

## Some Twists in the Cognitive Turn

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### 1. The Rhetoric of Cognitivism

Contrary to what the name suggests, the recent work of Howard Margolis (1987) and Ronald Giere (1988) demonstrates that the “cognitive turn” in the philosophy of science is *not* simply the application of cognitive science to the study of science. For one thing, neither one is what Jerry Fodor (1981) has called a “methodological solipsist,” that is, someone who wants to account for thought processes without presupposing an account of the world of which those thoughts are about. Margolis, in fact, comes perilously close to Fodor’s anti-cognitivist foe, J. J. Gibson (1979), whose “ecological” perspective requires that an organism’s thought processes be specified in terms of structures in the environment, “affordances,” capable of satisfying the organism’s desire to know. The Margolian focus on overcoming “barriers” to alternative “habits of mind,” as the psychic basis of Kuhn’s paradigm shifts, only serves to highlight this broadly “functional” side of thinking that typically has no place in cognitive science. Although more methodologically eclectic than Margolis, Giere is also more the solipsist in suggesting that the interesting story to be told about how conceptual change occurs—one which requires the resources of the neuroscientist, psychologist, and computer simulator—transpires entirely within the head of the individual scientist. Consequently, Giere says virtually nothing about the character of the environment (natural or social) such that conceptual change proceeds as it does. The big difference, of course, is that whereas Fodor and some cognitive scientists still suppose that solipsism implies apriorism, or at least innatism, Giere does not.

In what sense, then, are Margolis, Giere, and many of the people they cite taking a *cognitive* turn? I would say that it is in a quite conservative sense, insofar as their turn is biased toward findings and interpretations that support the image of the scientist as a competent, largely self-sufficient human agent. Consequently, they downplay research pertaining to the cognitive limitations of individuals, especially the failure of individuals to appreciate the context-dependence, and hence global inconsistency, of their thought and action. Moreover, our cognitivists underestimate the cognitive power that is gained via group communication and technological prostheses. But in the course of displaying these biases (cf. Fuller 1989, for the opposing biases), the cognitive turn has brought to light important philosophical issues—“metaphysical”

ones, as Giere himself now suggests—that previously eluded philosophers of science (cf. Fuller forthcoming). They pertain to the bearers of scientific properties: Where in the empirical world do we find knowledge, theories, rationality, concepts?—to name just four philosophical abstractions hitherto left in ontological limbo. Margolis and Giere are clear about arguing for the individual scientist as the relevant locus. Their focus is “cognitive” in the familiar sense of being concerned more with the individual’s thought *processes* than with the *products* of her thought. That is probably because neither challenges the idea that these processes produce the sorts of things that more traditional philosophers of science would regard as having “cognitive content,” such as theories. As a result, while the cognitive turn tends to give us a full-blooded sense of what theorizing is like (e.g. a pattern of neural activation), we are still left with a rather pale, abstract sense of what theoretical output is like. For example, I suspect that different “styles” of theorizing radically underdetermine the types of theoretical texts that are written, yet it was those texts that initially led philosophers and historians to believe that there was something cognitively special about science.

## 2. Some Mistaken Identities

My own perspective on the cognitive turn in the philosophy of science is very much like Marx’s on the capitalist turn in the history of political economy. In capitalism, relations among people are mistaken for properties of things. What Marxists mean by this claim is that goods do not have an inherent value, or natural price, but only an exchange value that is determined by the social relations among the capitalist, worker, and consumer. Likewise, I believe that, in its attempt to locate abstractions in the empirical world, cognitivism mistakenly identifies (1) rational reconstructions for actual history, (2) properties of groups for those of individuals, (3) properties of language for those of the mind, and (4) properties of society for those of nature. I will consider each in turn.

(1) Like Piaget’s genetic epistemology, the Margolian account of paradigm shifts as the overcoming of cognitive barriers is more pedagogy than history of science. In other words, teachers could use Margolis to get students to see beyond the shortcomings of their current framework to a more comprehensive one—but only once that next stage of comprehensiveness has already been achieved by the scientific community. His is a method for meeting standards rather than setting them. Margolis’ confusion here probably stems from his insensitivity to the normative dimension of Kuhn’s account of scientific revolutions. In particular, unlike the way it is used in politics, where it makes sense to speak of “failed revolutions,” all of Kuhn’s revolutions are success stories. That is, the only cognitive changes that he recognizes as “scientific revolutions” and “paradigm shifts” are the ones that moved scientists closer to our current paradigms. Beyond that, Kuhn has little to say about how such revolutions occur, for that would involve accounting for a variety of individuals, most with interests quite distinct from those of the original revolutionary, who nevertheless found that person’s work of some use for their own. Thus, Margolis mistakes reconstructed history for the real thing because he typifies in one individual a process that is better seen as distributed across a wide range of individuals.

(2) This last point is worth emphasizing, as it brings into focus the simplistic sociology that often informs the cognitive turn. Kuhn is more to blame here than either Margolis or Giere—especially his tendency to characterize scientists as having a common mindset or worldview, which, in turn, makes it seem as though, for a given paradigm, once you’ve seen one scientist, you’ve seen them all. Sociologists regard this typification of the group in the individual as a methodological fallacy, the “oversocialized conception of man” (Wrong 1961). The problem with the conception is

that, in attempting to account for the social dimension of thought, it actually renders the social superfluous by ignoring how the interdependence of functionally differentiated individuals makes it possible for a group to do certain things that would be un-doable by any given individual. Philosophers are prone to an oversocialized conception of people because of bad metaphysics. They tend to treat a part-whole relation as if it were a type-token one: to wit, society is an entity that emerges from the arrangement of distinct individuals, not a universal that exists through repeated instantiations. Indeed, I am inclined to think that the signature products of cognitive life—knowledge, theories, rationality, concepts—are quintessentially social in that they exist only in the whole, and not in the parts at all. For example, it is common for cognitive psychologists to treat conceptual exemplars, or “prototypes,” as templates stored in the heads of all the members of a culture, when in fact they may be better seen as concrete objects that function as public standards in terms of which the identities of particular items are negotiated (cf. Lakoff 1987). It may well be that each party to such a negotiation has something entirely different running through her mind, but their behaviors are coordinated so as to facilitate a mutually agreeable outcome.

(3) Continuing in the spirit of the last remark, if one is looking for an account of the brain that starts with minimal common capacities and then builds up quite different neural networks, depending on an individual's experience, one need look no further than the promising array of parallel distributed processing (PDP) models. However, contrary to what Giere seems to think, I believe that the extreme context-sensitivity of PDP models implies that whatever sustained uniformity one finds among members of a scientific community *cannot* be due to any uniformity in their thought patterns, but rather to some uniformity in the public character of their behavior, especially the language in which members of that community transact business. (In fact, that might be the *point* of scientific language.) For, if PDPers are correct about the variety of neural paths that can lead people to say, do, and see roughly the same things, then I take that to be an argument for the nervous system *not* providing any particular insight into the *distinctiveness* of science as a knowledge producing activity. (Of course, PDP would still say a lot about “how we know the world” in the looser sense of successfully adapting to the environment.) But even if one were to find this conclusion outlandish, it remains to be seen whether the cognitivists have a story to tell about scientific communication, the means by which findings are ultimately judged to be normal, revolutionary, or simply beside the point. From works such as Nersessian (1984), which Giere cites approvingly, it would seem that communication is the process by which a later scientist reproduces an earlier scientist's thought processes in order to continue a common line of research. However, if thought is as context-sensitive as PDPers suggest, then it is unlikely that this story could be literally true—especially if the relevant thought processes are defined in terms of what we now, only in retrospect, regard as a “common line of research.” And even if a later scientist wanted to pursue an earlier scientist's work, it is not clear that either her means or her motives would involve the reproduction of that work (cf. Wicklund 1989). My guess is that the “concept maps” and other heuristics that cognitivists elicit from scientific texts are more formal analyses of scientific rhetoric that conveyed the soundness of the scientist's work than representations of “original” scientific reasoning that readers followed step-by-step in their own minds. This is by no means to demean the accomplishment, but simply to put it in perspective.

(4) Finally, perhaps the grossest sociological simplification behind the cognitive turn may be termed its “visually biased” social ontology: to wit, social factors operate only when other people are within viewing distance of the individual; if no one is in the vicinity, then the individual is confronting nature armed only with her conceptual wiles. The solitary laboratory subject working on psychological tasks—the

source of much of Giere's evidence—certainly reinforces this image, but Margolis is the bigger offender in failing to see that cognitive patterns are memories of socially framed experiences, which are resistable and replaceable only in socially permissible ways. The project of altering one's point of view, not merely for the sake of entertaining the alternative, but for making the alternative the basis of one's subsequent research, involves the simultaneous calculation of what philosophers have traditionally called "pragmatic" and "epistemic" factors. This serves to bind "the social" and "the natural" in one cognitive package that cannot be neatly unraveled into, respectively, impeded and unimpeded thought processes. Relevant to this point is the *Machiavellian Intelligence Thesis*, recently proposed by two Scottish animal psychologists (Byrne & Whiten 1987). They argue that cognitive complexity is a function of sociological complexity, such that the organisms which respond to environmental changes in a less discriminating fashion tend to be the ones with a less structured social existence. One conclusion that Byrne and Whiten draw is that the complexity of nature distinctively uncovered by science may be little more than a reflection of the combination of people who must be pleased, appeased, or otherwise incorporated before a claim is legitimated in a scientific forum. A more simply organized science would, then, perhaps reveal a simpler world.

### References

- Byrne, R. and Whiten, A. (eds.) (1987), *Machiavellian Intelligence*. Oxford: Oxford University Press.
- Fodor, J. (1981), *Representations*. Cambridge MA: MIT Press.
- Fuller, S. (1989), *Philosophy of Science and Its Discontents*. Boulder: Westview Press.
- Fuller, S. (forthcoming), "Naturalized Epistemology Sublimated," *Studies in History and Philosophy of Science* 22.
- Gibson, J. J. (1979), *The Ecological Approach to Visual Perception*. Boston: Houghton Mifflin.
- Giere, R. (1988), *Explaining Science*. Chicago: University of Chicago Press.
- Lakoff, R. (1987), *Women, Fire, and Dangerous Things*. Chicago: University of Chicago Press.
- Margolis, H. (1987), *Patterns, Thinking, and Cognition*. Chicago: University of Chicago Press.
- Nersessian, N. (1984), *Faraday to Einstein*. Dordrecht: Nijhoff.
- Wicklund, R. (1989), "The Appropriation of Ideas," in *Psychology of Group Influence*, P. Paulus (ed.). Hillsdale NJ: Lawrence Erlbaum, pp. 393-424.
- Wrong, D. (1961), "The Oversocialized Conception of Man," *American Sociological Review* 26: 184-193.