THOUGHT AND OPINION

Velikovsky and Social Studies of Science

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The Velikovsky Affair (1, 2) has been cited by social scientists (3-10) as illustrating the resistance offered by science and scientists to new ideas; for example: the Velikovsky affair was "the most massive case of theoretical, methodological and 'social' non-conformity in the recent history of science," and of interest about Velikovsky is "the reception of his work by the scientific community" (8). Such citations are based explicitly or implicitly on the claim that the technical merits of Velikovsky's ideas could be left aside: "I am not interested here in the precise nature of Velikovsky's claims nor in their scientific validity, though the latter is relevant in some degree to the discussion which follows...." (8); "...the merits of the scientific issue do not alter the deplorable treatment that his ideas received from the profession" (11).

I shall argue that the nature and validity of Velikovsky's claims must be considered before one decides that the Affair can illuminate the reception of new ideas in science; that examination of the claims reveals that they have nothing useful to do with science; that therefore the Affair should not be used as an example in the social studies of science (unless it is used as an example of non-science or of pseudo-science).

The Demarcation Issue

There exists, of course, no general agreement on the criteria one might employ to distinguish science from other intellectual activities. It does not follow, however, that any intellectual activity can legitimately be called "science" or be regarded as relevant to the manner in which science reacts to new ideas. That there is no formula by which science can be distinguished does not entail that we cannot validly say of something, "this is not science." I shall argue that there is no good reason for talking about Velikovsky and about science in the same context, because Velikovsky's activities had none of the attributes of scientific work.

Science is, among other things, an activity carried on by an identifiable group of people: scientists seek to describe the natural world; they communicate with one another about their work; they train new workers; they have associations which offer membership on the basis of professional qualifications. The community has recognized rewards and sanctions. The modes of communication are structured: meetings both informal and formal, exchange of written communications both informal and formal. And so on. Though no simple formula exists for determining who

is a scientist and who is not, and though there are certainly cases where no clear-cut decision would be generally agreeable, there does exist agreement over a wide area, based implicitly on the sort of grounds just enumerated, about who is a scientist and who is not: Reverend Jerry Falwell, for example, is not a scientist, even though he says things about science.

None of the social characteristics of scientists provide any warrant to call Velikovsky a scientist. It is well to recall that "scientist" is a generic term that encompasses practitioners of recognized specialities -- there is no scientist who is not also a biologist or a chemist or a geologist or some other sort of specialist. Now Velikovsky's purportedly scientific claims have to do primarily with astronomy, geology, and physics (with chemistry also entering the picture thereby). In none of those fields did Velikovsky possess the qualifications required of certified practitioners. He had no advanced degree in any science, and not even a specialized first degree in any science. He never held a position as teacher or researcher in a college or in a public or private research institute. He never had a paper on astronomy or physics or geology or chemistry published in any of the technical journals devoted to those matters. He was never a member of any of the "invisible colleges" in any of those fields. There seems to be no basis at all, in Velikovsky's way of working or in his associations, for calling him a scientist.

Admittedly, Velikovsky made written as well as oral statements about matters of astronomy and physics and so forth, statements for which some people have claimed a certain validity. But we do not apply the term "scientist" to everyone who makes statements about astronomy, physics, and the rest, even when those statements have a certain validity. Much science fiction has had sound and prophetic things to say about several sciences, but those authors are not called "scientists," nor are studies made of how their statements were received by science, nor are such studies cited as instances of resistance by scientists to new ideas. Why should Velikovsky's work be thought to be relevant to science?

The literature and history of the controversy (2) reveal that, in point of fact, leading astronomers and geologists and physicists asserted from the beginning that Worlds in Collision was not a work of science and that scientists could not do anything useful with it. But some writers of popular science, and more journalists, and some humanists and social scientists thought that Velikovsky's substantive ideas had some plausibility; and they thought that scientists ought therefore to take those ideas seriously. When scientists then reacted strongly, overstated the implausibility of some of Velikovsky's assertions, and made ad hominem attacks, support for Velikovsky became more general, and issues of free speech and openminded examination of ideas and reception of new ideas by science were raised. But those issues were not validly raised. Even if Velikovsky pointed to some possible truths about the natural world, it does not thereby follow that scientists ought to have taken up his ideas for examination. It is not the case that scientists take up the ideas of others simply because they concern deep questions about nature: the ideas must seem to have some scientific plausibility, they must be scientifically examinable, they must seem important

to the relevant practitioners; and that is unlikely to be the case when the ideas are presented in the form of a popular book rather than in technical journals, by one who has no standing in any science and whose writings display notable ignorance of elmentary points of the relevant sciences. In point of fact, the notion that Velikovsky's work has something to do with science became widespread only because sufficient publicity and credence were given to Velikovsky's own claims that his work was significant for matters usually associated with the names of such as Darwin and Newton. A number of people (but not scientists) accepted Velikovsky at face value.

The commonly held belief, that science should concern itself with any seemingly important question that anyone poses about the natural world, displays a mistaken view of the character of scientific activity. That such a belief is indeed commonly albeit implicitly held throughout our society, I have illustrated in detail in relation to the Velikovsky affair (2). I want here to point out that such a belief actually is a scientistic one, for it builds on the implicit assumption that only science can make valid pronouncements about the truth of matters relating to the natural world. The demand that scientists should have seriously examined Velikovsky's propositions is grounded in part on that scientistic belief.

One cannot, then, take the Velikovsky affair as illustrating anything about the reception of new ideas by science without assuming that Velikovsky's particular claims are properly the subject of scientific enquiry. Those who insisted that Velikovsky's propositions should have been examined by scientists were assuming that scientists had dismissed those propositions on inadequate grounds.

The Nature of Velikovsky's Claims

Even a summary of Velikovsky's notions makes plain that scientists had ample and excellent grounds for not taking them seriously. Velikovsky presented a chain of interpretations of historical and mythic references as demonstrating that the accepted chronology of the Middle East was seriously in error; that global catastrophes not recognized in current established knowledge had occurred in recent historical times (around 1500 BC and 700 BC); that the accepted theory of gravitations is inadequate; that Venus had erupted as a comet from Jupiter, had caused the aforementioned global catastrophes, had also suffered close contact with Mars, and had subsequently settled into its present orbit as a planet.

It takes little particular or deep examination of those claims to recognize that they contradict established knowledge in many disciplines: Biblical exegesis, history and archaeology, geology, astronomy, and physics, among others. And that recognition in itself is adequate grounds for scientists not to examine the claims any further. Science, as all other intellectual disciplines, seeks to make new discoveries by grounding itself on some portion of already reliably established knowledge: it has no way of dealing with propositions that discard not only accepted theory but also accepted facts and accepted methodology -- and Velikovsky did reject accepted facts and methods of the various sciences as well as theories.

If one delves a little into Velikovsky's particulars about matters of physics and chemistry and astronomy and so on, one finds that many of his statements are plain wrong, and that he displays a

serious lack of understanding of the fundamentals of those disciplines (2). There was not common ground on which scientists could even have engaged him in substantive debate.

In point of fact, in this light Velikovsky can be properly compared with people whom we do not hesitate to class as pseudo-scientists. The reluctance to so label him stems from his manifest intellectual powers, from the massive documentation he brought to his writings, from his remarkable erudition and memory: but the substance of his claims no more warrants scientific scrutiny than do the claims of von Däniken, or for that matter those of Ignatius Donnelly, who had published in the 1890s a book strikingly similar to Velikovsky's (2).

Velikovsky and Social Studies of Science

In the event, some social scientists failed to recognize the spuriousness of the claim that Velikovsky could have some significance for science. As a result, they have drawn from the case some conclusions about science that are misleading.

For example, the public reactions of scientists might have been harsh, it was suggested (8), because Velikovsky "broke the (Mertonian) rule of communality...by allowing popular interpretations of his work to be published before the main opus itself had been presented." But the "main opus" itself was only a popular book; and Velikovsky was never a member of a community to which these norms or rules apply.

The critics, it was said (8), "did not subject Velikovsky's claims to rigorous examination before assessing the validity of these claims." Since the merit of the claims has been left aside for the purpose of that commentary, however, this statement implies that any claim at all needs to be rigorously examined; and thus this same criticism could be directed at science for its reaction to the rankest piece of pseudo-science. Surely, rigorous examination is called for only if cursory examination reveals something that is prima facie of scientific interest. But Velikovsky's notions do not possess that.

It was claimed that the laws of mechanics "operate as norms, departure from which cannot be tolerated" (8). In point of fact, the affair shows only that such unsound departures as Velikovsky's are ignored; such departures as relativity are tolerated very well.

"Particularly noticeable...is...the persistent tendency of scientists...to justify rejection of Velikovsky's claims simply by indicating the latter's departure from established beliefs" (8). But those established "beliefs" happen to be the most fundamental laws of physical science, massively supported by empirical evidence. Cavalier dismissal may not be appropriate for ideas that are a little unorthodox, but it is surely an appropriate response to ideas that are absurd. Science is not usually criticized for cavalier dismissal of astrology; the criticism in the Velikovsky case implies that Velikovsky's suggestions are somehow more scientific than that. In point of fact, however, whenever Velikovsky deals with physical science he is entirely out of his depth (2).

Another unfounded claim is that Velikovsky's work was rejected because it is interdisciplinary: "using historical records of natural events, [Velikovsky] put in question the essential methodology of modern science" (8). But Velikovsky's use

of historical records is itself most questionable (12). Further, the use of methodology from one discipline to obtain data for another is not a generally valid procedure, especially not when the disciplines differ as much as do folkloristics and astronomy. Velikovsky proposed unsound interdisciplinary activity, and was rejected for being unsound, not for being interdisciplinary.

In view of these weaknesses in the case (8), one must also reject the conclusions that "analysis of the Velikovsky case demonstrates...that the need for cognitive consensus which is reinforced by the educational process in science can impede consideration and generation of new ideas." Other cases than that of Velikovsky must be used if such a conclusion is to be regarded as empirically established, and preferably cases where the new ideas are good ones, useful ones, appropriate ones and not irrelevant to the technical content of science.

Another critic (9) took the Velikovsky controversy as illustrating the tendency of scientists to reject new ideas as a result of prejudice and of the threat posed by the possible overthrow of "an established way of looking at the world, a paradigm of science." Referring to the prejudice encountered by Copernicus, Galileo, Mendel, Newton, and Wegener, it was claimed that "one of the most sensational incidents of scientific prejudice" was that of Velikovsky. But any examination of Velikovsky's claims makes plain that they cannot be talked of in the same breath as those of Copernicus et al., which proposed a new view within the framework of accepting much of reliably established knoweldge; Velikovsky rejected fundamental principles of every discipline with which he concerned himself. Proponents of pseudo-science routinely point to the rejection of Copernicus et al. as analogies to their own reception by science, but that is not a valid argument in support of their own theses. Gruenberger (13) has called this "the Fulton non-sequitur" -- they laughed at Fulton; Fulton was right; they laugh at me; therefore I am right.

Once again, by not submitting Velikovsky's technical claims to prior scrutiny on intrinsic grounds, a critic of the affair has treated as science what is not science. So also Barber's seminal article (14) is quoted (9) to the effect that resistance to a scientific discovery is likely to be stronger, the less is the professional status of the discoverer; and "one might wonder what might have happened to Velikovsky's theories if he had been a professor at Harvard." I am indebted to Robert Schadewald for remining me that Barry Fell of the Harvard faculty has found no receptive audience of academics for his notions about pre-Columbian settlement in America, notions propounded like those of Velikovsky in popularly written books (15) and not in technical journals.

Another critic (7) maintained that "Like all 'true believers,' the scientist...resists...with every resource at his disposal or runs the risk of being destroyed as a person...committed scientists can exercise no other alternative to assure their own intellectual survival than to discredit the discoverer rather than the discovery." That is quite wrong -- the bulk of the literature showing resistance to new ideas in science is replete with arguments addressed to the ideas themselves; it is in the case of pseudo-science that one sees more in the way of ad hominem dismissals than analytical criticism of the propositions.

The same critic (7) posed the question, "why should some new ideas be acceptable to scientific fraternities while others are rejected on nonscientific grounds?" But it has not been established that Velikovsky's ideas were rejected on non-scientific grounds, so the question is out of place in this context. So also is the claim that the Velikovsky affair "represents a challenge to the self-correcting ethic [of science]"; that would be the case only if scientifically interesting and potentially valid ideas had been dismissed. To the contrary, since Velikovsky's ideas are of no relevance to scientific enquiry, science has indeed been "self-correcting" by not wasting time on them.

McAulay (10) looked for a possible influence on the reception of new views in science of "the metaphors and background assumptions which underpin scientific theories." "If in the past traditional religious sentiments have spurred resistance to Copernican and Darwinian theories, the Velikovsky 'affair,' conversely, marks the resistance of establishment science to a cosmology charged with fundamentalist religious significance. (Note: this should not be interpreted to suggest that Velikovsky's theories are rejected solely on extra-scientific grounds nor that this decision is necessarily wrong). In this light the response to Velikovsky's ideas...bears reexamination in terms of the tacit scientific world view at odds with catastrophism." But since rejection of Velikovsky's ideas is warranted solely on intrinsic scientific grounds, the case cannot be used in the search for extra-scientific grounds. One would at the least have to demonstrate that the scientists who rejected the ideas by stating their unacceptability in light of accepted laws actually had other reasons; and that has not been demonstrated. An examination of the unusually vehement reactions of Velikovsky's detractors might reveal other motives; but even then, one would have to view that as unusually vehement denunciation of pseudo-science, not of science.

Conclusion

The Velikovsky affair has been cited by some social scientists as exemplifying how science reacts to new ideas, and conclusions have been drawn therefrom about the nature of scientific activity. But there is no basis, other than Velikovsky's own wishes, for considering his ideas to have any relevance for science. What the controversy illustrates is how vehemently scientists can react against attempts by outsiders to insist that non-scientific notions should be taken seriously by scientists. Commentaries on the affair also reveal that some social scientists accepted Velikovsky's claims and not the claims of accredited scientists about the merits of Velikovsky's ideas; and thereby came to discuss as science what is actually in substance a rather typical piece of pseudo-science: thus pseudo-scientists and their supporters typically claim that their ideas have been rejected without rigorous examination, and point to such people as Copernicus and Galileo to prove that science has, in other instances too, been wrong about new propositions.

Such strictly externalist discussions as those here criticized (6-10), in which the merits of the intrinsic technical issues are left aside, may be appropriate when the issues considered are unquestionably matters of science; but in cases where there is a division of opinion over the issue, is this at all science or is this pseudo-science, when prior consideration of the

merits of the technical claims themselves is in order. In point of fact, this is unlikely to be a serious restrictions on the range of topics available for study by social scientists: scientists do not vehemently and essentially univocally decry as pseudo-science something in science that is new, even startlingly new; they may denounce it as unfounded, or as wrong, or as unduly speculative, they may not give it credence, but they do not call it pseudo-science and they do not call one another pseudo-scientists.

Notes

1. Alfred de Grazia (ed.), **The Velikovsky Affair --The Warfare of Science and Scientism**, New York: University Books, 1966.

2. Henry H. Bauer, **Beyond Velikovsky -- The History of a Public Controversy**, Urbana & Chicago: University of Illinois Press, 1984.

3. Philip D. Converse, Letter to the Editor, **American Behavioral Scientist**, November 1963, p. 22.

4. George A. Lundberg, **ibid.**

5. Charles Perrow, **ibid.**, January 1964, p. 29.

6. Alfred de Grazia, "The Scientific Reception System," pp. 171-231 in note 1.

7. Andre M. Bennett, "Science: The Antithesis of Creativity," **Persp. Biol. Med.**, 11 (1968) 233-246.

8. Michael Mulkay, "Some Aspects of Cultural Growth in the Natural Sciences," **Social Research**, 36 (1969) 22-52.

9. Harold Graff, "Scientific Prejudice: The Velikovsky Incident," **Bull. Philadelphia Assoc. Psychoanalysis**, 23 (1973) 288-306.

10. Robert McAulay, "Velikovsky and the Infrastructure of Science; The Metaphysics of a Close Encounter," **Theory and Society**, 6 (1978) 313-342.

11. Jacques Barzun, **Science: The Glorious Entertainment**, New York: Harper & Row, 1964; footnote on p. 78.

12. Bob Forrest, **Velikovsky's Sources**; 6 parts published 1981 to 1983 by Bob Forrest, 53 Bannerman Avenue, Prestwich, Manchester M25 5DR, England.

13. Fred J. Gruenberger, "A Measure for Crackpots," **Science**, 145 (1964) 1413-1415.

14. Bernard Barber, "Resistance by Scientists to Scientific Discovery," **Science**, 134 (1961) 596-602.

15. Barry Fell, **America B.C.**, New York: Quadrangle, 1976: **Saga America**, New York: Times Books, 1980.

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