

that science cannot disprove a wide range of possible religious beliefs? Because, it turns out, Johnson wishes a "supernatural Creator [who] not only initiated . . . but in some meaningful sense controls . . . [evolution] in furtherance of a purpose" (p. 4); and if one wants a Creator who intervenes tangibly, then one requires tangible evidence of intervention and is pushed to look for such evidence in "impossible" saltationist leaps between genera or classes or orders; one asserts that "In a word (Darwin's word), a saltation is equivalent to a miracle" (p. 32). Phillip Johnson "is creating something new" with this critique, according to *Christianity Today* (3). Not at all. Another lawyer made much the same argument twenty years ago (11), complete with the same misunderstandings of how science works and a reliance on Karl Popper for defining what science ought to be. New might be a discourse on the wide range of religious belief that remains plausible in the light of what science has learned about the physical mechanisms of life.

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American Epigraphy at the Crossroads, edited by James P. Whittall, Jr. Long Hill, Rowley (MA): Early Sites Research Society, 1991, vii + 143 pp. (paper), \$12 ppd.

The stated intention of this book is to lessen polarization and to increase productive dialogue about American epigraphy; but it also makes highly recommended reading for those who want to make sense of any anomalous claim or of the general societal circumstance whereby certain claims are summarily dismissed as pseudo-science. Perennial questions are raised about the nature of science, about the roles of amateurs and professionals, about the process of controversy that surrounds anomalous claims.

"American epigraphy" in this context means the study of American inscriptions in Old-World scripts and languages; epigraphers make the anomalous claim that pre-Columbian visitors left these signs of their presence. The best known epigrapher is Barry Fell who has written of Celtic, Arab, and other visits to the Americas as long as 4000 years ago (America B.C. [1976 & 1989], *Saga America* [1980]). Fell and his colleagues in the Epigraphic Society are treated as cranks by the mainstream of American archaeology which knows that there was no pre-Columbian contact between Europe and the Americas (other than the brief Viking presence about a millennium ago in Newfoundland and thereabouts and the newly admitted Red-Paint Culture of western Scandinavia and north-eastern North America 5 or 6 millennia before that).

The book under review is the work of the Early Sites Research Society which aims to take the intermediate stance that some of the inscriptions discovered by the epigraphers deserve to be seriously studied by mainstream scholars and scientists even though the case made by the epigraphers is flawed. The volume contains three major articles (by sub-sets of Rollin Gillespie, James Guthrie, Phillip Leonard, and William McGlone), a set of commentaries on them, and rejoinders to the commentaries. The first article—"The Two Sides of the Epigraphic Controversy"—shows through quotes how polarized the debate is and how it tends to sink into propaganda rather than reasoned discussion. The second, "The Confrontation of Science by Epigraphy", addresses substantive issues of method, evidence, and presentation of evidence. The third, "The Demand for Artifacts", makes the case that it is unreasonable to dismiss epigraphic claims merely by asserting that if those claims have substance then there should have been found material artifacts (other than inscriptions) left by the postulated visitors.

The articles, covering about 65 pages, do what they promise and provide room for much thought about general as well as specific issues. The commentaries, of very unequal length and interest, take up nearly as many pages, to much less purpose so far as the general reader is concerned; and the authors' rejoinders, too, add little to the original articles. It may well be, though, that the device of offering to publish what they had to say led more of the invited commentators to read the articles than might otherwise have been the case. In the remainder of this review, I shall point out the relevance of the book to certain major and perennial issues in anomalistics, in particular the role of amateurs, the nature of science, and the import of disciplinary viewpoints.

The epigraphers, it is remarked, are a largely amateur group. So too, by definition, are cryptozoologists, parapsychologists, ufologists, and other anomalists: if for no other reason, then because the nature of these anomalies defies any

attempt to define what the proper "professional" credentials would be: for epigraphers, presumably some combination of history, archaeology, linguistics, geology; for cryptozoologists, perhaps remote sensing, folklorology and mythology, photographic interpretation as well as biology and experience in the reliability of eyewitness testimony; for parapsychologists, proficient understanding of stage magic as well as statistics, normal and anomalous psychology, and so on—in all cases, some combination that one is unlikely to find in any individual.

In effect, the investigation of such an anomaly constitutes a proto-discipline which will—if the claimed phenomena prove to be real—develop its own idiosyncratic methods, knowledge, sense of values, and interactions with other disciplines. In any case, and as the authors point out, the charge of amateurism is less damaging in some fields than in others; in linguistics, great achievements are due to the amateurs Champollion, Grotfend, Rawlinson, and—as recently as the 1950s—Michael Ventris. It is probably partly a matter of how mature a field of knowledge is; and partly a matter of whether it is "Big Science" and "Big Scholarship", which require teams of people to do what is needed, as opposed to "little" science or scholarship in which dedicated individuals can do what is necessary (without having to be extremely wealthy). Thus in physics or chemistry, amateurs—or better, outsiders—are most unlikely to contribute significantly: to understand the state of those arts, one must have been immersed in years of formal study followed by an apprenticeship in research during which one begins to see in perspective what up to then one has imbibed without much questioning. As John Ziman has pointed out, that the scientific literature is publicly available does not make it open to everyone: it is really open only to the insiders, who appreciate its nuances and underlying presumptions. For the same reason Derek Price argued that the real value of professors of science being engaged in research is not that they are all able to make significant contributions in the search for knowledge but that by being so engaged they thereby have access to the state of the art, are able to understand what is actually going on. In consequence, genuine amateurs or outsiders are quite unlikely to doing anything of significant value in the physical sciences: so intelligent and scholarly a man as Immanuel Velikovsky spoke and wrote absurdities about physics and chemistry in part because he thought he could—as an outsider—read texts in the field and properly understand them.

To be thoroughly trained in a discipline entails more than just acquiring facts and learning how to work: one inevitably acquires at the same time a sense of values, a world-view even, that is peculiarly appropriate to that discipline but may be singularly inappropriate in another. The extent to which disciplines correlate with particular intellectual, political, religious, social viewpoints is not widely appreciated (for example, that physicists are notably liberal whereas chemists and engineers are notably conservative politically); for illustrations and detailed discussion, see "A dialectical discussion on the nature of disciplines and disciplinarity", *Social Epistemology*, 4 (1990) 215–227; "Barriers against interdisciplinarity", *Science, Technology, & Human Values*, 15 (1990) 105–119; "Tribal Stereotypes", chapter 16 in *To Rise above Principle: The*

Memoirs of an Unreconstructed Dean, by 'Josef Martin' (University of Illinois Press, 1988).

Guthrie begins to recognize the import of disciplinary background in his comment that "the epigraphers and their critics have two different kinds of minds" (p. 140): indeed they do, not of course inherently but because they have spent appreciable parts of their respective life-times training those minds in different ways, to different purposes. But when the authors invoke disciplinary peculiarities to suggest that academic rigidity "must be peculiar to the non-experimental disciplines", they go too far: perhaps the degree of rigidity is greater in the non-experimental fields, but even that is an intuition that remains to be established.

The authors' own viewpoint is understandable in part on the basis that they are engineers and physical scientists whereas archaeologists are not. In contrasting the stuffiness of American archaeology to the openness of technology to new ideas, our authors miss the fact that technology and technologists have also been open only to new ideas that struck them as feasible and warranted, and that they have been quite closed-minded to such things as 100-miles-per-gallon carburetors, let alone perpetual-motion machines. American archaeologists are closed-minded to ideas that strike them as similarly absurd, that are virtually bound to so strike them because of the schooling they had to have in order to become professionally qualified. Professionals who can entertain heretical notions are rare in all fields.

Again, the authors fail to grasp how strongly one is bound by disciplinary attitudes when they respond to an epigrapher that they are not "trying to convert academia based on the present evidence", only "arguing for their participation in judging it". Scientists and scholars will take time to judge a thing only if they have already made the *a priori* judgment that something of value might eventuate from it. That epigrapher, then, had a sound basis for misunderstanding them: he realized that urging scholars to look closely at the epigraphers' evidence is tantamount to urging them to accept it at least provisionally. That only 3 of 23 critics responded, compared to about 40% of the epigraphers and the uncommitted, makes the same point, as does that only "a few highly vocal, dogmatic scientists" have entered the debate: to most professionals, it appears as a waste of time, and only the most "evangelical", extreme, scientific individuals will usually do so, and their aim is not dialogue but conversion of the unenlightened.

Anomalists like to point out that those "professionals" who have learned the most about anomalous claims are also those who give those claims the most credence. Yet this in itself is no sound argument for the validity of those claims, it may reflect no more than self-selection among the professionals: those who took the time to learn about the claims had already made the preliminary judgment that those claims are not entirely beyond the realm of possibility.

Still on this point, when the authors see as part of a possible resolution, "science, operating as scientists claim it does", they fail to realize that science cannot usefully be open to heretical ideas offered by outsiders because too much time would be wasted.

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.