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R. J. Connelly

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R. J. Connelly, Ph.D.

The author of a recently published book, Whitehead vs. Hartshore: Basic Metaphysical Issues, *Professor Connelly was Moody Professor of Philosophy at Incarnate Word College where he presently teaches. He and his wife were appointed Danforth Associates, effective 1979-85.*

In a recent review of *Heraclitean Fire: Sketches from a Life Before Nature*, Evelyn Fox Keller observes that the author, Erwin Chargaff, is "a scientist with a different mission. He wishes to use his life as a levee against the corruption of contemporary science, a corruption he thinks is endemic to our times."[1] Chargaff may be mistaken about the degree of corruption, but his own personal suffering within the scientific community serves to remind us that science is a human activity, limited, and subject to human failing like everything else. But the dark side of science seems to warrant more attention than is implied in the familiar protest that those with dubious intentions are misusing or abusing science. Perhaps we can better appreciate the multiple possibilities for corruption if we acknowledge that the total context in which science is done is value-full, that science is not a privileged value-free domain just because science claims objectivity, that value judgments pervade scientific activity and therefore are fair game for evaluation according to value standards and principles. This realization seems to have emerged very slowly because the myth of objective science has clouded our perception of the many value dimensions of modern science. This paper will review briefly a few key dimensions in order to place the myth in better perspective and encourage us to be more objective about objectivity in science and the possibilities for corruption in science.

The myth of value-free, objective science has gradually evolved during the last 250 years. Myth in this context can be defined as a unified set of symbols that elicits an imaginative response involving recurring patterns of belief, commitment, and action. Many people cite

Descartes's body-soul dualism as the beginning of the myth. Descartes turned the physical world into one vast complex machine divorced from intrinsic soul principles which are only to be found in rational human beings. The physical world therefore could be approached by the scientist, with the proper preparation and discipline of course, as "object," not eliciting or responding to emotion, interest or subjective bias. Ideally, values were irrelevant to the study of this mechanistic world. Human beings were the exception considered in that respect which was most human, most subjective and the source of values, the rational soul. Whatever the origins of the myth, it is still with us today, though shaken precisely because of value judgments by counter-culture and anti-science movements, citizen and consumer demand for greater accountability, congressional pressure to divert funds from basic to applied research, and growing concern among scientists like Chargaff who want to place limits on certain controversial areas of research.

First consider the myth in outline form. As French Nobel laureate Jacques Monod articulates it:

> The adoption of the scientific method, defining "true" knowledge as having no possible source other than the objective confrontation of logic and observation, eliminates *ipso facto* the animist assumption of the existence of some kind of subjectivity in nature. The absolute *objectivity* of nature is the basic postulate of the scientific method. . . .[2]

Freedom of inquiry then must be protected at all costs so that scientists can be about their objective business of following well-defined rules to accumulate data, establish facts and test hypotheses. Raising value issues and making value judgments are simply not the concern of scientists functioning as scientists. Nor is it a scientific concern that society may on occasion misuse or abuse the value-free, value-neutral results of scientific inquiry. Science indeed can show us *how* to achieve defined goals but not *which* goals to aim for. In sum, science deals with objective facts not subjective values.

The purpose of this paper is not to aim one more attack at the myth of objective science in hopes of destroying it once and for all. Nietzsche, a would-be major myth killer in the 19th century, called for the "death of God," meaning that the Christian myth and concept of deity had outlived their usefulness and in fact were contributing to the disintegration of European civilization. For much the same reasons and in the same spirit, B. F. Skinner in *Beyond Freedom and Dignity* more recently has called for the death of the Autonomous Man or the myth of Individualism. It is certainly questionable whether the myth of objective science has outlived its usefulness. At least some demystifying is in order to temper misrepresentations of science that stress its near-absolute objectivity and value-free character. Insufficient attention has been given in the past to questions more or less appropriate to a philosophy of science. That is, what is the nature of the scientific method, its underlying assumptions, its limits, and what is the significance of the total value context in which science operates? The responsibility for pursuing such questions rests with professionals in the sciences and the humanities as well. Science misunderstood in our society today inevitably will be science misused. And we all suffer the consequences, not just the Chargaffs.

We intend the following discussion to be suggestive only — a challenge to professionals in the sciences and the humanities to create more cross-disciplinary opportunities for dialogue not only about values in science, but also the worth of greater value awareness in science. The end result would be, perhaps, a healthier, more realistic "myth" of objective science, one of greater benefit to society in the long run.

The thesis of this paper is that scientists may and indeed should aspire to even greater objectivity, but they and all of society should not underestimate or ignore the significance of the ever-present, value-laden context in which science is done.

For purposes of this paper, we will describe "value" as anything which can be conceptualized and prized, and both leads to action and serves as a standard of action. Without delving into the metaphysics of value, or various theories of ethics, or even deciding whether some values may be constant or absolute rather than relative from culture to culture, let us focus on the experienced fact that values are real in mind and in human behavior. An intelligent creature capable of having ideas is our starting point; experience reveals that certain ideas can acquire a quality or character of being of significance, relevance, importance, or desirability to the individual — in sum, prized. Such ideas I designate as incipient values. They merit the title of lived values when they become manifest in human behavior. Thinking about ideas is certainly action of a sort. But values are more strictly themselves when they serve as a springboard for overt externally manifest behavior. Hypocrisy is the name for values which remain incipient and are never translated into overt action. We are not always fully conscious of our values. But an analysis of our behavior or a process of values clarification can reveal those lived values which constitute our present value system. Every human being of course has such a system, whether or not the values that comprise it are consistent with one another. Coherent or not, the system may be broken down into categories such as political, economic, legal, socio-cultural, esthetic, and religious values. In any of these classifications, when a value assumes relatively major status and has bearing on what it means to lead the good life in some ultimate sense, we may designate such a value as moral or ethical.

Moral and non-moral values are relational; that is, they signify that an individual is establishing connections with the world around him. Values exist primarily in our mind but have a vector quality. They give direction to human behavior which in turn shapes the world of the individual according to how the individual thinks the world ought to be, not just how it is in fact.

Because value decisions have to do with setting priorities for action, and because without setting priorities (detemining to do this rather than that) no action is really possible, value decisions, whether conscious or unconscious, are behind every instance of human activity. This point of view encompasses both trivial and momentous actions ranging from deciding whether to wear a tie or initiate all-out nuclear war. Even so-called spontaneous or random activity may involve value decisions. Painters in the action movement, for example, placed high value on the intuitive-instinctive process of producing a piece of art, rather than a more cerebral or rationally controlled process. "Spontaneity," of course, is just as much a value as "rational control." Value decisions, then, are as inevitable and as commonplace as directed human activity. If engaging in science is a directed human activity, it inevitably involves such judgments.

The real issue, then, is not whether scientists can engage in value-free activity or refrain from making value judgments in science, but whether we can identify and clarify the more significant and, in particular, the ethical values which are actually involved in doing science in our society today. We assume that increasing sensitivity to value issues in science is a necessary condition for more responsible conduct of science as well as more responsible attitudes on the part of society toward science and its role in the world.

The Training of a Scientist

It is obvious that the scientist is not born or bred in an ivory tower, but is very much the product of his times and his society. Educational institutions, which provide the training ground for future scientists, tend to reinforce the dominant values of society.

All students, for example, learn the basic lesson of competition among individuals. Traditional approaches in higher education stress the need to achieve mastery of subject matter in relation to other students and often at the expense of the others. After all, the A-through-F grading system, to make any sense at all, requires some failures if there is to be any contrast between succeeding and failing. If everyone receives A, the grade is worthless. Those who are concerned about national trends toward grade inflation perceive correctly, it seems, that the inflation weakens the competitive system. There are other signs, however, that a reaction is setting in and the system will regain its previous "strength," such as it was.

Does competition affect or infect the way science is done? Consider the following. From our own perspective it seems that for many scientists science has become just another job (true of other professionals, of course). A major objective, then, is staying on the job. For example, we could cite the figures which show the job picture for new physicists or whatever, is discouraging. The other side of that coin is that "old" physicists will be even harder pressed to survive on the job. Publish/produce or perish is a common enough refrain in higher education. Visible, publicly acknowledged results are all. In such an environment it seems inevitable that quantity of output will begin to dominate and quality and social relevance suffer. Yet journals proliferate to accommodate increasing demand from institutions and individuals. Ironically, possibly the most interesting scientific work is not published at all because it must be kept secret for industrial or national security reasons. Journals do supply a forum for professionals to give evidence of continuing productivity, unmindful that the number of articles, like body counts in war, for example, may be a superficial and simplistic measure of success — certainly teaching success. One cause of superficiality is pressure to produce tangible results. This pressure must have import on the choice of problems to be studied and researched as well as the quality of effort that goes into the published product.

Take the situation of an individual who is relatively insecure in his research job with an institution. The insecurity may lead the individual to consent to do research, or, in the extreme case, to falsify experiments that he judges objectively ought not to be done. The justification for participating may be that survival is at stake: providing for family, children's education, house payments, maintaining a certain level of affluence. There are two kinds of competing value judgments involved here. The researcher definitely affirms the importance of self and family survival, but he also affirms, through his behavior (even though there is a feeling of being compelled), the values and objectives of that institution which demands the research.

Another alternative, of course, is to risk not surviving in whatever sense, and support values respecting scientific research which are in conflict with those of the institution. Non-survival in our society usually means sacrificing what is commonly understood as quality of life and rarely the fatal options of starvation, prison, or death which faced Russian scientists in the past and apparently continue into the present.

If an individual is relatively secure in his research job, competition may take another form and move to a different level. Survival on the job may be replaced by such values as achievement of power, prestige, social privileges or intellectual superiority. These goals in turn dictate that research will be chosen which promotes achievement of just these goals.

And whether we are dealing with the average scientist or the superachiever, competition to produce results seems to penalize the unorthodox, eccentric, maverick scientist. Perhaps Chargaff's life illustrates this point. Part of the purpose of competition is to insure strict conformity to prevailing scientific practice. The same value of staying within accepted bounds is the rule in most institutions of higher learning as well. Again, this seems to affect the kind and quality of science and education we have in society today.

The professionalization of knowledge tends to reinforce this conformism in science. Very early in his education the scientist learns that being a professional means becoming specialized in a particular region of thought. This is one way to make knowledge effective and research thorough, but it has its dangers. As A. N. Whitehead put it over 50 years ago:

> It produces minds in a groove. Each profession makes progress, but it is progress in its own groove. Now to be mentally in a groove is to live in contemplating a given set of abstractions. The groove prevents straying across country, and the abstraction abstracts from something to which no further attention is paid. But there is no groove of abstractions which is adequate for the comprehension of human life. Thus in the modern world, the celibacy of the medieval learned class has been replaced by a celibacy of the intellect, which is divorced from the concrete contemplation of the complete facts. Of course, no one is merely a mathematician, or merely a lawyer. People have lives outside their professions or their businesses. But the point is the restraint of serious thought within a groove. The remainder of life is treated superficially, with the imperfect categories of thought derived from one profession. [3]

Whitehead goes on to criticize traditional educational methods that have grown up with the ideal of professionalism and still prevail today.

> My own criticism of our traditional educational methods is that they are far too much occupied with intellectual analysis, and with the acquirement of formularized information. What I mean is, that we neglect to strengthen habits of concrete appreciation of the individual facts in their full interplay of emergent values, and that we merely emphasize abstract formulations which ignore this aspect of the interplay of diverse values. [4]

Overspecialization can lead to narrowing of vision concerning what constitutes a scientific problem. It can also lead to analysis of problems isolated from their real life value-laden context. Not that values can be eliminated from the analysis. Whitehead's point is that too often too few values are considered by the specialist, and this tends to distort the specialist's view of the way things really are.

Interestingly enough, there is a good practical reason for avoiding overspecialization. In some fields there is a growing need to educate scientists to career alternatives in light of the job market crunch. Beverly J. Alexander, for example, cautions educators in chemistry:

> We must discourage some of our students from specializing too much because so many of today's problems are complex and cannot be solved by specialists alone.
>
> I think they are too important to leave entirely up to non-scientists. We chemists have a valuable contribution to make in areas such as energy, environment, conservation of natural resources, new materials, and food. And I don't think our contribution should be solely at the laboratory stage. [5]

Whether for practical reasons or as a matter of principle, we may agree with the need to encourage more of a balance between generalized and specialized education and with the importance of multi-disciplinary approaches for dealing with complex, multi-faceted problems in contemporary society.

We should mention briefly two other areas of concern that seem to be connected with the notions of competition and professionalism. Besides competition within a profession, professions as institutions have a tendency to be in competition with one another over the limited economic resources of society. Many problems and issues are involved here. Let us focus on just one. In the context of intra- and inter-professional competition, scientists along with the rest of society seem conditioned to accept without question the value that scientific knowledge is private property and that an individual succeeds by accumulating the most or the best or by selling it for profit. In our society this is not an unusual development. We do have other models, however, present and past. There was a time when scholarly work was not signed, when plagiarism was not a problem, because scholars were accustomed to duplicating the work of others without acknowledgement, adding to it and passing it on in the name of enriching the intellectual heritage of mankind.[6] In our society, and perhaps rightly so, we are definitely in the age of individual right to private property, of copyrights and patents, and plagiarism as an evil. It still seems important to raise the question: is scientific knowledge best understood as a commodity, produced and owned by individuals, to be used for bartering in the marketplace? The same basic question has been asked about health care, for example: is it a right, a privilege, or a commodity that goes to the highest bidder? Is private property the best model we have for understanding these issues? It goes without saying, private property is a paramount value in our society. Scientists need to ask, is it central to the development of scientific knowledge? Should it be?

A second area of concern has to do with the type of person who becomes a scientist. Like other professions in our society, competition for membership results in a majority of scientists being white males from middle-class backgrounds. Upon graduation they return to the middle or upper-middle class. The relative exclusion of lower classes, minorities and women from science professions suggests the need for critical questioning. Does the under-representation of groups just mentioned deprive science of needed perspective that could make a difference in the kind of problems selected for study and research? Alfred McClung Lee, in his presidential address to the American Sociological Association in 1976, observed that sociologists "usually do not transcend their own habitual intellectual orientations related to their sex, social class, ethnic, racial, and other identities."[7] Background undoubtedly colors scientific inquiry in the social and behavioral sciences. The amount and degree of coloring may be different, but we suggest that the natural sciences are not exempt from the problem. The question is, in what sense is science human? Howard Smith, from the University of California at Berkeley, once stated, "The 'humanity' of physics resides with my own humanity. Physics is human because I am human and not because it has something 'to do with human relations.' "[8] We can agree with the concept, but then wonder to what extent the values and background of the man carry over into his physics.

Are scientists self-critical enough to be aware of the values at stake? For instance, it is still both fashionable and economically and politically profitable to be a part of the war on cancer. In comparison, there are only minor skirmishes against malnutrition or tropical diseases, for example, which plague a sizeable portion of the world's non-white population. Should we expect white male middle-class scientists to have values any different from those of society's mainstream? Perhaps not. But neither should we blindly accept the sort of arguments which claim the values of scientists as persons who have nothing to do with dictating the direction of the science they do.

Dominant social values pervade the training and shaping of the scientist. The value system of the scientist in the concrete necessarily influences the science. This makes even more sense if we stop to think that scientific knowledge does not have independent existence once produced. Science primarily exists in a human mind thinking scientifically — the same mind that continuously makes value judgments. In the real world it seems impossible to separate the person who is scientist from the science.

Technological Applications of Science

Similarly, it seems impossible to separate science from related technological applications. This is contrary to the usual reading of the myth of objective science: the production of scientific knowledge is one thing; the use or misuse of that knowledge is something altogether different. The myth presents science as value-neutral and therefore somehow beyond good and evil.

This aspect of the myth made more sense in pre-modern times. John Dewey discusses the origin of what he calls the spectator theory of knowledge. In the ancient Greek world, theorizing in general was the prerogative of the privileged leisure class. Their meditation or contemplation of the truth was not to be tarnished by association with daily, routine practical matters. Such was the responsibility of the lower classes. Knowledge, therefore, was strictly theory, a way of "looking on" the world like a spectator, rather than a way of being actively involved in the world. Thinking was not connected with making or doing. Theory and practice were kept distinct.

This dualism was plausible perhaps until Francis Bacon in the early 17th century urged science to aim at power over nature for the purpose of improving the material conditions of existence. Knowledge *is* power to control and manipulate nature. As Bacon puts it, "Human knowledge and human power meet in one."[9] Consequently, theory and practice must be intrinsically connected. Today we have come to realize, some would say with a vengeance, Bacon's ideal of taking full advantage of the theory/practice connection.

The value of "knowledge for its own sake" may still inspire some scientists. But it never was true, psychologically or any other way, that the search for scientific knowledge only affected the researcher and never other human beings or the environment. There is no such thing as knowledge without effect in the world. The growing interconnection between the enterprises of science and technology merely sharpens our perception of this fact. "Scientific knowledge for its products or benefits" has all but replaced the ancient ideal of pure, disinterested theory.

Can there be serious doubt that technology is wedded to science, for better or worse? There is no branch of science in which discoveries do not have some technological applicability. Every discovery invites consideration of technological applications even to the point of initiating new technologies. Hans Jonas shows very convincingly that the relationship is causal, not just accidental.

> First, much of science lives on the intellectual feedback from precisely its technological applications.
>
> Second, science receives from technology its assignments: in what direction to search, what problems to solve.
>
> Third, for solving these problems, and generally for its own advance, science uses advanced technology itself: its physical tools become ever more demanding. In this sense, even purest science now has a stake in technology, as technology has in science.
>
> Fourth, the cost of those physical tools and of the staff to use them must be underwritten from outside. The mere economics of the case calls in the public purse or other sponsorship; and this funding of the scientist's project (even with "no strings attached"), is naturally given in the expectation of some future return in the practical sphere.
>
> There is mutual understanding on this. The anticipated payoff is put forward unashamedly as the recommending rationale in seeking grants or is specified outright as the purpose in offering them.
>
> In sum, science has its tasks increasingly set by extraneous interests rather than its own internal logic or the free curiousity of the investigator. This is not to disparage those extraneous interests nor the fact that science has become their servant, that is, part of the social enterprise.
>
> But it is to say that the acceptance of this functional role (without which there would be neither science of the advanced type we have nor the type of society living by its fruits) has destroyed the alibi of pure, disinterested theory. It has put science squarely in the realm of social action where every agent is accountable for his or her deeds.[10]

Where scientific knowledge is power to be applied in the world, by necessity moral value and moral responsibility come into play. The greater the power the greater the responsibility. Scientists, because of their vantage point on the inside of the process, perhaps more than the general public, must accept responsibility for technological applications of their science.

Scientific Method Itself

Sandwiched between the topics of the "training of a scientist" and the "technological applications of science" we find the meat of the myth of objective science — the scientific method itself. There are those who might admit at this point that value judgments are involved in the decision to become a scientist in the first place, and later a specialist; in the selection of problems or foci of interest for investigation; and in the technological applications of theory. But they still might resist concluding that value judgments are intrinsic to the procedures of science, the scientific method itself. If extra-scientific value judgments get mixed up in science, they say, this is more a commentary on the limitations of the scientist than the method since the scientific method is only a tool which does not logically involve value judgments.

A number of commentators in recent years have raised serious questions about the idea of a value-free method. Let me briefly summarize a few main points.

T. S. Kuhn in his landmark work *The Structure of Scientific Revolutions*[11] shows the significance of paradigms in normal scientific activity. He describes paradigms as "universally recognized scientific achievements that for a time provide model problems and solutions to a community of practitioners."[12] In other words, normal scientific activity is governed by some basic theory which is accepted as true and has been applied to many of the problems in the field. The goal of continuing scientific activity is the successful use of the established theory to solve the remaining problems in the field.

Kuhn notes that normal, but not revolutionary, science tends to be dogmatic. The typical scientist is not concerned with validating the paradigm-theory: he merely accepts it, consciously or unconsciously, and then spends his time articulating the paradigm and interpreting new data according to it. Kuhn claims this dogmatism has many advantages. He even thinks that the presence of a paradigm can aid in the recognition of the need for revolutionary change to new paradigms.

The main point for our purposes is that Kuhn shows very clearly that adherence to a paradigm in science is a matter of sharing certain values and standards, rules, assumptions, and points of view in a scientific community. In effect, a paradigm defines a certain value context which includes the values of the paradigm and excludes other possible values. The value context in turn guides the use of the scientific method in relation to the paradigm.

Other commentators have focused on the traditional distinction between theory and observation. N. R. Hanson, Paul Feyerbend, Henry Margenau and others try to show that all observation is theory-laden, that we build our theories into our observation more than we build our observations into our theories. This means it is not possible to isolate a class of scientific statements which can be known directly and from which the remainder can be inferred by some inductive scheme. Observations as well as theories must be dependent on inferences. In other terms, reasoned judgments rather than so-called acts of simple observation are the basis for determining the acceptability of scientific data. What seems to be implied in this analysis of the theory-observation relationship is that every theory constitutes a value or value-system which functions as a standard for selecting data relevant to that theory. This would be consistent with the above interpretation of Kuhn.

Richard Rudner was one of the first philosophers of science to call attention to the role of value judgments in the "acceptance" of scientific theories. His premise is that validating hypotheses is intrinsic to the scientific method. That is, the scientist is in the business of deciding when a hypothesis is true enough. Since perfect verification is not possible, the scientist must decide when the available evidence is "'sufficiently' strong or that the probability is 'sufficiently' high to warrant the acceptance of the hypothesis."[13] "Strong enough," according to Rudner, involves a value judgment concerning the importance of making a mistake in accepting or rejecting the hypothesis. He asks, for example,

> how high a degree of probability the Manhattan Project scientists demanded for the hypothesis that no uncontrollable pervasive chain reaction would occur before they proceeded with the first atomic bomb detonation or even first activated the Chicago pile above a critical level. It would be equally interesting and instructive to know how they decided that the chosen probability value (if one was chosen) was high enough rather than one that was higher; on the other hand, it is conceivable that the problem, in this form, was not brought to consciousness at all. [14]

The issue is whether a mistake would make any significant difference according to our moral standards. "How sure we must be before we accept a hypothesis depends on how serious a mistake would be."[15]

Rudner implies, I think, that we have entered a new era. Gathering evidence is not simply a function of innocent observation. Active intervention in nature or experimentation is critical to all of modern science; animate nature is becoming more and more the subject of experimentation. A scientifically controlled atomic explosion may serve the interests of some theory or other. But it inevitably affects the whole atmosphere and therefore human life present and future. The exploding field of biomedical research even more graphically demands, not just occasions, experimentation on human subjects present and future. But when the animate world itself becomes the laboratory, experimentation and scientific method with it lose their innocence, and moral issues command attention.

Rudner offers an excellent conclusion to this section and our paper as a whole:

> The traditional search for objectivity exemplifies science's pursuit of one of its most precious ideals. For the scientist to close his eyes to the fact that scientific method *intrinsically* requires the making of value decisions, and for him to push out of his consciousness the fact that he does make them can in no way bring him closer to the ideal of objectivity. To refuse to pay attention to the value decisions that *must* be made, to make them intuitively, unconsciously, and haphazardly, is to leave an essential aspect of scientific method scientifically out of control.
>
> What seems necessary . . . is nothing less than a radical reworking of the ideal of scientific objectivity. The naive conception of the scientist as one who is cold-blooded, emotionless, impersonal, and passive, mirroring the world perfectly in the highly polished lenses of his steel-rimmed glasses is no longer, if it ever was, adequate.
>
> What is proposed here is that objectivity for science lies at least in becoming precise about what value judgments are being made and might have been made in a given inquiry — and, stated in the most challenging form, what value decisions ought to be made.[16]

As a final comment, we should re-emphasize the idea that the "radical reworking of the ideal of scientific objectivity" be the responsibility of a cross-disciplinary effort involving both scientists and humanists. Perhaps a concerted effort would provide better perspective on the issue of corruption in contemporary science. Then we can see how much help Chargaff deserves at the levee.

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