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ROBERT A. RUBINSTEIN

Belief in Science[1]

Introduction

What is the epistemological status of anthropological and other social science knowledge? How does that knowledge grow? In what ways is it conditioned and shaped by the social contexts of investigation? How is it shaped by our cognitive capacities as a species? These are among the topics with which Rik Pinxten has grappled during his career (Callebaut and Pinxten 1987; Pinxten 1983; Pinxten 1984; Pinxten 1997). Whether dealing extensively with Navajo cosmology (Pinxten, et al. 1983) or with the scientific study of religion generally (Pinxten 2010), he has argued for a science of human experience that recognizes the double nature of the anthropological encounter. He recognizes the dynamic and interactive nature of anthropological research, and he views ethnography as essentially 'double-biased,' involving intercultural discourse, and the construction of theory (Pinxten 1997: 10).

In this paper, I wish to engage with the themes Pinxten treats by giving an anthropological perspective on 'belief in science.' On its face, the domain 'belief in science' is an oxymoron. After all, it might be argued, science is about the investigation of the empirical world in ways that are designed to eliminate belief from our thinking. Anthropologists study people, their lives and the ways they make meaning of their experiences. Thus, from an anthropological perspective, to speak of the 'belief in science' is not oxymoronic at all. I say this because science is the aggregate and collective activity of people who recognize themselves and are recognized by others as belonging to a discipline that undertakes the systematic study of a particular class of phenomena (Rubinstein, et al. 1984: xvii-xix). Through their activity and experiences these people form social institutions that affect the local communities in which they live. These institutions also have broader influences as well.

As Leslie White pointed out, to the anthropologist 'Science is not merely a collection of facts and formulas. It is pre-eminently a way of dealing with experience' (White 1938: 369). For me, then, 'science' must be understood by studying the activities of scientists. This requires treating the study of science as we would the study of any other human institution or activity. That implies that our analysis must be limited by what we know are the structure, operation and functioning of human cognitive systems, and also by our understanding of the diversity of ways in which humans organize their social lives and understand and symbolize their lived experience.

The project that results from this approach is a kind of 'naturalizing' of history and philosophy of science. Earlier, my colleagues Charles D. Laughlin, Jr., John McManus and I sketched how such a naturalizing might be accomplished for some traditional issues in philosophy of science, including the modeling of explanation, theory reduction, induction, and paradigm shift (Rubinstein, et al. 1984). We concluded that programmatic study with the understanding that scientists' internal images of the world play a greater role in scientific activity than is traditionally credited.

When we published *Science as Cognitive Process* in the early 1980s, the idea that the analysis of science should be studied in this way was still not widely accepted. In the ensuing two decades naturalized analyses that focus on the 'social construction of science through practice (by examining how scientists actually work and what they do)' have become more the norm in Science Studies. The focus on the social contextual nature of science and the linking of scientific knowledge with power and social discourse has led to the wide popularity of extreme epistemological relativism (Bloor 1991; Latour 1987) and an anthropology of science following this line of analysis has developed as well (Rabinow 1999; Traweek 1992). Although this view has been extremely influential, especially beginning during the 1990s, it is beginning to be called into serious question (Bloch 1998; Koertge 1998).

I think that this is a healthy development. Here I agree completely with Pinxten (1997: 6) when he writes:

> 'To my eye the provisional result of this critical movement again seems to be twofold: a genuine critical search on the epistemological status of ethnography is finally being engaged in, and a more or less vague epistemological relativism is being forwarded as a respectful alternative stand in anthropology (against both the old "scientific" and the naïve phenomenological approaches). The first result is an important one, I think. …The second result is a category mistake and indulges in an illusionary pretense of philosophical sophistication.'

Like Pinxten, I do not believe that to argue that science is constrained by our human capacities, and is social in an essential way, means that we must necessarily conclude therefore 'everything is equal.' While knowledge is contingent, it seems clear that some knowledge claims can be characterized as better than others (for some purpose). In *Science as Cognitive Process* we explored some of the ways that human cognitive processes play a role in producing more adequate data and explanations. In this paper, I continue this exploration, by focusing on several aspects of cognitive processes and beliefs in relation to the day-to-day activities of investigators and to the status of scientific knowledge in society.

Transforming Bears and the Dawn

Nearly forty years ago, T.F. McIlwraith told the following story about his fieldwork among the Bella Coola of British Columbia:

> 'An informant described casually an incident in which a bear had changed into a stump and added that a white man living in the valley had seen this happen. I know the white man and asked him about it. He remembered it and laughed. He had been in a canoe with five Indians when they all "saw" a bear, which, when they paddled closer, turned out to be a stump. His explanation was an optical illusion; that of the Bella Coola, a transformation' (McIlwraith 1964: 183).

What should we make of this report? Did the bear transform? Is this a matter of optics? Were the Bella Coola informants merely enjoying themselves at the anthropologist's expense?

At about the same time as McIlwraith related the story of the transforming bear, Norwood Russell Hanson was calling attention to the role of language and perception in the development of scientific knowledge. Hanson (1958: 5) says: 'Let us consider Johannes Kepler: imagine him on a hill watching the dawn. With him is Tycho Brahe. Kepler regarded the sun as fixed: it was the earth that moved. But Tycho followed Ptolemy and Aristotle in this much at least: the earth was fixed and all other celestial bodies moved around it. *Do Kepler and Tycho see the same thing in the east at dawn*?'

Hanson suggests that how one answers this question opens a rich field for exploring concepts of scientific observation and explanation, to which he devotes *Patterns of Discovery*. What one makes of the transforming bear and the breaking day, and how one approaches the questions raised by them depends on what one believes to be possible and it depends also upon the investigatory tools considered useful and reliable.

Cognized and Operational Environments

The principal function of the central nervous system is to model the world in which we live. Indeed, what we take to be 'real' is what is included in our models. I don't want to say that just because we don't include something consciously in one of our models of reality it does not exist; that it's not part of our environment. To help us talk about this my colleagues and I have developed the distinction between what we call the Operational Environment (Eo) and the Cognized Environment (Ec).[2]

The Operational Environment consists of the world. The Cognized Environment consists of all of the information about the world that is modeled by individuals and groups. We proposed seven possible ways that the Ec/Eo might fit with one another (Rubinstein, et al. 1984: 25-27). These are summarized in Table I, below.

Table I: Some Relations of Isomorphism in Eo/Ec Processes

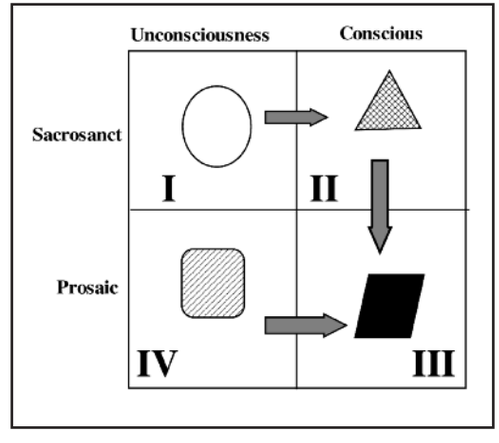
1. Complete	Total mapping of Eo by Ec
2. Partial	Incomplete mapping of Eo by Ec
3. Adaptive	Eo/Ec 'fit' is sufficient to lead to production of offspring
4. Developmental	Accuracy of Ec mapping of Eo continually adjusted by equilibration
5. Diaphatic	Ec models repetitive shifts in Eo by recurrent shifts in its structure and content
6. Self	Ec models of an individual's own organism as an element in the Ec (the 'cognized self'
7. Secondary	The fit between Ec models of Ec models

Axes of Belief

Together with Hahn (1973) I take beliefs to be the class of cognitive and emotional intuitions about the world that people take to be true, although unlike Hahn I treat some beliefs as not consciously held (see also, Carey 1992). In this sense they form the premises for thought and action, and are often based on the reported experiences of others.[3] Beliefs in this sense are the foundational stuff of cognized environments. Our interpretation of the physical world is shaped by our beliefs. For example, our ability to admit the transformation of a bear into a tree, or of the sun to be the center around which the earth orbits, depends upon what we believe to be true about bears and the sun.

When seeking to understand the role of belief in science, it is useful to conceptualize belief as having two axes. I call these the 'Sacrosanct – Prosaic' and the 'Unconscious – Conscious' axes. Sacrosanct beliefs are those beliefs that are not open to question, while prosaic beliefs may be questioned. Unconscious beliefs are those that one holds outside of their conscious awareness, while conscious beliefs are those about which one is aware. Figure 1 shows the four kinds of belief that are produced by the interaction of these axes.

Figure 1: Axes of Belief and Science



The growth of understanding of the world takes place when lived-experience is related to the beliefs in Cell III of Figure I and we modify our cognized environment models.

For science the process is similar, though it is formalized in ritual activities. (See Rubinstein (1984) on the ritual nature of scientific activities.) Lived experience becomes the activity inherent in the conduct of studies – experimental, observational or introspective – that are intended to evaluate and extend the formal cognized environment model held by members of a discipline. In this circumstance the outcomes of the studies will be used to elaborate further the disciplines models of the world.

These investigations are tacitly informed by the three other kinds of belief represented in Figure I. The development of an individual's understanding of their world and their continued investigation of that world depend upon their making unconscious beliefs available for conscious manipulation, and on making sacrosanct beliefs open to questioning and critical examination. In this model, Cell III represents the domain in which science takes place.

Moving beliefs from Cells I, II, and IV into Cell III, and thus to making them the subject of systematic empirical investigation, depends upon human cognitive activities. Such shifts may happen when one confronts unexpected results or when there is an emotional crisis. But, in general, and as Kuhn (1970) pointed out, such shifts face of great resistance, and may take place rather haphazardly unless practices are developed to facilitate such migration of beliefs (Duranti 1997: 65-68; Laughlin, et al. 1990: 212-240).

Cognitive Process and Science

Anthropological studies of science regularly find that scientific activity is firmly rooted in universal cognitive capacities. This includes systematic classification and theory change. These studies also frequently conclude that for science to create a better fit between a discipline's models and the domain they are investigating, researchers need to make part of their practice the regular examination of the beliefs that inform their work. This includes examining the data collection tools, categories and inferences that a discipline uses (Atran 1998; Giere 1988; Laughlin, et al. 1990; Rubinstein, et al. 1984). Special attention must be given to improving the operation of the scientist *qua* instrument of investigation because unexamined cognitive processes lead us to submerge these beliefs rather than surface them for examination. Here I discuss briefly some of the ways in which beliefs are submerged in daily cognition.

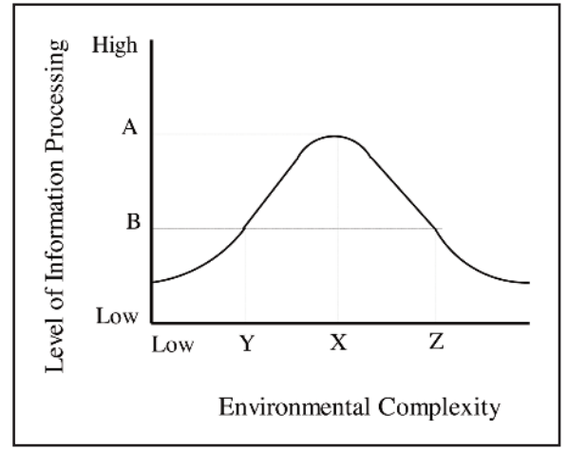
Observational and social psychological studies show that people develop strong commitments on the basis of limited experience. This is one of the origins of ethnocentrism, which exists among nearly all human societies (LeVine and Campbell 1972). Once such beliefs are formed, they become the basis of the cultural principles in accord with which people act (Laboratory of Comparative Human Cognition 1978).

One of the features of these processes is that beliefs survive data that should lead to their modification or abandonment. Indeed, such negative data can even

strengthen belief. Sometimes belief is held so strongly that it continues to be held even after the initial evidentiary base is shown to have been faulty. Anthropologists observe these 'mistakes of reason' among non-Western and pre-literate peoples. As well, they are observed among Western and literate peoples, making these 'mistakes' kind of a human universal (Piattelli-Palmarini 1994; Shweder 1979). Experiments designed to test for the occurrence of these phenomena in daily life have consistently produced empirical evidence that shows them to be very common (for instance, Ross and Anderson 1982). Equally, those who have been concerned about how scientists react to anomalous data have found that data are interpreted with a 'confirmation bias.' Data that do not support scientists' theoretical expectations can be discounted or used to support their model of the world (DeMonbreun and Mahoney 1976; Mahoney 1976a; Mahoney 1976b).

These results should not be surprising. Science is organized cultural activity that symbolizes the world through language and practice (Richter 1972). Indeed, observations in the history of science suggest that the taking of negative data as actually confirming for a theoretical perspective is all too frequent (Kuhn 1970; Lakatos and Musgrave 1970).

Figure 2: Information Processing and Environmental Complexity



The relationship between environmental complexity (made up for instance by, stress, multiple options, and high stakes, among other components) and the quality of information processing can be described by an inverted U-curve, as shown in Figure 2. This is a robust finding in studies of human information in laboratory and

field settings. By these accounts, the quality of information processing increases with an increase in the complexity of the environment in which decisions are being made. But, there is a point after which increasing environmental complexity results in decreases in the quality of information processing (Baron 2005; Chen, et al. 2009; Laughlin and Brady 1978; Schroder, et al. 1967).

There are many factors that may make the environment in which a scientist works more complex at some times than at others. Among these may be the pressure of fulfilling research grant commitments, whether the work is in an area that is controversial, the bearing of the research on the central tenets of a theory (rather than on more peripheral aspects), the ability of a research to 'save the phenomenon' in the face of negative evidence, and the public nature of the work. These are only some of many factors that may affect the environment in which information processing takes place. (Mahoney (1976b) lists other conditions that may affect the level of information processing and commitment to a position.)

The need for scientists to be alert to these factors and to develop ways of offsetting their effect has been the focus of some work. Suggestions include developing contemplative traditions within the Western sciences, instructing scientists to pay particular attention to the 'epistemological window' created by unexpected results, and the development of computational models drawn from artificial intelligence to identify and resolve anomalies. These suggestions lie along a continuum from those that propose addressing the problem by focusing on the internal cognitive processes of scientists on the one extreme (Laughlin, et al. 1990; Mahoney 1976b; Rubinstein, et al. 1984; Zukav 1979) to those that place the solution completely external to scientists (Darden 1992).

Belief and (Local) Biology

From 1989 until 1994, I worked with a multidisciplinary team on projects seeking to deal with problems of eye disease in Egypt. During that time we had occasion to work in villages in the Egyptian Delta and to work with drug company officials in the design of clinical trials. Interacting with the latter group, I learned something of the power of belief in relation to biological science. (The following account is drawn from Rubinstein 1998.)

In my interviews, I was repeatedly told by drug company officials, themselves Egyptian, that in-country clinical experience is important for establishing appropriate Egyptian dose ranges. Echoing lay assertions that Egyptians are a physically

robust and biologically strong population, drug company officials told me that particularly for anti-hypertensive, anti-psychotic and antibiotic drugs Egyptians require larger doses than were used in the West. The history of clioquinol in Egypt is illustrative.

Clioquinol, introduced as Enterovioform by Ciba in 1934, and marketed as a treatment for diarrhea was linked convincingly in the 1970s to SMON (subacute-myelo-optico-neuropathy), a condition characterized by irreversible paralysis, blindness and sometimes death. It was removed completely from the U.S. market in 1972 (Silverman, et al. 1992), after damage suits brought against the company and Japanese government were settled for nearly $900 million.

The Egyptian minister of Health was alarmed by reports of SMON. He convened a committee of experts to advise him on the matter. Despite acknowledging that clioquinol products caused problems *elsewhere* the committee concluded that the Egyptian population differed significantly from others. As a result of this privileging of local biology, the committee concluded that rather than withdrawing clioquinol from the market, what was needed for Egypt was a continued, inexpensive supply of such products. Thus, for instance, 'In December of 1982 a committee of Egyptian experts … met to pool findings for the minister. Egyptian patients, they reported, were consuming about 70 tons of Enterovioform and other clioquinol-like products each year. No matter the harmful effects these substances might have had in Japan, they had done no harm in Egypt. They were generally recognized as safe and effective — and appreciated as relatively inexpensive. The committee recommended that drugs containing clioquinol should continue to be allowed on the Egyptian market' (Silverman, et al. 1992: 23).

Enterovioform was still sold in Egypt as recently as 1985. In 1993 I purchased the clioquinol compounds, Enteroquin, Entocid, and Neo-Enterocin in pharmacies in Cairo and Alexandria. None of the package inserts for these products contained warnings about the neurological damage that had been documented elsewhere (Lane 1985).

A second case speaks to belief in anthropological discourse. In the early 2000s anthropologists and human biologists engaged in a dispute that caught the attention of the public. James V. Neel and his colleagues (including anthropologist Napoleon Chagnon) were accused by the journalist Patrick Tierney and by other anthropologists of starting a measles epidemic among the Yanomami Indians (Tierney 2000). All agreed that Neel and his colleagues administered Edmonston B, a measles vaccine, to Yanomami Indians. Tierney and other accusers alleged that this act started

the measles epidemic among the Yanomami. They argued further that this was an intentional act designed to allow the testing of human genetic hypotheses and that genocide was anticipated. In the discussion (much of it on the internet) that followed this accusation, two sharply divided views emerged. On the one hand, a number of socio-medical scientists have asserted that while the attenuated live virus that is in the Edmonston B vaccine might cause difficult side effects; it could not cause the disease. On the other hand, others argue that the vaccine *might have been* the source of some of the disease, and they assert that they (or others they consider reliable) observed that to be the case.

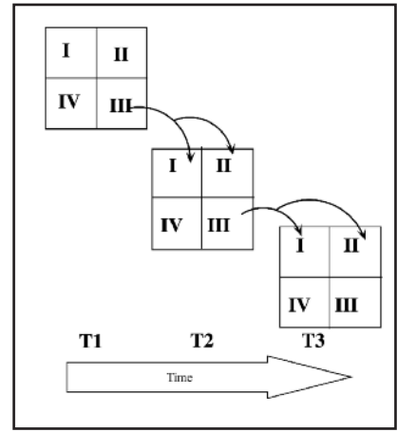
What is of interest in this discussion of belief in science is to notice that how these two 'camps' approached the discussion. It was framed by their belief in the results of biomedical science. For one group, the non-disease causing property of the vaccine was a kind of sacrosanct belief. Their response to the controversy was framed from this perspective. For the other group, the non-disease causing property of the vaccine is a prosaic belief, and their argument proceeds from that position. The result was that in this conflict what counted as evidence, how that evidence was understood, what counted as proper inference from it was different for each group.

Cognitive Stasis and the Development of Science

Troublesome situations can only be dealt with effectively once they have been defined as problems. In most areas relying on specialized knowledge, the ability to construct problems from the analysis of troublesome experience develops through practice. For professional researchers, problematic situations are constituted by data that depart from the expected. Such anomalous results may be dismissed, as they too often are. Or alternatively they may be taken as a site for profitable investigation.

To understand the problem that anomalies pose for science as a social institution we must understand something of the contribution of belief to the status of scientific knowledge in society. Prosaic-conscious belief is the domain in which science works. The results of those activities produce useful theories and applications. When these have become well established through experiment or repeated observation, they stop being a matter of curiosity and become settled fact. At some later time these become taken for granted. As Figure 3 shows, they join the class of cognitive and emotional intuitions about the world that people take to be true.

Figure 3: Development and Belief in Science



Once this happens, earlier contestable knowledge achieves the status of belief. As for the individual scientist, these beliefs inform the cognized models collectively accepted by the discipline. In addition to the cognitive processes that lead to the discounting of anomalous data, the collective activities of the discipline (lab work, periodic meetings, awards, grants, and so forth) add a social dimension that praises support of these beliefs and punishes departure from it. Indeed, may even circumscribe what is taken to be possible and legitimate. The social nature of this process reinforces the strength of the pressure to see confirmation of theory in data.

In other words, the construction of problems is tacitly framed by the beliefs that have become taken-for-granted as fundamentals of the discipline (Schön and Rein 1994). We must then encourage scientists — including anthropologists — to investigate more critically these frames – these fundamental tenets of their discipline. They need to be encouraged to reframe by changing the conceptual and social setting in relation to which unanticipated data are experienced. These data need routinely to be placed in another frame which fits these troublesome data equally well or even better. Doing so will thereby change the entire meaning of these data (Watzlawick et al. 1974: 95).

Barbara Ehrenreich reports an exchange that helps to underscore the importance of such an open critical stance. She writes:

> 'When social psychologist Phoebe Ellsworth took the podium at a recent interdisciplinary seminar on emotions, she was already feeling rattled.

> Colleagues who'd presented earlier had warned her that the crowd was tough and had little patience for the reduction of human experience to numbers or bold generalizations about emotions across cultures. Ellsworth had a plan: she would pre-empt criticism by playing the critic, offering a social history of psychological approaches to the topic. But no sooner had the word "experiment" passed her lips than the hands shot up. Audience members pointed out that the experimental method is the brainchild of white Victorian males. Ellsworth agreed that white Victorian males had done their share of damage in the world but noted that, nonetheless, their efforts had led to the discovery of DNA. This short-lived dialogue between paradigms ground to a halt with the retort: "You believe in DNA?"' (Ehrenreich and McIntosh 1997: 11).

Do you?

Only by being in a position that takes questioning the foundational beliefs of a discipline as a legitimate enterprise will we be able to respond to such skeptical queries in a responsible way. By paying attention to the cognitive processes that bring belief into our science, we can begin to design social systems for science that support the questioning of belief and the further improvement of our models of the world.

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2 We drew the terms originally from Rappaport (1968) but have expanded and altered his meaning somewhat. Our work on the Eo/Ec distinction can be found in Laughlin, McManus and D'Aquili (1990) and Rubinstein (1984).
3 Laughlin, McManus and d'Aquili (Laughlin et al. 1990: 227) say that there are three levels of knowledge the first of which is belief: 'which is the introductory, catechismic level where knowledge is restricted to the symbolic expression of cosmos committed to memory.'

EDEL MAEX

Openheid.
Over zen, religie en Darwin

Religie of spiritualiteit

In deze tekst kies ik ervoor om, tegen de actuele conventies in, toch de term religie te gebruiken in plaats van spiritualiteit. Het grote voordeel van 'religie' is dat niemand weet wat het juist is en dat iedere definitie ervan onmiddellijk door iemand anders tegengesproken wordt. Zelfs over de etymologie ervan bestaan verschillende opvattingen. De bruikbaarheid van het woord bestaat er blijkbaar in dat iedereen een vaag idee heeft van waar het over gaat en het vervolgens naar believen zelf invult. Ook dit soort woorden moeten er zijn.
Daarenboven heb ik grote bezwaren tegen de term spiritualiteit. Enerzijds lijkt die te verwijzen naar iets geestelijks, aan het aardse en het lichamelijke ontstegen. Ik ben een te aardse en lijfelijke mens om met deze beperking te willen leven. Daarenboven lijkt de term spiritualiteit te passen in een (post)moderne opvatting van het individu en te verwijzen naar een individuele ervaring los van de gemeenschap. Ook in die beperking kan ik mij niet vinden.

Vragen

Drie vragen vormden voor mij de aanleiding tot dit betoog.
Ik heb het voorrecht om in de lerarenopleiding van Groep T als keuzevak boeddhisme te mogen doceren. In de discussie met studenten kwam de vraag naar boven: zijn religies (levensbeschouwingen, erediensten...) nodig in een samenleving?
In de zengroep die ik begeleid, wierp iemand, naar aanleiding van een gesprek over seksuele schandalen in verschillende tradities (ook boeddhistische), de vraag op: Hoe is het mogelijk dat het *Requiem* van Fauré ooit geschreven is? Het had ook Bachs *Mattheus-Passion* kunnen zijn of John Coltrane's *A Love Supreme*. Ondanks al-