradigmatic adherents of medieval Aristotelian epistemology, this description is highly problematic. These Aristotelians all agreed that the material phantasm does not itself enter the immaterial intellectual power; transmitted species are not "absorbed" by the intellect from the external world, but rather the agent intellect, as a result of the presence of the phantasm, creates an intelligible species (Aquinas 1911–1912, qu. 85, art. 1, ad 3; Coimbra College 1600, bk. 3, chap. 5, q. 3, 5, 6; Suarez 1856, bk. 4, chap. 2). Meyering is right that this species is alleged to be, in some sense, "identical" with the object knowledge, but this "identity" amounts to an identity in form and hence a "real resemblance". Second, his discussion of the identity theory in the important and influential work of Alhazen masks the continuity between Alhazen and later optical theorists, including Kepler and Descartes. His general thesis about the development of information theories of perception requires that Alhazen's theory posit no "gap" between what the senses receive and the accurate perceptual representation of external objects. Such might be the case for Alhazen's theory of the reception of color considered as a "real quality" (pp. 41, 43); here, received form and perceived quality are the same. Meyering, however, devotes most of his analysis to the perception of shape, and sees Alhazen's postulation of a "homomorphic" shape received in the crystalline humor as an instance of the identity theory (pp. 40–41, 44–53). Yet this shape is a two-dimensional cross-section of the so-called "visual pyramid", and hence is (essentially) geometrically equivalent to the retinal image as described by Descartes and Kepler (ignoring inversion; see Hatfield and Epstein 1979). Once the cross-sectional character of the received species is acknowledged, it becomes apparent that for Alhazen some process or mechanism is needed to bridge the gap between the received species and the perceived three-dimensional shape. Contrary to Meyering's thesis that the need for such processes was felt only after the discovery of the retinal image and the translation of visual physiology into Descartes's mechanistic idiom, Alhazen (1572, 30–61; Sabra 1978) and his medieval followers (Bacon 1928, vol. 2, 523–525, 530–532; Pecham 1970, 134–147; Witelo 1572, 111–113, 121–124, 130–133) discussed the need for such processes at some length.

The second major assertion of Meyering's rational reconstruction is the claim that Descartes, Locke, and Berkeley did not posit unconscious judgmental operations to mediate between the signs or cues in the retinal image and the perception of a world in three dimensions. Meyering is certainly correct that each author explicitly stated that all occurrent mental states are conscious. But exclusive attention to such pronouncements obscures important features of each author's position. Thus, Descartes repeatedly ascribed the perception of size and shape to judgements (1984–1985, vol. 1, 172–175), and explained that these judgements occur so rapidly they typically go unnoticed (ibid., 295). Descartes's unnoticed judgements are instances of mediating cognitive processes in perception. Locke, too, posited such judgements (1975, 145–147). And Berkeley posited rapid and unnoticed transitions between the proper object of sight (which does not include distance) and a viewer's usual experience of objects at a distance (1709, secs. 1–77), transitions he considered to be mediated by acquired patterns of "suggestion" between ideas (ibid., secs. 45, 51, 58; 1733, secs. 10, 42). These aspects of Descartes's, Locke's, and Berkeley's theories of vision are simply left out of Meyering's account.

A third major assertion is Meyering's contention that Helmholtz originated the information-processing account of perception by being the first to posit unconscious interpretive processes in perception. Meyering acknowledges that Hume had allowed mediational processes in perception, but he contrasts Humean mechanistic association with the genuinely cognitive and interpretive processes allegedly posited by Helmholtz (pp. 192–193). As I have said, Helmholtz was not the first to posit mediating judgmental processes, though he was among the first explicitly to label such processes "unconscious". Additionally, it may be doubted whether Helmholtz's position is different from Hume's in the way Meyering suggests. Helmholtz ascribes unconscious inferences to association, as Meyering observes (pp. 190, 205). Yet Helmholtz also talks of the perceiver as forming perceptual inferences through a process of active experiment and cognitive testing (pp. 184–190). Meyering finds Helmholtz to be inconsistent on this score (pp. 192, 224–226), and adopts the "active testing" account (pp. 192–199). Others might contend that Helmholtz's repeated appeal to association (1925, 26, 533–534; 1971, 213) deserves the greater weight. In any event, the thesis that perception is mediated by judgements of which the observer is not normally aware has a long history prior to its classical formulation in Helmholtz's writings.

More generally, the claim that Helmholtz's theory rendered possible the rise of cognitive science must be evaluated against a specific conception of cognitive science. Meyering describes cognitive science as "the conception of the mind as a physical symbol system with intricate representational capacities and unimaginably rich computational resources" (p. xiii). His wording indicates that he views cognitive science not merely as the cooperative study of cognition by psychologists, philosophers, linguists, and computer scientists—the rise of which might be explained by the rejection of behaviorism and the return to mentalistic models and modes of description by members of the first two disciplines, coupled with the emergence of the second two as distinct disciplines with interests in mental modeling—but rather as a topical area united by allegiance to explanations that posit internal symbols and computational processes. He thus identifies cognitive science with an approach to cognition driven by the "computer metaphor". Helmholtz can hardly be credited with "making possible" the rise of that conception of cognitive science, for despite his talk of sensations as "signs", he forsook the identification of perceptual inference with linguistic inferences (1971, 217–219); the characterization of internal processes as "computations" had no part in his conception of psychological processes. A rational reconstruction of the birth of the "symbol systems" approach to cognitive science should look to the fascination with the computer metaphor during the 1950s and subsequent influence of that metaphor in psychology, philosophy, and linguistics.

Meyering's thesis that his historical narrative supports philosophical "naturalism" and a "naturalistic epistemology" deserves brief comment. If by "naturalism" in epistemology one means simply that philosophical theories of sensory knowledge cannot reasonably be formulated independently of empirical knowledge of the functioning of the senses, and that in the past such theories typically have interacted with psychological theories, then naturalism should, I think, be endorsed without qualification. But this sort of naturalism must then be distinguished from the more radical "naturalized epistemology" of Quine (1969) and others, according to which epistemology is essentially a branch of psychology, and the vocabulary of analysis for cognition in general is that of natural science (say, the vocabularies of physics, chemistry, and neurophysiology). One could easily be a "naturalizer" of the first sort without subscribing to reductive naturalism of the second sort.

Despite my reservations about the book's central argument, I recommend the work to anyone interested in Helmholtz's theory of perception. For the scholar who has an independent acquaintance with the history of perceptual theory from ancient times, this book provides a boldly stated and challenging interpretation of the historical interplay between philosophy and psychology. Those who seek a first understanding of that history should use this book with care, supplementing its treatment of authors prior to Helmholtz with standard works that Meyering mentions (e.g., Lejeune 1948; Lindberg 1976) as well as those he does not (e.g., Hamlyn 1961; Pastore 1971); the book itself provides a useful comprehensive summary of Helmholtz's physiological, psychological, and philosophical program in the theory of perception, and of his theory of unconscious inference. *Gary Hatfield, University of Pennsylvania.*

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FRANK CLOSE. ***Too Hot to Handle: The Race for Cold Fusion.*** Princeton: Princeton University Press (1991), 376 pp. $24.95.

This is an engaging piece of science journalism, even if a bit repetitive at times. Frank Close, himself an active research scientist at Oak Ridge National Laboratory in the United States and at Rutherford Laboratory in Britain, presents a detailed recounting of the scientific and historical backgrounds leading to the dramatic announcement of "cold fusion" by Stanley Pons and Martin Fleischmann on 23 March 1989, and of the reactions by the scientific community and of the attendant legal and political intrigues. While the book provides a good read, one can well ask what of *philosophical* concern there warrants a review in this journal. That was my initial reaction when the editors asked for a review and it still remains, I believe, a difficulty. This is not said to imply the unimportance of questions such as *why* Pons and Fleischmann continued to behave as they did in the face of mounting evidence against their original claims. However, that can be seen as more a psychological than a philosophical question. And, besides, Close provides relatively little real insight into the personalities of the major participants in this debacle.

The basic facts are well enough known and need not be rehearsed here. These chemists claimed to have produced huge quantities of energy release (in the form of heat) with fairly simple equipment and at room temperatures and attributed this to a nuclear fusion process. The two most interesting aspects of Close's book, relative to the history and methodology of science, are his discussion of the historical precedents of work on cold fusion and his detailed presentation of the reactions, both pro and con, of the scientific community in the aftershock of Pons and Fleischmann's dramatic press conference release. (Incidentally, Close does an excellent job of providing elementary but correct accounts of the basic physics involved.)

The parallels between the cold-fusion incident and the case of René Blondolt with his N-rays in France in the early 1900s is by now widely known, both in the philosophy of science literature and in the semipopular physics journals. However, less well known is the historical background Close provides, including the work of two German chemists, Fritz Paneth and Kurt Peters, at the University of Berlin in the 1920s; the lavish project, funded by Peron in Argentina in the 1950s and headed by a German nuclear physicist named Richter, to produce controlled nuclear fusion in a huge electrical spark gap; the origin of the cold-fusion idea with Charles Frank at Bristol University in 1947; and the unfortunate, premature announcement of controlled nuclear fusion in 1958 by Sir John Cockroft, a Nobel Laureate in physics and director of Harwell Laboratory. But, in fact, these fascinating events actually had little or no impact on the recent cold-fusion story as it played out.

In retrospect, it is difficult to draw any conclusions other than that Pons and Fleischmann were careless in their work and announced stunning results based on little and flawed evidence. This was compounded by their refusal to admit their errors and to withdraw their (by then) baseless claims. The most obvious "heavy" in the entire episode is James Brophy, the vice president for research at the University of Utah, who pressed for early announcement by the chemists and who remained active at the center of the financial and legal intrigues that followed. But what has this to do with the methodology of science? Not much. What is relevant, though, is the way the scientific community studied the claims and eventually rejected them, in spite of an initial commitment, or at least a neutral curiosity, on the part of many investigators to corroborate the claims of Pons and Fleischmann. This *is* important if one wants to view science as a *social* phenomenon shaped by societal forces. For, while it is true that science collectively had a vested interest in the "received" views on nuclear fusion that it had established over the previous fifty years or so, it is also the case that there were great pressures, from industry, financial institutions and the public in general, to substantiate these claims. But it just could not be made to fly. While it is undoubtedly so that evidence does not *uniquely* determine a theory, it is as true that