I think the authors have done very well in explicitly choosing to eschew discussion of the nature of science. They report quite accurately that the ideas of Kuhn, Lakatos, Popper and all the others so often mentioned in this connection remain ideas, certainly interesting but by no means generally accepted, and absolutely inadequate to the task of judging whether or not a particular endeavor is or is not "scientific" or destined to be fruitful. They respond most appropriately to commentators who urge some theoretically desirable approach, for example the method of multiple working hypotheses. As with all else, the philosophical approach that works well in one discipline will not necessarily work well in another. Multiple working hypotheses make sense in geology, for example, where a multitude of possible explanations often present themselves; but hardly in physics, where Nobel prizes get awarded to those ingenious enough to create even a single plausible hypothesis. In any case, whether or not a claim is "scientific" is a red herring: what one wants to know is whether or not the claim has substance, which is not at all the same thing.

Among the many commentaries, a few make interesting points. Carter on plant diffusion and Steele on 1st-century bricks in Mexico point out patches of ignorance in mainstream knowledge. Cole and Kane give good expositions of the mainstream point of view. Among the more uncommitted, Hayden, Jett, and Morehouse are worth attending to. The rest of the commentaries, however, and the replies to them, add little.

Why such polarization, such a messy controversy, so little attention to evidence?, ask the authors of this volume. Fundamentally because even in the most developed human societies, logic and reason and respect for evidence have as yet attained only fragile toeholds. Thus currently in our own universities, it is "politically correct" to deny that quotas are quotas and to pretend that it is not racist to base decisions on a person's race. Rather than deplore what is and always has been the case, however, let us rejoice at the efforts of those pioneers who, like the authors of this volume, seek to become increasingly discriminating and logical and empirical in their quests and who thereby make it possible for human knowledge to expand its range.

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How We Know What Isn't So: The Fallibility of Human Reason in Everyday Life, by Thomas Gilovich. New York: Macmillan, 1991, 216 pp. ISBN 0-02-911705-4, $19.95.

For people who try to think, it is preeminently important to understand what gets in the way when we try to think objectively. For anomalists, it is particularly important to be clear why we dare not trust our beliefs absolutely. Important findings about human fallibilities in reasoning have until now been sequestered in the specialist literature and in textbooks (1), glimpsed by wider audiences only occasionally (2). Now at last here is a book accessible to all, and much gratitude is owing to Thomas Gilovich for writing it.

Most of us have heard that infertile couples who adopt children are likely then to succeed in conceiving one themselves, and we have no trouble finding reasons for it; the real trouble is that the "fact" isn't so, it is an example of how we come to know what isn't so, by attending selectively to unrepresentative data. We find it striking or interesting when couples who have adopted then conceive a child of their own; we don't find it interesting when infertile couples don't adopt, or when those that do adopt then remain without offspring of their own; and so we make a false count of the relative frequencies.

The weaknesses in our thinking are the other side of the coin of the strengths of our abilities: "dysfunctional as they may be on occasion, our theories, preconceptions and 'biases' are what make us smart" (p. 52). We couldn't function effectively without selecting what to attend to and thereby ignoring other ("irrelevant") things; yet that leads us to be instinctively wrong about what is likely to happen and what isn't. We think better than computers do in part because we are so good at perceiving patterns in events; and for the same reason we are paranoid as computers are not, for we espy patterns where there are none. Semmelweis recognized a pattern of connection, which led to knowledge of sepsis and antisepsis; sports fans recognize the pattern of the "hot hand or the lucky streak, which computer analyses show not to exist. What's the difference? Primarily that Semmelweis's pattern was tested by others before being accepted, whereas we accept the lucky streak and other "facts" of daily life without subjecting them to stringent tests that could prove them wrong.

We don't test our beliefs because we take them to be so obviously right. Part of the trouble is our wrong instinct about probabilities and randomness. We imagine that "random" means something like "regular alternation", so that we expect approximately regular sequences of head-tail-head-tail in the tosses of a coin; whereas sequences of just heads (or tails) are not all that unlikely, as we realize when we start to calculate: a second head is no less likely than a tail; if two heads, then a third is just as likely as a tail; a priori, we would expect that once in every eight trials we would get a run of three heads, and once in every four trials a run of three heads or three tails. Even experienced bridge players regard hands dealt by computer to be peculiar, yet they are strictly random.

Another part of the trouble is that we are so good at creating explanations; once we have accepted a "fact", we can readily fit it into our structure of beliefs.

We fall into error so easily because we tend to:

—notice confirming evidence and miss contradicting instances;

—accept what fits our preconceptions and thus test them inadequately;

—query what contradicts our preconceptions and therefore continue tests (until, if possible, another result is obtained, which we then accept at once);
—interpret ambiguous information as confirming our preconceptions;

and for all those reasons, when evidence is mixed (as it usually is), both opposing points of view see the evidence as favoring their side. Thus controversies become more rather than less polarized as more data are gathered: only when evidence is indisputably decisive (as it very rarely is) can "the facts" resolve a dispute. An additional barrier to the resolution of arguments is an asymmetry in the burden of proof: "For propositions we want to believe, we ask only that the evidence not force us to believe otherwise—a rather easy standard to meet, given the equivocal nature of much information. For propositions we want to resist, however, we ask whether the evidence compels such a distasteful conclusion—a much more difficult standard to achieve" (pp. 83–4).

Since anomalistics is embedded in controversy, those insights are worth having; we would do well to recall that "people do not hold questionable beliefs simply because they have not been exposed to the relevant evidence. . . . Nor . . . because they are stupid or gullible" (p. 2).

We err also because we tend to:

—adopt self-serving or comforting beliefs;
—accept second-hand information that has been distorted: and so we accept untrue stories about the results of conditioning experiments (pp. 88 ff), about the evil doings of Richard III (3), and innumerable "urban legends" (4);
—assume that others share our beliefs: in part, because "it's impolite to contradict", and so our friends and acquaintances often don't try to disabuse us of our mistaken opinions;
—answer questions as they are posed: if asked for similarities we look only for similarities, and only for dissimilarities if asked about those, so that our conclusions are biased toward one or the other side;

and further, we err because we fail to:

—allow for statistical regression toward the mean;
—allow for uncollected data: teachers of large classes hear largely from students who have complaints or alibis, so they get an incorrectly jaundiced view of the student body; similarly academic administrators tend to get a jaundiced view of the professoriate because it is chiefly the persistent complainers who seek them out.

All these pitfalls and more, Gilovich brings to ready understanding by illustrative example. Scientists are as prone as anyone else to these pitfalls (pp. 56–60), and that underlines the extent to which the success of modern science can be ascribed to the communal nature of scientific activity (5). Individuals, be they scientists or not, find it hard to think of crucial tests that might prove their pet ideas wrong, or to recognize such evidence; but since grant proposals and manuscripts for publication have to run the gauntlet of colleagues and competitors,

research and publication become less subjective and more reliable. Isolated scientists are liable to error, which is why it was good for the atomic-bomb project that Robert Oppenheimer was able to resist the compartmentalization that the Army initially wanted to impose; and why the best definition of "pseudo-science" may be isolation from the mainstream scientific community; and why SSE is on the right track by fostering forums in which ideas and evidence can be discussed critically by people with a variety of backgrounds and beliefs.

Gilovich makes excellent sense on the much-argued issues of motivation versus cognition, of how much our actions are influenced by our interests: "To the extent that there is a motivational 'engine' responsible for our self-serving biases and beliefs, it . . . delivers its effects through processes that look suspiciously cognitive. . . . Cognition and motivation collude. . . . " (p. 80); "The wish may be father to the belief, but like all fathers it requires a mate—some supporting evidence" (p. 174).

The generalizations or principles outlined in the earlier part of the book are applied in Chapters 8, 9, and 10 to belief in alternative medicine, the use of counter-productive strategies in everyday life, and belief in ESP. Chapter 10 is fairly evenhanded, though Gilovich is closer to a disbelieving than a skeptical stance and the passing reference to Jahn's work on psychokinesis leaves the unwary reader with a misleading impression.

The concluding chapter of the book is judicious and sensible. The author hopes to stimulate us to acquire habits of thought that will counteract human tendencies that cannot be eradicated: preference for black-and-white views of complex matters, taking things personally and imagining that we can control what happens to us, seeing patterns whether or not they are really there. If the author's hopes are not realized, then it will be the reader's fault, for this book offers all of us the opportunity to become aware of and correct our failings more frequently.

The book is not without its faults. Gilovich, sure that ESP does not exist, asks: "Why is the word of science accepted [that the earth is round, contrary to common experience, or that such bizarre things as quarks or black holes exist, or that there is a dangerous hole in the ozone layer] . . . but not with respect to [the spuriousness of claims of] ESP?" (p. 180). But if, contrary to Gilovich, one takes the view that people should not accept the current opinion of mainstream science on ESP, then one might reply: because the compelling evidence is not yet in; and because so many people have personal evidence for the reality of ESP, whereas they have no personal knowledge of (or much interest in) quarks and the like. Rather weak, too, are the reasons given (in Chapter 1) for regarding superstition as dangerous; and the suggestions (in Chapter 11) for what might be done (particularly by social science) to stimulate better thinking. The index is scanty and fails to do justice to the text. But these weaknesses should not obscure the fact that this is a book that everyone should read. I hope it will have many editions with continuing refinements.

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Notes

1. The most frequently cited being R. Nisbett & L. Ross, *Human Inference: Strategies and Shortcomings of Social Judgment*, Englewood Cliffs (NJ): Prentice-Hall, 1980.
2. For example, in the illuminating talks by Lee Ross at the first CSICOP conference (Buffalo, 1983) and by Daryl Bem at the 8th Annual Meeting of SSE (Boulder, 1989).
3. Richard III (Plantagenet) did not have his nephews killed, contrary to what has been taught to generations of schoolchildren. The lie was promulgated to legitimate the usurpation of the throne by Henry VII (Tudor). Scholarly histories, by contrast to school textbooks, have not repeated the lie, whose provenance is charmingly told in the mystery story by Josephine Tey, *The Daughter of Time* (1951 and many later editions).
4. Jan H. Brunvand, *The Choking Doberman and Other "New" Urban Legends*, New York: W. W. Norton, 1984.
5. Henry H. Bauer, *Scientific Literacy and the Myth of the Scientific Method*, Urbana & Chicago: University of Illinois Press, 1992.