



VELIKOVSKY'S PLACE

IN THE HISTORY OF SCIENCE



A Lesson On the Strengths and Limitations of Science

By Henry H. Bauer

Ask any historian of science what's exciting or new and you are likely to hear something like this (from Robert Kohler's *Lords of the Fly*, 1994): "Would the study of material culture and craft practice become a broad and productive new genre or remain an embellishment of familiar intellectual, institutional, and disciplinary histories?"

This quotation illustrates, I think, what historian of science Frederic Holmes recently admitted (1994, p. 39): "historians of science write about issues...that are interesting only to themselves." In other words, there is a difference between issues in the academic field of history of science and issues of what has gone on within science. This also applies to the philosophy of science, virtually all the sociology of science, and an overwhelming part of STS (science, technology and society) studies. Scholars in each of these disciplines speak primarily to themselves, rather than offering insights about science of which others can make use. That, of course, is but one concrete instance of the over-specialization of academic discourse that has crept up on us during the second half of this century. Meta-analysis is becoming the rage. That is, analysis of issues has given way to the analysis of analyses, and even the analysis of analyses of analyses. One might even say that meta-science has metastasized.

So the "history of science" has nowadays become an ambiguous phrase. It can mean something that scientists are interested in, something about the development of science, or something that historians of science, rather than scientists themselves, find of interest.

All of that is a long introduction to a brief conclusion as to Velikovsky's place in the history of science—he is well remembered in the historians' history of science, in the interpreters' history of science; but not at all in the scientists' story of what has happened in science itself. The evidence is direct and unimpeachable—Velikovsky's name is all over

the literature of the history, philosophy, and sociology of science, but his name never appears in science literature.

What Does Science Tell Us?

An important distinction to make in this discussion is that between individual learning and the collective knowledge of humankind. We honor effective teachers for stimulating individual learning, though very few teachers contribute anything new to humankind's *collective* understanding. That is why we often speak of deploring the "publish-or-perish" approach and insist that teaching be valued equally with research. That is also why I suggested in my book about the Velikovsky Affair (1984) that many of us bear a debt of gratitude to Velikovsky for having stimulated thought and even action that has served us well and that we might have missed were not for his initiatives. It is irrelevant in this respect that Velikovsky contributed nothing at all to the collective knowledge of science, as such.

I became involved in the Velikovsky Affair about the time I was beginning my move from being a scientist (a chemist) to being an interpreter of science. What I learned from the interactions between Velikovsky, his critics, his supporters, and the assorted and associated pundits shaped my view of what interpretation of science ought to be. Among other things I learned that scientists, no less than laymen, need to be educated about the nature of the scientific enterprise. I also learned that many interpreters of science and media pundits need such educating as well.

As I started my teaching career, like every teacher I kept uncovering more and more points of my own ignorance. I began to understand, especially through the work of John Ziman and Michael Polanyi, that science is not a matter of individuals who have been trained in an established and

well defined method simply applying that method to generate objective knowledge in a new area. Rather, the scientific enterprise is a matter of overlapping communities producing purported or presumptive knowledge, whose reliability depends on the extent that continued interactions within those communities are open, honest, disinterested, and skeptical. In my recent book, *Scientific Literacy and the Myth of the Scientific Method* (1994), I noted some implications of that view. Among them are the need to distinguish well-tested textbook science from tentative frontier science, and the differences between the natural and the social sciences.

Among other things, Velikovsky's work raised the issue of interdisciplinarity. I have found that a very interesting subject to be talked about and aimed for more often than practiced. My experience as a Dean of Arts & Sciences has enabled me to see how deep-seated and important the differences between the various academic or intellectual domains are. C. P. Snow's *The Two Cultures*, captures the essential point that the differences are in fact cultural. Engineers and sociologists differ not only in that the former knows about machines while the latter knows about human groups. They also profoundly differ in what it means to "know about" something, in the relative value they place on practice versus theory, as well as in their views on a variety of such seemingly unrelated matters as politics or religion. Whether these different outlooks stem more from self-selection in our choice of profession or from the training received and internalized after making that choice, the facts of the matter are quite plain, as I have noted elsewhere (1988, 1990a, 1990b). These cultural differences need to be accommodated before there are any attempts to design or practice interdisciplinarity. This fits well with the rule of thumb that close, continual interactions among people from different disciplines are by far the best way to produce proper interdisciplinary work or form effective working teams. It is also plain that individuals who seek to practice interdisciplinarity must first acculturate themselves thoroughly into each of the disciplines on which they wish to draw.

Recognizing disciplines as cultures, and not just fields of knowledge, inevitably raises epistemological questions on which there is no consensus among philosophers, sociologists, and other scholars. These include—What is knowledge? What can we know? And how can we come to know it?

Like others who were first trained in one of the natural sciences, I find compelling the view that scientific knowledge is a map-like sort of

knowledge, a knowledge of how things happen, of how to do things, of what follows if a certain thing happens. That is, I prefer operational as opposed to ontological or causal knowledge. When science explains "why" a thing happens, it is, in reality, describing only how it happens under certain circumstances. That view is faithful to what the history of scientific discovery tells us. Theories survive only for as long as they accurately describe what actually happens in the world.

An obvious, inescapable, yet often neglected corollary is that scientific theories are not truth, not even presumptive truth. Rather, they are simply the most useful way, at any given time, of rationally explaining a certain selection of natural phenomena. Utility in science most often means heuristic value, that is, reflecting the degree to which further fruitful research is stimulated. Utility is also judged by how well a theory can replace a set of observations by serving as a shorthand way of describing those observations. Neither of these attributes of a theory, however, is identical to the commonly-used meaning of the word "truth," (let alone "Truth"). The history of science has convincingly taught that wrong theories can be very useful in bringing about progress through pointing to research that turns out to be fruitful. This explains Wolfgang Pauli's oft-quoted definition of mediocre work as "not even wrong." We use theories because they permit accurate calculation even if their basis is quite wrong. For many decades now we have used wave equations and particle equations to predict the behavior of things that are certainly neither waves nor particles.

The pervasive confusion between the scientific concept of truth and truth as used in ordinary discourse plays a great and greatly mischievous part in the Velikovsky controversy, as well as in the seemingly never-ending bickering between "Science" and "Religion." Truth, as commonly used, connotes something humanly meaningful. But in science its use is limited to describing the world "out there." Science cannot truly elucidate what it is that exists, let alone why. And it is the "why" that would imbue human existence with meaning. The problem for each of us is how to fit a properly "scientific" description of our environment into a world view whose most humanly significant aspects—moral right and wrong, for example—fall outside the jurisdiction of science.

Velikovsky wanted to uncover things of human significance. The substance of his claims includes a lot of interesting and instructive material about history, legend, human belief, and religion. A great deal of it can reasonably be believed by any rational person who chooses to



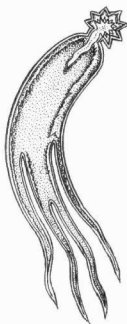
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do so. But that is outside the proper scope of science, which can only proceed, as it always has, opportunistically, operationally, and more and more through greater specialization. On the one hand, outsiders can often and rightly perceive that what "science" says is faulty, or sometimes even patently not true. What outsiders cannot usually do, however, is to improve science's understanding, which first requires acculturation into the specialist culture. Let me give a few examples of this little-understood point.

Mainstream American archaeology insists that the earliest time at which humans entered the Americas is given by the dates of the earliest settlement sites so far discovered. A little thought shows that this is surely absurd because it is highly unlikely that we have discovered the very earliest sites! Nevertheless, that realization helps not one whit to advance the state of archaeological knowledge nor to indicate where one might most fruitfully look for earlier sites.

Next consider the recommendations regarding lifestyle and diet with which we are so frequently regaled. As one who has benefited from angioplasty and cardiac by-pass surgery, I take some interest in them. As I left the hospital on one occasion, I was given a sheet listing what to eat and what not to eat; and already there were several inked-in corrections representing "the latest" belief. No sensible person follows slavishly these ever-changing recommendations, particularly perhaps since they are incapable of accommodating differences of individual metabolism. As a friend of mine once expostulated to his doctor, "You mean if I do what you tell me, I'll live forever?" These recommendations are just the current best guess that medical science can offer. They are obviously inadequate, but realizing that does not indicate how to make them better. Science, in other words, is a highly technical and specialized pursuit whose theories are suited to further progress in science, but they are not necessarily useful truths for human living.

Science in Context

My fascination with the Velikovsky Affair began in the mid-1970s. I wanted to move from working in chemistry to commenting about science. In 1978, I took a position as Dean of Arts & Sciences and progenitor of a Center for the Study of Science in Society. I can now look back at my first effort in science studies—my analysis of the Velikovsky controversy—from a somewhat more informed vantage point. Perhaps I can make some repayment for the stimulation I gained from it by attempting to do what historians do so well—to see people in the context of their time, thereby deepening our understand-

ing of that time and, in turn, further deepening our understanding and appreciation of those who lived in that time.

Velikovsky's energy and talents were extraordinary and prodigious. Yet like all of us, he was a product of his time and of his place. His attitudes and beliefs, toward science and toward knowing, were far from his alone. His views were formed at a time when positivism still held sway. Science, it had become generally agreed, could produce positively new and positively reliable knowledge. Rigorous thought, coupled with dedicated research and scholarship, could carry knowledge to an ever higher level. Though distinct disciplines were growing and flourishing, there was little hindrance to moving from one to another. Talented individuals could independently and individually add to humankind's store of understanding. To be an all-round scholar—a polymath—was a magnificent aim rather than a deluded impossibility. For an energetic and talented man like Velikovsky, it was not absurd to attempt the sort of major contribution he thought he had wrought.

Velikovsky in Context

I want to see Velikovsky, then, as representative of a major tradition of grand, individual scholarship in the context of his time. Even as he embarked on and carried out his life's major work, positivism was collapsing. Thinkers such as Karl Popper, Robert Merton, Derek de Solla Price, and Thomas Kuhn were changing the interpretation of science and the definition of knowing. Because of their work, we now view science as more the product of a coherent community rather than the accomplishment of solitary intellects.

Our popular present image of science, as is the wont with such images, is that of a romanticized past. We are nostalgic for heroes, for leaders, for thinkers who really and fully know their own minds (as well as ours). We want to believe that great advances can be made by great individuals. We want to believe grand claims made by charismatic thinkers and doers. So, given the opportunity, we become entranced.

The opportunity to be charmed comes rather readily, for even though the scholarly interpreters of science know that positivism is dead, hardly anyone else does. The Velikovsky Affair shows clearly enough that Velikovsky's scientific critics of the 1950s, no less than Velikovsky himself, spoke out of a thoroughly positivist belief; no less so did Velikovsky's social-scientific supporters of the 1960s. It seems to me, admittedly from only cursory acquaintance, post-Velikovskian or neo-Velikovskian supporters and critics still share a

rather positivist stance.

One of the characteristics of positivist confidence is the willingness to forecast that advances will be made, moreover that quite specific advances will be made. In *Beyond Velikovsky*, I argued that confidence of that kind was, in fact, characteristic of inauthentic science. That diagnosis was confirmed for me when I saw in Korzybski's *Science and Sanity* (1933), 11 volumes identified by author and title as well as another 52 works identified by title only listed as "in preparation." When I now look at Kuhn's *Structure of Scientific Revolutions* (1970), I detect the same positivist confidence. The book is self-described as belonging to Volumes I and II of the *Foundations of the Unity of Science*, being Volume 2, Number 2 of the *International Encyclopedia of Unified Science*. The front cover of my paperback copy of the 2nd edition of Kuhn's book places the cautious modifier "Toward" in front of "an *International Encyclopedia of Unified Science*." The back cover tells us that "Circumstances during World War II and the death of Professor Neurath...limited the scope of the *Encyclopedia*," which was evidently the grand vision of an individual belonging to some positivistic time and place. (It is quite interesting to note the people listed under the grand project of the *International Encyclopedia of Unified Science*: editors Otto Neurath, Rudolf Carnap, Charles Morris; Committee of Organization, those three plus Philipp Frank, Joergen Joergensen, Louis Rougier; Advisory Committee, Niels Bohr, Egon Brunswik, J. Clay, John Dewey, Federico Enriques, Herbert Feigl, Clark L. Hull, Waldemar Kaempffert, Victor F. Lenzen, Jan Lukasiewicz, William M. Malisoff, R. von Mises, G. Mannoury, Ernest Nagel, Arne Naess, Hans Reichenbach, Abel Rey, Bertrand Russell, L. Susan Stebbing, Alfred Tarski, Edward C. Tolman, Joseph H. Woodger.)

Velikovsky, insofar as his positivist leaning is concerned, in the 1950s and the 1960s was quite in the mainstream with respect to the interpretation of scientific activity. Velikovsky's determination to be given clear credit for his production belongs also to that time and place in which discreet discoveries could be assigned to discreet individuals. Without adding to what others and I have written about his precursors, let me emphasize the degree to which historians of science and of philosophy now recognize that everything intellectually substantial has precursors and that intellectual history is much more a continuity than any series of discreet steps. That marvelous insight often attributed to Newton ("If I have seen further...it is by standing upon the shoulders of giants"), has been re-discovered and re-attributed countless times over the course of a

couple of millennia (Merton, 1965). Indeed, Stigler's Law tells us that "eponymy is always wrong: a discovery is named after the last person to discover it, not the first; because once a discovery has been named, no one else tries to claim it as a discovery" (1980; see also Good, 1985). Appropriately, Stigler validated his own law by showing that he was not the first to formulate it.

What Velikovsky Really Taught Us

Some of Velikovsky's scientific critics waxed furious over what they saw as his attack upon science. From the vantage point of today's circumstances we can see clearly that Velikovsky's quarrels over the detailed content of science were much less an attack on science itself than are the attacks now stemming from intellectual relativists and social activists of various stripes, as delineated by Gross & Levitt in their recent book, *Higher Superstition*. These post-modernist attacks are directed at the very basis of science itself, not merely on the validity of certain bits of knowledge or a specific theory. Velikovsky was of his time and place in believing in the possibility of knowing. He was also driven by an urge to understand, rather than by the urge to bend knowledge to the service of partisan ideology, as are today's barbarians and know-nothings. Velikovsky thought that much of accepted science happened to be wrong, but he did not believe the enterprise of science to be a wrongheaded activity vitiated from the outset by the impossibility of knowing anything, as all too many contemporary pundits do.

In many significant ways, then, Velikovsky was properly faithful to humankind's level of understanding given his time. At our distance I want to suggest that it has become largely irrelevant that Velikovsky's science was largely wrong. After all, if we insist that everyone's worth be measured by the degree to which their beliefs are objectively correct, then we are all in the deepest trouble. It makes much more sense to follow the historians' path of trying to understand people and events in their own context. In making judgments of people's value or worth we should recall Madame de Staël's wisdom that "to understand is to forgive" and "to understand everything makes one tolerant" (1968). Looking back and judging by what we now believe or know is what historians pejoratively call "presentism" or "Whiggishness." It is not irrelevant to note that historians themselves reached that understanding only relatively recently.

Even error can produce beneficial by-products or spin-offs. In fact, lots of errors have left lots of beneficial by-products. Everything we now value has its roots in the past, a past riddled with mistakes and misunderstandings. Science



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progresses as much by discarding things later proved to be wrong as by discovering "absolutely" right things. We are slowly becoming used to the view that what science now believes is not what science will believe in the future.

With this view in mind, consider some of the more notorious people whom I have mentioned as sharing some of Velikovsky's time and place. Most remember Wilhelm Reich only for the grand errors of his later years, his spurious discoveries of orgone energy and of spontaneous generation, his imagined battles against UFOs; yet, in my opinion, some of Reich's therapeutic insights and approaches remain useful, and his bringing together of psychological insights and social activism was an important advance in its time.

Korzybski is largely remembered as a single-minded crank, yet still-extant journals and societies illustrate his positive influence on issues of expression and thought. Martin Gardner writes about "the anthroposophical poppycock of Rudolf Steiner" (1957); yet dozens of schools that owe something to Steiner's then-progressive ideas about education continue to flourish, to be highly regarded and prized.

Sigmund Freud, if we are to believe a torrent of debunking recent works, was wrong in just about every particular, and untrustworthy to boot. But even if so, innumerable among us

have been heavily influenced by his writings or from trying to use his ideas and insights.

Being wrong is human. I like to recall with my daughters the occasion when they began to use the refrain, "No one's perfect, not even Daddy." My own mentors Breyer, Iredale, Elving were quite often wrong. Their feet were made of the same clay as are ours. Nevertheless, their influence on me was wonderfully beneficial. I am often conscious of that, and of the fact that therefore I do some things not only because of them but to some extent on their behalf. They have a genuine measure of immortality as they live on in me.

We do our forebears no service by seeking, in this new time and place, to disguise their errors or, far worse, to re-commit mistakes that they made. After all, there are many other things to recall about them than their errors. All told, it is obvious enough that everything we hold dear and good comes to us from them, because there is no other place or time from where it could have come.

I suggest that we move beyond the "debunking" stage to a stage where we can honor Velikovsky's memory without stint by focusing on the beneficial influence he and his work have had on many of us and by reflecting on what he enables us to understand better about his time and place. □

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