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SCIENTIFIC RATIONALITY AND HUMAN REASONING*

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The work of Tversky, Kahneman and others suggests that people often make use of cognitive heuristics such as availability, salience and representativeness in their reasoning and decision making. Through use of a historical example—the recent plate tectonics revolution in geology—I argue that such heuristics play a crucial role in scientific decision making also. I suggest how these heuristics are to be considered, along with noncognitive factors (such as motivation and social structures) when drawing historical and epistemological conclusions. The normative perspective is community-wide, contextual, and instrumental.

1. Introduction. Experimental work by Tversky, Kahneman and others on the psychology of belief change shows that human decision making under conditions of uncertainty is frequently guided by a few general heuristics such as salience, representativeness and availability (see, for example, Nisbett and Ross 1980 and Kahneman, Slovic and Tversky 1982). These heuristics differ from the normative guidelines of logic, probability and confirmation theory which are typically thought of as constitutive of rationality. In fact, the experiments suggest that human reasoning does not even *approximate* such norms of rationality, either in method or results. The purpose of this paper is to find and implement an appropriate strategy for assessing the epistemic import of these experimental results. I will especially examine reasoning in scientific inquiry. I make use of a concrete case—the development and reception of continental drift theory in geology this century—to show the operation of these heuristics in scientific reasoning and begin their epistemic assessment.

2. The Heuristics and Their Normative Assessment. The experimental results are familiar, so I will simply summarize them here. Subjects

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weight salient and available information more heavily in their decision making than normative models from probability and confirmation theory suggest that they should. Factors making data salient include concreteness (the detail, even irrelevant detail, with which things are described or actually experienced), proximity, emotional interest and perceptual biases. Factors influencing availability include biases in exposure and attention to data, biases in memory retrieval, and salience. The heuristics of salience and availability are also responsible for the phenomenon of belief perseverance (confirmation bias). Individuals hold onto beliefs at least partly[1] because memory retrieval and perceptual attention operate in such a way that the evidence for beliefs already in mind is made more available and salient, and therefore weighted more heavily, than the evidence against them. The representativeness heuristic underlies, among other things, causal reasoning. Subjects assume that similar events have similar causes, and reason informally, without considering base rates or the scientific relevance of the similarities found.

What implications do these kinds of data have for assessment of human rationality? It is useful to mention what others have said before introducing my own views. Psychologists conducting the experiments occasionally make provocative remarks, such as that the experiments show "bleak implications for human rationality" (Nisbett and Borgida 1975, 935). Usually, however, they express a qualified optimism about human rationality. Tversky and Kahneman, for example, insist that the heuristics "lead to severe and systematic errors", but qualify this by saying that "in general, [the heuristics are] quite useful" (Kahneman, Slovic and Tversky 1982, 4). Nisbett and Ross (1980) go further, suggesting that while the heuristics lead us into "profound, systematic and fundamental errors, [such failings are probably] closely related to, or even at unavoidable cost of [our] successes" (pp. 6, 14). They compare human reasoning strategies with human perceptual strategies, likening error to perceptual illusion. Just as we do not regard our perceptual system as inherently faulty even though we sometimes have visual illusions, they argue, we should not regard our cognitive processes as inherently faulty or lacking in rationality because they sometimes produce error. The heuristics often produce results in agreement with what may be obtained by formal statistical methods, or results as much in agreement with them as matters for ordinary human purposes (Nisbett and Ross 1980, 14, 60).

Furthermore, Nisbett and Ross claim that the error brought about by the heuristics can be reduced by teaching appropriate statistical models and by working in groups where one individual who uses the appropriate statistical model can correct others. Some error even cancels other error—when the biases are in opposite directions. And some error is to the advantage of the individual; for example, an overpositive evaluation of one's own abilities can be positively motivating. (Shelley Taylor 1989 has explored the connection between positive egocentric bias and motivation.) Psychologists do not, however, think that all error is likely to be eliminated or even turn out to be useful, positively motivating, error. Some bias and error are both ineliminable and undesirable (Nisbett and Ross 1980, chap. 12). Further experimental work suggests that this is so for groups of people working together as well as for individuals (see, for example, Hill 1982).

Some philosophers have reacted to the provocative negative statements that psychological researchers have occasionally made about human rationality. In various ways they have argued that, despite the experimental findings of error, humans must be reasoning rationally. For example, Jonathan Cohen (1981) argues that an account of human intuitions about rationality cannot, in principle, show that humans fail to be rational. And Daniel Dennett (1987) insists not only that humans must reason with a great deal of success to have survived the harsh evolutionary tests that they have, but also (as Davidson claims too) that humans must be regarded as rational if they are to be regarded as having beliefs at all. These and similar general defenses of human rationality have been effectively criticized by Stephen Stich (1985; 1990, chaps. 2 and 3), and I will therefore not discuss them here.

The more fruitful philosophical discussions are closer to the data. A general defense of human rationality is not really the issue, even if it worked. The considered view of Tversky and Kahneman, Nisbett and Ross, and others involved in the experimentation on human decision making is not general skepticism about human reasoning. Regarding such skepticism, Nisbett and Ross adopt the rejoinder "If we are so dumb, how come we made it to the moon?" (Nisbett and Ross 1980, 249). Such rejoinders do not, however, answer important questions concerning the nature and implications of particular departures from Bayesian strategies. Humans sometimes employ Bayesian strategies, and sometimes reason using heuristics such as representativeness, availability and salience. Current domain, prior experience and education determine which strategies are used on each particular occasion. Often, inappropriate strategies are used. (Osherson 1990 argues similarly that human departure from Bayesian strategies must be taken seriously, and assessed by looking at these departures in detail.) My central concern is to find where heuristics are employed in *scientific* reasoning, where they lead to bias and error, and what the consequences are of such bias and error.

Psychologists have for the most part investigated error in everyday rea-

[1]There are other causes of failure to change beliefs under positive and negative undermining. Availability and salience are thought to be the "cold" cognitive causes; there are also "hot" motivational causes such as pride and wish fulfillment.

soning, rather than scientific reasoning. Although they occasionally use scientists as subjects, they rarely test them in their professional domains. Faust (1984) discusses the existing data on cognitive bias in scientific practice. It largely comprises cases of cognitive error in medical and psychiatric diagnosis and confirmation bias in peer review of scientific work. Faust only speculates (and encourages research) on the presence and implications of cognitive bias in other aspects of scientific practice such as theory development, experimental evaluation and choice between competing theories.

Philosophical literature contains some discussions of cognitive bias in scientific reasoning. For example, Cherniak (1986), Giere (1988), Goldman (1986), and Harman (1986) discuss the degree to which the heuristics are error-prone and how they contribute to scientific success. I will mention and criticize some of their views in the course of my own discussion. It turns out that their views are seriously incomplete or incorrect.

Scientific reasoning differs from ordinary reasoning in a number of relevant respects. Heuristics which are successful in ordinary domains may not be successful in scientific domains. For example, although vividness and concreteness might be a rough indication of causal importance in the ordinary world, they are not in some scientific domains. (One such case is that electric and magnetic fields are neither vivid nor concrete, yet they play an important role in causal explanations of electromagnetic phenomena.) Scientific reasoning often has different goals from ordinary reasoning. For example, ordinary reasoning can often be assessed simply in terms of its effects on motivation, but this is rarely so for scientific reasoning where an important goal is to correctly explain and describe a domain. Statistical reasoning is often explicitly adopted in place of the heuristics, more so than for ordinary reasoning. Scientific work is done in groups of individuals more frequently than is ordinary reasoning, giving opportunities for correction of some errors as well as opportunities for creating error of other kinds. For all of these reasons, the area of scientific reasoning requires separate scrutiny. We need to see whether the heuristics are operative in scientific decision making and if so when, and with what results.

Philosophers of science adopt a variety of approaches to normative questions. Some see no room for the normative in an account of scientific change, or identify the normative with what is licensed or negotiated by the scientific community. Feyerabend's views and much work in sociology of science (e.g., Bloor, Barnes, Collins, Pinch, Latour, Woolgar) are examples of these views. Others locate the normative in a more general human context. Giere, for example, claims that a decision which satisfies the various interests—scientific and social/personal—of the individual scientists is a rational, normatively appropriate one (Giere 1988).

Others draw on traditional ideas about the nature of scientific rationality, modifying them to make them historically supportable and psychologically realistic. For example, Paul Thagard (1989) and Gilbert Harman (1986) each offer an account of scientific rationality in terms of coherence of belief. None of these accounts of what is normative in human decision making captures the sense of "normative" important for philosophy of science. Methods might be socially negotiated, or satisfy individual interests, or even involve settling for the most coherent organization of an individual's beliefs. But that leaves open—as an empirical question—whether or not, and where and where not, our methods are conducive to scientific success. This question, in my view, is the normative question of most interest to philosophers of science. Scientific rationality is thus viewed instrumentally.

No particular definition of success is presupposed here. All that is needed is the generally accepted list of scientific successes, which includes favorite examples such as the Copernican revolution, Newtonian mechanics, Harvey's discovery of the circulation of the blood, the oxygen theory in the chemical revolution and plate tectonics in the revolution in geology. Of such cases, the question is asked, is what scientists do related to the successes that they sometimes achieve? (If our intuitions about which accomplishments are scientific successes change, the domain of the question will change in tandem.)

I will argue that cognitive bias and belief perseverance on the part of geologists during the geological revolution was, contrary to what might be expected, in fact conducive to scientific success. This was so because bias and belief perseverance made possible the distribution of research effort, and this in turn led to the advancement of the debate over drift. The project is new in two ways. First, this normative framework for evaluation of reasoning is not generally adopted by philosophers of science.[2] Second, historians and philosophers of science have not considered whether heuristics such as salience and availability are important determinants of scientific decisions. Those working in the Kuhnian tradition have focused on social, motivational and institutional factors contributing to scientific change. New work in experimental psychology, cognitive psychology and neuropsychology supplements and challenges explanations in terms of social, motivational and institutional factors. Philosophers and historians of science are already making use of the work of Herbert Simon and his associates on discovery and reasoning, and also the work of Rumelhart, McClelland and others on parallel distributed processing (see, for example, Pat Langley et al. 1987 and Ronald Giere 1988 for work inspired

[2]David Hull (1988) and Larry Laudan (1987) are exceptions who adopt the same normative framework as I.

by Simon's ideas and Paul Thagard 1989 as well as Paul Churchland 1989 for work inspired by connectionist theories). This paper shows the need to also draw on the work of Tversky, Kahneman and others on human decision making under uncertainty to account for scientific decision making and change.

3. The Revolution in Geology. The theory of continental drift was not widely accepted in geological circles until the mid-1960s. Contractionism (associated with stabilist, permanentist views regarding the locations of continents) was the dominant view and expansionism (allowing only minor continental movements) had a small following. However, individuals devised, developed and were committed to the theory well before then. Alfred Wegener ([1915] 1966) was the first to develop a mobilist (drift) theory.[3] He was first impressed by the geometrical complementarity of coastlines on either side of the Atlantic, taking this as an indication that the continents on either side might have originally been fused as one (Gondwanaland or Pangaea). The idea deepened and took root when he learned of the paleontological and geological similarities on either side of the Atlantic, and when he realized that climate changes over the earth's history could be explained by postulating drift. Wegener hypothesized that the mechanism for drift of continents is similar to the mechanism for drift of large polar icebergs (Wegener [1915] 1966, 37).

The most serious difficulty with Wegener's theory was that there was no known force of sufficient magnitude to move the continents over the ocean floor. Wegener acknowledged this, writing that "The Newton of drift theory has not yet appeared" (Wegener [1915] 1966, 167). He made various suggestions about possible causes of drift, such as centrifugal forces, but none of these turned out to be of sufficient magnitude to account for drift. However, as Wegener pointed out, contractionism (the leading theory) also had serious geophysical difficulties. There is no physical evidence for the past existence of most land bridges or connecting continents, although contractionists proposed that they had existed. Also, ocean floors were known to be composed of a different and denser kind of rock than the rock composing continents, which makes it difficult to regard ocean floors as former continents. Furthermore, if the ocean floors had once been continents, the earth would have been flooded, and there is no geological evidence for this.

Several creative and successful geologists adopted and developed Wegener's theory from the 1920s onward. Although they were in the minority in espousing drift, their contributions were influential. This situation should be contrasted with the situation after the mid-sixties, when there was a vocal minority *opposing* drift, but this minority was *not* composed of geologists working creatively and successfully.

[3]Drift theories were proposed earlier than this (e.g., by Evan Hopkins, Richard Owen, Antonio Snider-Pellegrini, Osmond Fisher, W. H. Pickering, H. B. Baker and F. B. Taylor) but Wegener was the first to put forward a seriously developed theory.

Those adopting drift were disproportionately geologists working with southern hemisphere materials (for example, DuToit, Van der Gracht, King, Molengraaf, Hutchinson, Baker and, later, Carey), and geologists working on orogeny (for example, Argand and Staub). Wegener's work was the most strongly rejected in the United States where low professional activity (measured as low rate of publications) was correlated with acceptance of drift (Stewart 1990, 235).

During the 1950s and 1960s, new data bearing on drift came from unexpected sources: the emerging fields of oceanography and paleomagnetism. As the data grew, oceanographers and especially investigators of paleomagnetism, became convinced of the drift hypothesis. Blackett and Runcorn, for example, who worked on geomagnetic reversals, adopted drift in the mid 1950s (LeGrand 1988, 141–143). In 1962, Hess proposed a mechanism for drift called "sea floor spreading": The continents ride passively on seafloors which spread out from deep sea ridges and sink in troughs, powered by convection currents (see LeGrand 1988, 197). This is essentially the plate tectonics model, which has since been generally accepted. Plate tectonics is a long way from Wegener's theory that the continents ride over the seafloors. Nevertheless, Wegener's hypothesis of drift is clearly the ancestor of our current drift theory.

Hess's proposal was mostly ignored at first. Hess himself stated it with some trepidation, claiming that it was "geopoetry" in need of greater confirmation (ibid., 197). However, Hess did get an appreciative audience in Fred Vine at Cambridge. Vine, as a schoolboy, had been impressed by the geometrical fit of continents, and he was readily influenced by Hess into belief in mobilism. Drummond Matthews, also at Cambridge, and later Vine's supervisor, was persuaded of the truth of mobilism during his travels and observations in the Falkland Islands. Vine and Matthews used new magnetic data—the linear magnetic anomalies around deep sea ridges—to argue for seafloor spreading (the Vine-Matthews hypothesis; see Vine and Matthews 1963), and their work was crucial in the confirmation of Hess's hypothesis of seafloor spreading. Further work on linear magnetic anomalies produced in the Lamont laboratory at Columbia (a stabilist stronghold) convinced the Lamont hierarchy of mobilism. By the late 1960s, the revolution in geology was over.

This discussion shows the distribution of belief in drift before the mid-sixties. Those working on southern hemisphere materials, orogeny and paleomagnetism were much more likely to be drifters than those working in other areas. Wegener and, much later, Vine were both newcomers to geology when they adopted drift. Others were dismissive of drift, even though other theories had serious difficulties. Few individuals actively working on the drift debate suspended judgement (J. Tuzo Wilson is a notable exception).[4] Such distribution of belief occurs frequently in scientific communities. The Copernican Revolution, the Chemical Revolution, nineteenth-century evolutionary biology, the history of quantum mechanics and the recent debate over cold fusion are just a few sources of numerous examples. In the absence of compelling evidence for one position, people actively working on a problem often adopt opposing positions and develop and defend them over time with considerable conviction. This distribution of belief leads to distribution of cognitive effort (i.e., different individuals work on different theories) and the consequent development of competing theories. The scientific debate is advanced and, historically, has often been settled. Distribution of belief is thus conducive to scientific success.[5]

4. Explaining the Revolution in Geology. What caused the distribution of belief in drift? Clearly, geologists were not all following "the scientific method", whatever that turns out to be. Before the mid-sixties, the best judgement seems to be that no one theory was clearly superior, yet many successful geologists took sides. Frankel (1978), Rachel Laudan (1978), Paul Thagard and Gregory Nowak (1990) and others who argue that it was most rational to choose contractionism before 1960 tend to dramatize the problems with drift and downplay the problems with contractionism. They also fail to acknowledge the importance of early work on drift or explain why some successful geologists adopted drift theories and others rejected them.

Others working on the geological revolution have emphasized the role of social, institutional and personal factors in determining the decisions of individual geologists. Historians and philosophers such as Stewart (1990), LeGrand (1988) and Giere (1988) have argued that such factors as pride, "local interests", variation in "cognitive resources", as well as "scientific" or "rational" considerations, influenced scientific decision making. They subscribe to a traditional platonic view of the mind, where decisions

[4]Rachel Laudan has discussed this case (R. Laudan 1980). She shows that Wilson considered contraction, expansion and mobilist theories of the earth's crust, and suspended judgement on the truth of each during his investigations. Wilson's methodology is the exception rather than the rule. I explain why below.

[5](1.) Some (for example, R. Laudan 1980) might say that it would be better if individuals acted more like Tuzo Wilson, and avoided commitment to any theory before success of one theory was apparent (this is the "method of multiple working hypotheses", first expounded by Chamberlin [1890] 1965). However, they do not, and it is doubtful that they could be trained to do so in general. As stated above, I am asking the question, "How is scientific success in fact possible?" I say more about this in section 5.

(2.) No one knows exactly what belief is, or what the different shades and kinds of belief are. I am particularly interested in the kind of conviction that leads to action.

are determined in a competition between two processes—rationality and the emotions.

The details of their accounts are disappointing. No more is given in explanation of, for example, Matthews's, support of drift than his "local interests" and cognitive resources (expertise) due to his travels in the southern hemisphere. Matthews had little personal investment in the data that persuaded him to accept drift, and indeed the data were well known, not a special resource of his (Raymond Adie, an advocate of drift, had suggested that he repeat some measurements of Devonian sections in the Falkland Islands). We are not told how such accounts based on interests or cognitive resources would be spelled out, either for Matthews or for many others who worked with southern hemisphere materials, on orogeny or on paleomagnetism. The accounts might be developed and improved. However, this is not the only option, and indeed it is hard to see how convincing accounts could be given, given the resources of folk psychology on which the strategy rests. I present a competing account, and explain several decisions of geologists in terms of cognitive factors alone, without appeal to interests or scientific (rational) factors. I acknowledge that interests sometimes play a role in decision making, and I will comment on this later. Sometimes, also, formal statistical and evidential techniques play a role in scientific decision making, but this does not feature importantly in the revolution in geology. Here, then, is an account of the distribution of belief and effort during the revolution in geology, understood in terms of the heuristics of availability, salience and representativeness.

Wegener was not trained as a geologist, but as a meteorologist. He did not begin with the stabilist doctrines of the time, so he held no entrenched prior beliefs. Belief in mobilism began when he contemplated the geometrical congruences on either side of the Atlantic. Belief perseverance phenomena took over: Confirming data from geology, paleontology and meteorology were eagerly assimilated, despite the claims of geologists that these data were ambiguous and could be explained in other ways. The data were made more available because of agreement with a hypothesis already in mind. Meteorological data were especially available, owing to Wegener's early training. Wegener's reasoning that the motion of continents through ocean floors was physically possible in the way that the motion of icebergs through water is physically possible is an example of the use of the representativeness heuristic, and the salience (because concrete) of Wegener's 1906 observations of icebergs in Greenland increase the strength of this inference using the representativeness heuristic. This use of the representativeness heuristic is not a piece of convincing scientific reasoning—the situations of icebergs and continents are not shown to be sufficiently similar to support the conclusions drawn.

The paleontological and geological similarities observed between currently separated continents in the southern hemisphere are well explained by the hypothesis of drift. Other theories account for the same data with more difficulty. So it is to be expected that those working on southern hemisphere materials—for whom the data were salient, because concrete—would take the drift hypothesis to be more strongly confirmed. The data from the southern hemisphere were widely *known*; the issue here is how the data were *weighted*, and salience affected that.

In Switzerland, Emile Argand led a fairly general acceptance of Wegener's work. Before Argand even read Wegener's global theories, he proposed local horizontal continental movements to account for Alpine structures. When he became aware of Wegener's work, he immediately adopted drift, even though anti-German feelings were running so high in Switzerland at that time (1916–1917) that it was forbidden to read any material printed in Germany (Carozzi 1985). I suggest that Argand's prior view that there were horizontal movements and forces sufficient to create the structures of the Alps lent credence, via the representativeness heuristic, to the thesis that there were even larger horizontal movements and forces—large enough to cause continental drift. Furthermore, I suggest that the salience (because concreteness) of the data on the formation of the Alps led to weighting this argument for drift more heavily. The situation in Switzerland should be contrasted with that in other countries with anti-German sentiment. For example, in France there was little concern with orogeny and drift was rejected (Carozzi 1985).

Most geologists rejected drift in favor of the prevailing contractionist theory. I have already disputed the claims of Frankel and Laudan that this was the rationally appropriate decision. Other historians have explained the rejection of drift in terms of social and motivational factors. They note that the pattern of rejection and acceptance of drift comes in national and sometimes institutional groups, and hypothesize that the interests of a few powerful individuals dictated their attitude toward drift, and then these individuals influenced groups. So, for example, Chamberlin's remark at the 1926 AAPG symposium on drift "If we are to believe Wegener's hypothesis we must forget everything which has been learned in the last 70 years and start all over again" (Waterschoot van der Gracht 1928, 87) has been widely interpreted as a confession by a powerful geologist that personal investment in stabilism influenced the rejection of drift. Certainly, Chamberlin's remark is consistent with that interpretation. However, it is also consistent with the interpretation that Chamberlin saw all the data as fitting into a framework that supported stabilism; that is, it is consistent with the interpretation that belief perseverance phenomena were responsible for his, and others', rejection of drift. (Indeed, on the basis of this remark alone, even a traditional confirmation theory

understanding of Chamberlin's decision is possible.) Few geologists in the United States worked on southern hemisphere materials or the formation of folded mountain structures like the Alps. The important data in support of drift was not salient for them. Hence, the general rejection of drift is to be expected for cognitive reasons: It is a belief perseverance phenomenon.[6] Cognitive considerations also explain why those geologists with low publication rates were more likely to accept drift; their beliefs were less entrenched (cognitively speaking) than those who had reasoned more and produced more, so belief perseverance was less of an impediment to acceptance of drift.

Geologists (such as Blackett and Runcorn) working on paleomagnetism during the 1950s and early 1960s concluded that continental drift, along with magnetic reversals, were together the best explanation of variations in the earth's magnetism. The data were salient (because concrete) for them, leading them to weight the data heavily in their decision to accept drift. Others only read about the data and so they were not salient for them. They regarded the data skeptically—more skeptically than it deserved—emphasizing experimental problems in the new field of paleomagnetism. Belief perseverance phenomena probably came into play here, as well as personal interests.

Vine and Matthews, for different reasons, were predisposed toward drift. Vine had always been impressed by the geometrical congruences, and was only an undergraduate when he heard Hess's seafloor spreading hypothesis. Vine, as Wegener, came to drift without any prior cognitive commitments, and belief perseverance phenomena reinforced the initial belief in drift. Matthews had worked with southern hemisphere materials, which were therefore salient to him, biasing him toward drift. Vine and Matthews produced new paleomagnetic data taken from the ocean floor surrounding the deep sea ridges. They argued that the linear magnetic anomalies found were best explained by Hess's model of seafloor spreading. Their paper was, however, largely ignored—again, at least partly a belief perseverance phenomenon.

However, when more data showing linear magnetic anomalies began showing up in the Lamont laboratory at Columbia University (Lamont, headed by Ewing, was strongly antidrift), investigators rapidly converted to drift. Beginning with Opdyke and Pitman, who first analyzed the data from the Reykjanes Ridge, all eventually were persuaded of drift. Part of what was going on here was simply that there was more data in support of drift—data from not one ridge (the Juan de Fuca ridge investigated by Vine and Matthews) but two. But I strongly suspect that salience and

[6] Of course, social and institutional reasons may have influenced geologists' choice of subject matter. Cognitive and social factors are often intertwined.

availability play a role. Since they were working with concrete data which were produced from their own laboratory, they weighted the data more heavily and were less likely to overlook it than they would have had they just read the results of another laboratory.

Data in favor of drift continued to be produced, supporting Hess's model of seafloor spreading. Over the next few years, almost all geologists were converted to drift. In this conversion process, cognitive bias and social pressures may sometimes have played a determining role, although, less than they did before 1965. (They still play a role—the same mental processes continue—just not a *determining* one.)

I have argued that in the case of the work leading up to the revolution in geology this century, distribution of cognitive effort is explained in terms of different beliefs of the individuals involved. These different beliefs are largely explained at the level of individual cognition, as due to the heuristics of availability, representativeness and salience, which lead to different results with different individual experience and prior belief, even when all the data are public knowledge.

I do not regard this cognitive account as the complete explanation of distribution of belief in drift. Experimental work on motivation establishes the effect of personal, institutional and social factors on beliefs. Tversky, Kahneman and others do not deny this. Their concern was to argue, through the use of controlled experiments, that cognitive factors are an important and unacknowledged influence on decision making. The precise contribution of cognitive and motivational factors has not yet been determined.

Since we have no established theory regarding the relative contributions of cognitive and motivational factors, and historical studies are far from being controlled experiments, an account of belief change in the geological revolution has to be tentative. It is likely, for example, that personal investment, in addition to cognitive belief perseverance, was responsible for rejection of drift throughout the revolution, but we do not at present have a means for quantifying these factors for the historical case.

The work of Frank Sulloway (n.d.) on the effect of birth order on theory choice is a persuasive suggestion regarding the role of motivational factors that takes the debate between cognitive and motivational accounts beyond folk psychology. Sulloway shows that birth order is highly correlated with theory choice. Oldest siblings tend to adopt the entrenched or conservative theory; youngest siblings tend to adopt the more radical theory. In the case of geologists who entered into debate over drift during 1912–1967, 36 percent of firstborns and 68 percent of laterborns adopted drift. The most likely explanation of this is in terms of personality: Younger siblings challenge authority more than firstborns. This is a serious challenge to my position, because Sulloway is giving a partial explanation of

the distribution of belief in drift during the years before the mid-sixties in terms of motivation. (It is, incidentally, at the same time a challenge to social and institutional accounts of factors influencing belief.) I respond to Sulloway in two ways, and think he would have no objection to either response. First, Sulloway does not explain some of the important facts regarding the distribution of belief; he does not explain why those working on southern hemisphere materials, or those working on orogeny, or those working in paleomagnetism, were more likely to be mobilists. In one study (discussed by Giere 1988, 240) 91 percent of geologists with southern hemisphere experience who wrote about drift theory supported it, while only 48 percent of those without southern hemisphere experience who wrote about drift supported it. Sulloway and I are explaining different facts regarding the distribution of belief. Second, the way in which personality affects decision making needs to be examined, because cognitive factors very likely enter in here (more on this below).

So far, I have discussed how cognitive factors will take their place among social, institutional and motivational factors in an account of scientific decision making. I conclude by mentioning briefly some new experimental work that shows the cognitive heuristics discussed by Tversky, Kahneman et al. to be important in understanding the nature of motivation. Similar work is to be expected on institutional and social influence. This experimental work suggests that we should be looking not only for a combined, multivariate account of the various factors affecting scientific decision making, but for an integrated psychological account also.

Ziva Kunda (1990) suggests a plausible model for the effects of motivation on belief which makes crucial use of the cognitive heuristics of availability and salience. She argues that a motive to believe something, say *X*, brings *X* to mind, and through that makes more available and salient the evidence for *X*, with the result that belief in *X* is more likely. She points out that this explains why people do not believe just anything they want; unless making the supporting data more salient and available makes the data in support of *X* seem strongest, *X* will not be believed. Recognition that a better understanding of motivational factors may come in terms of cognitive factors expands the scope of my project in a way that is important for normative assessments. Here we have a limitation on the effects of motivation on belief; it is not surprising that Kunda talks of "motivated *reasoning*" and not just "motivated belief change". Feyerabend's slogan "anything goes", should thus not be considered a fair normative proposal: Humans will in fact only believe some things. Biased reasoning is not delusion and motivated reasoning is not unfettered wishful thinking. When evidence against beliefs is overwhelming, cognitive and motivational bias have little effect. Diversity of belief, and any consequent distribution of effort, may be expected only within limits specified by the structure of motivation and the heuristics of cognition. In place of the usual folk psychological explanations of human decision making, which sometimes are so loose as to be capable of explaining anything, we are moving toward a more precise model that promises to specify the role of both cognitive and motivational factors, interacting together. Institutional and social factors may also have their effects on the individual through these cognitive and motivational structures.

A final, somewhat tentative suggestion about the direction of this research draws on the work of Susan Cary (1985) on early childhood theories of the world and Shelley Taylor (1989) on positive illusion. My suggestion is that theories regarding one's own abilities, often formed in childhood, can affect cognition in just the way that motivation is theorized to affect cognition. That is, one might overvalue one's own data (say, paleomagnetic data) because one has a theory that everything associated with oneself is generally better than things not thus associated. Such a theory might be formed in early childhood experiences. We do not, of course, know what motivation is, and it is possible that motivation will be understood largely or completely in terms of cognition.

5. Normative Remarks and Conclusions. I have argued that, during the geological revolution, the phenomena of bias and belief perserverance were responsible for much of the distribution of research commitment. This distribution facilitated the various directions of research effort that went into the geological revolution. So the cognitive heuristics of availability, salience and representativeness had desirable epistemic effects in the geological community.

It should be emphasized that the epistemic value of bias and belief perserverance phenomena was just now evaluated *from the point of view of a scientific community working on a specific research project*. This is not the traditional evaluative standpoint; usually, the epistemic value of decision-making processes is assessed from the point of view of the individual and is not relativized to any domain. The traditional way of assessing rationality cannot show how the heuristics are conducive to scientific success; individuals are merely biased, while communities distribute cognitive effort.

My conclusions may disappoint some. I do not show that bias and belief perserverance phenomena invariably or even reliably produce good results (generalized across domains). All I find so far is that in one suggestive case, the revolution in geology, the heuristics brought about division of cognitive labor, and this division of cognitive labor contributed to scientific success. It is likely that the heuristics serve us well elsewhere, because they produce distribution of research effort wherever differences in individual experience and prior belief arise, but this re-

mains to be shown by a detailed examination of cases. These heuristics may also lead us astray sometimes (especially when individual experience and prior belief do not differ much), causing us to invest too much effort in hopeless theories. This is no Panglossian discussion of the virtues of human reasoning.[7]

I am also not claiming that the cognitive heuristics are necessary for producing distribution of research effort. I do claim that scientific decisions have in fact been importantly affected by the phenomena of bias and belief perseverance, and, given the entrenchment of such phenomena, they are likely to continue to be thus affected. This is how humans distribute research effort.[8] If humans did not reason with bias and belief perseverance, effort might still be distributed, but in a different way, and with a different outcome.

Future work will examine the distribution of research effort, its causes, and its contribution to scientific success in other scientific disputes such as the chemical revolution and recent debates in linguistic theory. Other effects of the heuristics on scientific change will also be examined. For example, the representativeness heuristics incorporates a similarity measure that is context sensitive and changeable, and thus may make possible the kind of theory-relative reasoning that Richard Boyd (1980), among many others, regards as characteristic of scientific reasoning.

The project begun in this paper is interdisciplinary, drawing on work in psychology, cognitive science and history of science as well as epistemology and philosophy of science. It is an example of work in the new tradition of epistemology naturalized, for it asks how various human scientific successes are, in fact, possible. I found that some cognitive factors—the heuristics of salience, availability and representativeness—previously overlooked or inadequately discussed, importantly affect scientific change. They must be included, along with discussion of social, institutional, motivational and scientific factors, in a multivariate, integrated account of scientific decision making. The normative perspective is community-wide and contextual, and geared to scientific success. Such an account will yield not only a better understanding of scientific change,

[7]Harman's (1986) suggestion that belief perseverance is an acceptable strategy for finite human beings who cannot keep track of the justifications for their beliefs does not provide reassurance that the strategy is enough for doing science. His suggestion (pp. 47–50) that people should keep track of the justifications for their beliefs in the area of active inquiry—which presumably includes scientific inquiry—is not shown to be psychologically feasible.

[8]Philip Kitcher's (1990) suggestion that cognitive effort can be distributed by unbiased normative evaluation and self-interested action on the part of scientists suggests a more optimal distribution of effort than that produced by bias and belief perseverance. Perhaps scientists can move their decision-making processes in this direction, and perhaps they sometimes reason this way already. However, at least in the geological revolution, much distribution of both evaluation and effort were produced by bias and belief perseverance.

but also suggestions for normative improvement that are more psychologically realistic than those usually offered by philosophers of science.

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BSSCHEACH

SCIENTIFIC RATIONALITY AND HUMAN REASONING

by Miriam Solomon

SUMMARY

Solomon asserts that human rationality does not follow the traditional normative guidelines of logic, probability and confirmation theory, but instead follows a few general heuristics such as salience, representativeness and availability. She cites experimental evidence from work on the psychology of belief change by Tversky, Kahneman and others which seem to support this view. The purpose of her paper is to show the operation of these heuristics in scientific reasoning. She uses the historical example of the development of the plate tectonics theory in this century as an example.

Her main point in this example is that the different cognitive biases and beliefs of geologists lead to a natural distribution of research in the field that was conducive to scientific success for geologists as a whole, even though some individuals were pursuing avenues of research using dead end models. The idea seems to be that the errors within a group will be distributed evenly and that biases in the opposite directions will cancel each other out. The question remains open as to whether or not this will always occur in a scientific community.

I would like to point out that the freedom these geologists had to pursue their own ideas is a relatively recent phenomenon. In order for this approach to be effectively used in an educational setting, it would seem to me that the teacher would have to allow that same freedom. This arguement would lend support to the social constructivist approach to education espoused by Freire and others.

Wegener believed that the motion of the continents through the ocean floor was possible in the same way that the motion of icebergs through the ocean was possible. Although faulty, this use of the representativeness heuristic at least partly served to **convince** Wegener that the continents could and had drifted, but it was not sufficient to **persuade** the general community of geologists that this was so.

I think that the general heuristics and the scientific heuristics described here may be seen as opposite ends of a continuum. Other continuums which may parallel this one would include internal to external, inductive logic to deductive logic, personal belief to persuasion, and everyday domain to scientific domain (Reif, 1991).

COGNITIVE CONTINUUMS

Internal	**External**
Personal belief	**Persuasion of others**
General heuristics	Scientific heuristics
Inductive logic	Deductive logic
Everyday domain	Scientific domain

QUESTIONS

1. If people generally use the heuristics described here, would it be feasible to use these heuristics **first** to bring about a conceptual change (radical or non-radical) in students, and then help them understand and express this concept using the heuristics of the traditional scientific domain?

2. May the use of drills, rote memorization, laboratory exercises and so forth be justified on the basis of increasing its availability to the student? After all, something you have in your head is a lot more available than any other source.

3. Does Solomon's explanation of belief perserverance speak to the concept of core ideas and protective belt ideas?

4. Solomon refers to a positive correlation between an overpositive evaluation of one's own abilities and motivation. Is it reasonable to infer that there is also a negative correlation between an underestimation of one's own abilities and motivation?

5. Would the emergence of plate tectonics theory qualify as a radical conceptual change by Chi's criteria?

6. Can the positive effects of distribution of research due to individual bias and belief perseverance occur within an educational setting such as the classroom? If so, how, and what would be the teacher's role?

7. Do formal statistical and evidential techniques <u>really</u> play the major role in scientific decision making? What justification is there for teaching them? Have they proven effective in the classroom in inducing radical and/or non-radical conceptual change?