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ABSTRACT

Several science education researchers are embracing postmodern relativist notions of both evaluation methodology and of the structure of all scientific knowledge. They blame bad educational evaluation and research on several causes, the worst being the following of the fundamental tenets of mainstream science. These researchers feel science educators trying to act like scientists have caused their own demise. However, several facets of this discussion have been ignored. Problems appear in the stated beliefs and views of these science education researchers. These problems are discussed through the current and former theories of educational philosophy. The need for balanced perspectives when discussing problems in science education is also probed. The views of science education should not be tied to two extreme theories rather they should relate to the many philosophies of science and science education. Contains 69 references. (MVL)

From the Summit of "Truth" to the "Slippery Slopes:" Science Education's Precarious Descent Through Positivist-Postmodern Territory

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Part 1: ORIGINS

In a *Journal of Research in Science Teaching* editorial entitled the "Slippery Slopes of Postmodernism," Good (1993a) asks readers to beware of the affinity the National Committee on Science Education Standards seems to have in its 1992 draft document with what he calls the "wispy world of postmodernism." He also implies that there is little value in reducing explanations of science to pigeon-holing every interpretation as either positivist or postmodern. Guba and Lincoln (1989) in their widely read *Fourth Generation Evaluation* see things a little differently. They do, in fact, just what Good warns us about--embracing postmodern relativist notions of both evaluation methodology and of the structure of all scientific knowledge. Guba and Lincoln seem to blame bad educational evaluation and research on several things, the most villainous being the fundamental tenets of mainstream science. Trying to act like scientists has really "done in" the first, second, and third generation of evaluators, according to them. This is probably true if their description of science is what has been followed.

Guba and Lincoln (1989) describe contemporary science rather unrealistically. They describe it as having *a* method, a clear separation between observational and theoretical terms with discovery being outside the main scientific process, and of adhering to a John Stuart Mill brand of empiricism. They summarize science, rather brusquely and with little detail or range of examples, as a kind of "parochial absolutism." Attaching the term "positivism" to much of mainstream science (which is, of course, "white male," and hopelessly "political"), they seem to reduce it to a singular stance. The summary statements about science as a whole--unlike their carefully crafted justification for holistic, qualitative approaches to evaluation--are too narrow to be an effective polemic.

Other than Thomas Kuhn and Michael Polyani, there are no apparent references to any post-positivist works in philosophy of science by Guba and Lincoln (1989), some of which focus on how scientists work, not just how they should work (Giere, 1988 for example). There is, therefore, little indication that the authors have adequately explored or even acknowledged the range of views within the philosophy of science community, nor within the different disciplines of mainstream science that have existed for at least forty years. (Anyone who thinks evolutionary biology goes about its work the same way that theoretical physics does is ignoring critical pieces of information about methods, aims and theories in each discipline). On the other hand, one can identify easily at least fifteen entries in Guba and Lincoln's book with clearly feminist approaches. While many of these encourage us to look at the scientific enterprise in new ways, the absence of other post-positivist dimensions is too apparent. In the end, the authors' notions of how science ought to be done reduces to their pronouncement (with no qualifiers) that "there exist multiple, socially constructed realities ungoverned by natural laws" (p. 86). There is, therefore, a need to explicate both the terms "positivism" and "postmodernism"--beginning with their origins---and to put them into a perspective that will be helpful to today's science education community. This is the purpose of this paper.

In his 1980 book, *Theory and Evidence,* philosopher Clark Glymour began by stating, "If it is true that there are but two kinds of people in the world--the logical positivists and the god-damned English professors--then I suppose I am a logical positivist " (pg. ix). It would be unfair to assume from this one statement that he embraces that philosophy--he does not. He is expressing misgivings about much of what those at the other end of the spectrum have to say; and his statement captures the spirit of a debate at least twenty-five years old and still alive today.

The key to "mainstream" philosophy of science is the analysis of the structure of theories, since they are the "vehicle of scientific knowledge" (Suppe,1977, p. 3). Theories in science consist of a closely related system of statements, whether they be accepted laws, propositions, assumptions or rules of procedure that together serve to predict, analyze and explain phenomena in nature. Terms like "fruitful," "rigorous," "core," "center," "fringe," are often used to categorize a theory's usefulness, stability or general acceptance. Plate tectonic theory, evolutionary theory, quantum theory are examples of contemporary, highly complex systems of explanation, prediction and analysis. How one construes theories more than any other single problem determines their philosophical bent. The method by which one arrives at a theory, the interpretation of the language associated with it, the relation of one theory to another, the role theory plays in observation and subsequent experimentation are but a few of the questions typically dealt with by philosophers of science.

History of Positivism

One view concerning the role of theory in science originates in the thinking of Plato, who believed nature held certain unalterable ideas which must be interpreted. Centuries later came the so-called Copernican Revolution, made possible largely by development of the brilliant theoretical/mathematical models accompanying the experiments of Galileo (Matthews, 1994), along with the observations of Brahe and the mathematics of Kepler. Francis Bacon, as a lawyer with deep concern for the rules of evidence, conceived "modern" science as dealing with a nature that was there for man to discover and define through a new kind of induction. This started with Bacon's rejection of the Aristotelian insistence on absolutist theories whose causal analysis had to be final and ultimate, yet whose methods amounted to "naive examination of experience" (Suppe, 1977, p. 684). Bacon's two notion's of induction, one depending on pure discovery, the other a more practical step-by-step method involving observing then testing guesses (hypotheses) along the way, carved the way for a "logic of justification."

By the eighteenth century, Kant's *The Critique of Pure Reason* and to a greater extent ***The Metaphysical Foundations of Natural Science***, according to Patrick Suppes (1977), supported ideas that scientific knowledge, that is, general propositions alleged to be true were largely a priori judgments whose truth is assured by the nature of our faculties that were not dependent on experience but still concerned the phenomenal domain. While quite distinct from Aristotelian empiricism, Kant's transcendental idealism added a new kind of absolutist tradition.

Later, the idea of Kant's a priori judgments was defeated by those who, in their Neo-Kantian writings, said the quest to find the key to nature's secrets should be through experience only--pure empiricism. This was best espoused by John Stuart Mill and was based on a logic of discovery with certain pure givens, or unalterable metascientific concepts definitory of science. Even without the a priori of Kant, the empiricists still held that there were ultimate laws of nature and that they were deterministic not probabilistic. The principle of a uniformity of nature was understood here (Shapere, 1984). These presuppositions about nature caused modification of the earlier inductive methods of Bacon and supported a Scientific Method of solving nature's mysteries.

These new, 19th century empiricist traditions led to increasing belief in dependence on sensory experience and decreasing belief in any a priori element of a formal character in a theory. All statements in a theory must be empirically verifiable and reduced to statements about sensations. These views were attributed to a group known as the Neo-Positivists and led by Ernst Mach [the term positivist originating in the work of French mathematician and philosopher, Auguste Compte, whose brand of mechanistic materialism can be best described as belief in the idea that a real, objective world exists independent of individual perceivers and science merely discovers the mechanisms in this objective world]. This neo-positivist view rejected even a priori mathematical models; in addition, space and time were not conceived of as absolute. The emergence of the new physics of quantum mechanics and relativity theory therefore had some support from the Machian school, but the weak link was their insistence that theoretical terms be reduced to phenomenal language. The common-sense principles of the positivist view of science ultimately were inconsistent with the theoretical terms which were a part of the new physics. Because of their mathematical nature, these theoretical terms could not be reduced to sensations.

Mathematics was not to stay out of the picture for long. Between 1902 and 1913 a series of important events and publications occurred (Suppe, 1977). Henri Poincare wrote of mathematically dependent terms like "mass" as being just explicit definitions of phenomena--mere conventions for explaining science as it was. Of course Einstein's publications on special relativity in 1905 made good use of these mathematical theoretical terms. All seemed to agree that the positivist attempt to keep metaphysical entities (probably in reference to the philosophy of Hegel) out of science was proper, since their associated terms had neither phenomenal or observational language. But what seemed to be emerging was a tolerance for theoretical terms that had with them specific correspondence rules (coordinating definitions, rules of interpretation, operational definitions, or interpretive systems) that could give an explicit definition. Many mathematical terms fit this description. Force and mass are two examples.

This newly interpreted brand of positivism soon had a powerful ally in the writings of Alfred North Whitehead and Bertrand Russell. Their three-volume *Principia Mathematica* convinced many that all mathematics could be done in terms of logic, and this formed the basis of the claim that scientific language consists of axiomatic calculi. This logical positivism was received well in philosophical circles in Berlin and Vienna as well as in this country. Two of its earliest proponents were Hans Reichenbach and Rudolf Carnap, both of whom published over a forty-year period (1920's to the 1960's) many works supporting various aspects of formal logical analysis as the key to evaluating good science. A new kind of absolutist tradition was still alive many centuries after Plato.

Logical positivism in its purest sense has been rejected by philosophers, because of the inability of many theories which still can be called scientific to be "axiomatized fruitfully," as Suppe (1977) insists. The only theories which seem to come close to this positivist criterion are those which embody an already well-developed body of empirically derived knowledge (p.64). Here there is a highly systematic interconnection of well-understood concepts--as in the case say of rigid body mechanics. The problems arise virtually everywhere else, particularly in the biological or social sciences where trying to develop rigid axioms, say, in theories on evolution or natural history would be both premature and fruitless.

When one looks at the abundant literature in philosophy of science on the subject, it turns out that positivism's rejection has occurred on all its major fronts: on causal explanations, on global induction, on the over-importance of confirmation, and on inter-theoretic reduction of theories (one subsuming the other). In contrast, there are somewhat related scientific, mathematical, and philosophical dimensions that still enjoy support in varying degrees today.

Formalism Today

What follows is a brief description of some of the tenets of what some call "Formalism" which, while not holding to the logical positivist notions of a rigid language for theories, does signify certain unchanging norms--perhaps since Copernicus. With various degrees of affiliation with elements of the positivisit tradition, names like Carl Hempel, Ernst Nagel, Wesley Salmon, Israel Scheffeler, and Robert Causey might serve as a partial roster of formalists. One of the tenets is a strong emphasis on epistemology, the study of the acquisition of knowledge. Another is the notion of a *reasonably* theory-free observation language available to all legitimate sciences. Scientific knowledge is thought to be largely cumulative, each new theory which replaces the older coming closer to a truth. In fact science might be defined here as a search for truth (with a capital "T" for some). Toulmin (1982) describes another formalist tenet--knowledge as deductive generalizations coming from pre-existing facts. The truth-seeking activity is often achieved by limiting sites on the rejection or acceptance of a hypothesis after its being proposed, empirically tested and analyzed. This was essentially the reigning philosophy of science through much of the 1950's--and despite challenges which would shake the very foundation upon which it stood--it was also the philosophy which continued to prevail in most science textbook writing and in most classroom presentations of science. Science education ignored the earth-shattering challenges of Thomas Kuhn and a group that came to be known as the Contextualists, Postmodernists, or Relativists--depending on extremes and emphases.

The Beginnings of Postmodern Interpretations of Science

With the publication of the first edition of Kuhn's *The Structure of Scientific Revolutions* in 1962, a philosophical revolution began. Trained as a theoretical physicist with a strong informal interest in philosophy, he became interested (as a doctoral candidate) in the history of science after he enrolled in a new course for non-scientists. This led to a career shift first to studying the traditional history of science, followed by interests back in philosophy where he saw the long-standing conceptions of science he had developed as a scientist and amateur philosopher begin to crumble and "their failures of verisimilitude therefore seemed thoroughly worth pursuing" (p.v). It must be noted that much of what Kuhn proposed that was most revolutionary has been heavily debated. Larry Laudan, for example, an acknowledged Kuhnian in the 1970's, wrote *Science and Values* (Laudan, 1984) largely to show that Kuhn's *Structure* is "deeply flawed, not only in its specifics but in its central framework assumptions".....and should not be regarded as the "*locus classicus.*"..for treatment of questions about scientific change.

Kuhnian views of science have also been altered quite a bit by Kuhn himself, beginning with his second edition of *Structure* in 1970, which included a 35-page clarification of what he meant by his use of the term "paradigm." In a recent publication which looks at the whole body of Kuhn's writings, Paul Hoyningen-Huene (1993) attempts further clarification of Kuhn's meaning of paradigm and his 1970 substitute term, "disciplinary matrix," both of which were so problematic for Kuhn that when he used them after that it tended to be only after further attempts at clarification (Kuhn, 1977, for example). Hoyningen-Huene's work on Kuhn does much to show his strengths and weaknesses, specifically with regard to the notion of scientific development and revolutions. For example, he focuses on Kuhn's discussion of the extent of individual scientist versus group Gestalt switches and Kuhn's special notion of a phenomenal reality versus a world-in-itself reality, which he views as inaccessible. In the Foreword to Hoyningen-Huene's book, Kuhn acknowledges his continuing need to clarify his writings--especially what he intends by the incommensurability of theories, which is explained later--and how Hoyningen-Huene's book has helped him.

Despite finding much in Kuhn's writings with which to disagree--sometimes severely--Laudan (1984) like so many others, acknowledges a great debt to Kuhn for forcing us to "rethink our image of what science is and how it works" (p. xii-xiii). Few would argue that the impact of his work was indeed worthy of being called "a landmark in intellectual history which has attracted attention far beyond its own immediate field.." as Nicholas Wade wrote in *Science* ( ).

Various labels are given to those considered part of that philosophical (and historical) revolution initiated by*Structure.* Feyerabend, Hanson, Toulmin, Laudan, Polyani, and Holton are some of those writing either supporting or modifying Kuhn's work. When compared to the formalist tradition, the terms "relativist" or "contextualist" are often used--although the degree of commitment to non-formalist views varies widely. (Using the term "constructivist" to describe one's philosophy of science seems to come more out of recent work in the social sciences as well as the art world, and the related areas of cognitive psychology and learning theory and not the "natural" sciences. So there is no tradition of constructivist philosophy of science per se, other than its connection to relativist notions of anti-realist or non-rationalist explanations). This is important to remember, as careless labeling leads to oversimplification and does injustice to many of the writings of those mentioned. One of the more extreme, acknowledged non-formalists is Paul Feyerabend, who went a step further and called himself first an anarchist, then changed that to a Dadaist ("they wouldn't hurt a fly," he said), in the tradition of the German group of artists who threw out traditional culture for dependence on whim, intuition, chance and even irrationality from time to time (Feyerabend, 1975, p.21).

Often labeled "postmodern" science, specifying its separation from Baconian beliefs (which we have seen had several reincarnations into the 20th C), this broad category dismisses positivist and many formalist tenets and concentrates on how the natural sciences are *actually* carried out (rather than how they *should* be carried out) in the context of social, political, psychological, cultural, or other dimensions of being human in a setting with other humans. One area of broad variation involves the degree to which "rational" thought--a highly explicated term itself--still prevails in decisions regarding scientific theory choice and the degree to which scientific explanations can ever be thought of as representing "reality"--another loaded term. One extreme is that one can never know the Truth, but can value theories for their usefulness. Rather than either/or answers to positions regarding these and other notions, there are sometimes subtle shades and degrees of support or rejection. While some who embrace a relativist approach applaud the view that science is highly interpretive and variable, they sometimes seem not to appreciate that the study of science--its philosophy, for example--is also highly interpretive.

One of the principal points common to postmodern philosophers is the view that history of science can reveal a great deal about what science is really like and how it is done. They are often viewed as historicists rather than logical analysts. If one glances at Kuhn's (1970) footnotes alone, it is easy to see most are either primary or secondary references on the work of Galileo, Planck, Darwin, Bacon, Newton and others. This is not to say that all postmodern philosophers of science agree on what history teaches us. Postmodern views can range from believing we can judge science only in the context of its time and not by any standards of today to believing that we cannot judge science at all! A more formalist view might suggest using history cautiously or not at all--depending on the questions being asked.

One of the biggest spin-offs from Kuhn's and other closely allied work has been detailed analysis of the degree of objectivity in observation. In this view the observer is not detached and objective but rather is a participant, or is at least a reasonable spectator (Toulmin, 1982). Observing is a theory-laden event, with the observing (seeing as) being interpreted with perception (seeing that) (Duschl, 1985). "Observation of x is shaped by prior knowledge of x" and "another influence on observation rests in the language or notations used to express what we know" (Hanson, 1958, p. 18).

This influence upon the researcher of what she knows already or what she thinks going into an investigation is critical to contextualists. In fact, according to some, we cannot arrive at the essence of a theory independent of any semantical, psychological, or socio-political contexts (Aronson, 1984). The researcher must "reconcile rational claims with a new hermeneutic richness" (Toulmin, 1982, p.96). Hermeneutics, or critical interpretation, is a new emphasis, rather than epistemological concerns, in postmodern views of science. Toulmin, for example sees all sorts of extraneous influences enter into the criteria for acceptance of theories--such things as interest, feelings, world view (geocentric, heliocentric, Newtonian, relativity etc.) prejudices, and political interests. Explanatory systems are really multiple styles of interpretation rather than being based on one set of norms. There is in this view even validity to the claim of various explanatory systems for different sciences, different intellects, different purposes, or different stages in history of a particular discipline. There is little possibility of the unity of all sciences through reduction, as the positivisits predicted, in this contextualist view.

One of the most controversial and often critiqued notions is one proposed by Kuhn involving the non-cumulative nature of science. Rather than newer theories taking the best of the older ones, or reducing them as corrections occur, adding to the knowledge base, this view promotes scientific revolutions where new theories completely replace old ones--leaving little or no room for the baggage associated with the old theory--especially the scientific language, what Kuhn refers to as the taxonomy associated with that theory. In other words the two theories are said to be incommensurable. They do not contain translatable language; therefore no meaningful communication can occur to allow for any kind of theoretical co-existence or between scientists associated with the two camps. This inevitable confrontation and resulting takeover of one theory over another is seen by adherents of Kuhn's notion of incommensurability as resulting not in a better approximation of "Truth" but a model of what works best for the time being. This view brings natural science much closer to views being offered in other disciplines, from Jerome Bruner's (1987) contextualist (constructivist) views of psychology in his book, *Actual Minds, Possible Worlds* to the deconstructionist movement sweeping America from the studies of literature to law, which in its purest form suggests the only meaning of a text comes from the reader's interpretation.

To summarize this brief description of the developmental paths of positivist and postmodern thought, there are degrees of formalism (coming out of the positivist tradition) and of postmodernism (basically a reaction to positivism and earlier forms of empiricism). As we will see suggested in the descriptions of the next section, when postmodernism reaches the stage of total relativism it involves the "deconstruction" of science to a form unrecognizable to many.

The various components of science--or of one particular discipline in science--which give it an identity and a culture can therefore only be identified after the science educator takes some philosophical stand along the spectrum which colors it as everything from a norm-based search for Truth to a richly interpreted, multi-dimensional, context-laden discipline. How one views this discipline and its culture depends on their answers to a variety of questions frequently discussed in philosophy of science circles. This in turn can also help science teachers avoid teaching "final form science," as Richard Duschl (1990, p.3) calls it, and avoid treating all knowledge claims equally; reviewing theories in isolation rather than as one element in a network of theories; and oversimplifying theories so that it is easy to ignore or not bother to accurately portray important theory changes.

Part 2:
PERSPECTIVES NEEDED FOR BALANCED VIEWS IN SCIENCE EDUCATION

With background on the origins of postmodernism and positivism from Part 1, let us consider 1) some current problems and their treatment in science education, 2) the debate in the context of other disciplines, and 3) a rationale for balanced philosophies of science for all science educators.

Problems With Reductions To Two Extremes

In somewhat simplistic terms, the argument reveals two opposing views: 1) all or at least many explanations and methods of arriving at those explanations are equally valid (since validity may be a moot point and impossible to achieve by individually constructing human beings); 2) there is a superior explanation--maybe even an ultimate truth--usually the result of what can be agreed upon as good "Western" science--involving rational thought and careful methods.

My concern is that too few who educate others about science are aware of the spectrum of current philosophies, their historical connections, and their answers to fundamental questions. For example, how are theories generated? How does evidence bear on theory and theory on evidence? What is the relationship between methods, aims and theories? Of what value is history to the understanding of how we do science today? How are history, philosohy and science related? How are physics, biology or geology the same or different in the way questions get answered, or what kind of evidence is acceptable to justify theories? Without awareness of careful arguments, questions like these are overlooked and we tend to oversimplify, trivialize, or even ignore the issues of how science is done, or how it should be done. This can lead to such extremes as suggesting all standards associated with doing science be discarded or that Western science is the only way of knowing the natural world. Surely, Western science has learned from other long-enduring conventions of exotic, non-western cultures--such as using leeches to stop bleeding, folk medicines that work, transcendental meditation and acupuncture. Also, few working scientists would rule out the human side to their discipline, be it ideology, intuition, luck, greed, personal needs, publishing pressures, etc. It is a question of whether these are the predominant factors driving good science or factors that make science a very human endeavor--despite its overall gains towards better explanations.

Too often in academic meetings from the National Association for Research in Science Teaching (NARST) to the Modern Language Association (MLA), ideas and people get lumped and labeled as either positivist or postmodern--that is, one either has blind faith in rational modes of thought and the Truth content of knowledge or she is convinced that ideology enters into every aspect of the work we do; that is, any knowledge--even that which has withstood rigorous tests of time and intense scrutiny by many--is bound and gagged by the inevitable biases and limitations of the particular seeker (or seekers) of that knowledge. Here I define ideology as powerful socio-cultural preconceptions--sometimes derived from a false or faulty consensus not always reviewed in a rational, discriminating way. Terms are dangerous here and positivist versus postmodern are both highly complex concepts with wide-ranging definitions. We saw this in the changing views associated with theories in Part 1.

Is there a kind of "political correctness" currently in science education where all interpretations are being viewed as equally valid--a step *beyond* acknowledging alternative frameworks for their socio-political-cultural-psychological significance? Is this being closely associated with multicultural perspectives such that, if one does not see interpretations or frameworks as equally valid one risks being viewed as a "scientific racist?" These are difficult questions. One recent session at NARST is illustrative. We listened to a script of dialogue between an Aboriginal and a health care worke·' (Brickhouse & Stanley, et al, 1993) indicating totally different world views regarding the value and use of high-protein foods. The food is valued as nutrition, especially for children, in the West and valued as gifts in adult relationships to Aboriginals. The result of these strikingly different world views is continued high infant mortality for children under two years of age despite health care workers' careful use of Socratic methods to dignify the alternate views while educating the Aboriginals. Similar discussion occur.'ed at the same session on the world views of Alaskan Native Americans, heavily dependent on myth for their explanations of natural phenomena. These examples were presented as equally valia *as a kind of scientific explanation* in terms of having an equal place in science class.

One left that NARST session with the notion that all possible explanations from many cultures had more than just a place from which to start discussions in this person's science class. This is equivalent to saying the Aboriginal rationale for distributing the best food to important adults was equally *scientifically valid* with nutritional value. If all explanations are equally valid and there is no attempt at understanding best *scientific* explanations, we are reduced to relativism of the worst kind. Opposing relativistic approaches to decisions about "what knowlege is most worth" in science class is *not* the same as denying the value of others' *cultural beliefs.*

Still another example at the same session was criticism lodged against the 1990 *California Science Framework* because the *Framework* cl:.rifies and supports an official position that creationist explanations are not based on good *science* and should not be given equal time with evolutionary theory. One researcher's view essentially was that since it is, in fact, a world view of a culture, or in this case a subculture--similar perhaps to that of the Native American or Aboriginal view--it has a place in a *science* classroom. "Place" was never clarified, but the implication was that equal time should be devoted to alternative explanations.

What is particularly interesting when one looks at the creationist view is that those espousing creation science are not an isolated culture whose long traditions are far from western science--and whose explanations may work well for them in their very different world. In the case of the Aboriginals, high infant mortality due to their denial that food is related to nutrition is accepted in their culture. After all it is only when the health care workers invade *their* cultural space or when *their* world views somehow invade or interfere with western life that the problem arises.

The creationists, on the other hand, are an integral part of a culture that does western science; it is all around them--they attend western schools --and for various reasons they deny the most fruitful explanations that biology has had for the last 100 years for how speciation occurs. One or more reasons are probably involved: 1) strong religious beliefs that prevent acceptance or meaningful understanding of reasoned scientific explanations, 2) ignorance, or 3) failure of Western science to be portrayed authentically to the uninitiated in public schools. All the more reason for science teacher education that informs.

There is an important distinction between the criticism mentioned above of California's stance against creation science and Cobern's (1991,1995) thoughtful work on "world view theory," which suggests that students' beliefs may actually prevent learning the best explanations in science--even after engaging activities that should encourage meaningful learning. Students' metaphysical beliefs may have to be addressed in a way that allows an opening for them -to accept information to construct better meanings. The task engagement and conceptual change research of Lee and Anderson (1993) suggests that "the success of science teaching may depend on the establishment of a kind of 'social bonding' in which both teachers and curriculum accommodate students' agendas, needs, sociocultural backgrounds in such a way as to lead the students into identifying the goals of the science class as their own " (p.). This is not the same as inviting those beliefs to be construed as equally valid in the sense of being *scientific* explanations.

For a helpful approach to teaching controversial issues that makes best use of science class time, Reiss (1992) suggests that teaching the relationship between science and religion is critical. The ways scientific knowledge differs in methodology and scope from religious, aesthetic, and psychological knowledge should result in better distinctions between the two. He suggests this can be done by being prepared when it comes up, by explicitly raising the issue during controversial topics, or meeting the issue head on in the context of the construction of all scientific knowledge.

There is a useful example of two distinct belief systems within western culture: the fundamentalist, creationist would still want the best brain surgeon by Western standards--whereas a strict Christian Scientist might opt for prayer rather than any surgery despite the western medical odds against them. The first, illustrative of very selective beliefs, whereas the latter case is sort of a blanket belief system which prevents acceptance of medical science's explanations of all kinds. One can only imagine the result of all the belief systems at work in a typical culturally diverse classroom were allowed their sway. One such example comes to mind. It was a Limited English Proficiency (LEP) biology class in Fresno, California. The teacher and I counted twenty-seven different culturally distinct groups in the class of 32. There were around ten different Spanish-speaking countries represented--each potentially affecting world view to some extent. Sharing beliefs openly upon beginning study of a controversial scientific topics and sustained, small group, *student-led* discussions that encourage a kind of Socratic exchange that is valued, encouraged and practiced could bring beliefs to the surface to be examined not by an authority figure, but by a colleague-collaborator--and finally by the believer herself. These are efforts worth pursuing.

I am currently developing a model for a more multicultural approach to science learning based on Carlos Cortes' (1993) notions of "adducation" and "E pluribus unum," which balances respect for beliefs and conceptions of best scientific explanations by making use of and valuing cultural background/beliefs, while at the same time justifying to both students and their parents the need to embrace "mainstream" ideas and tenets--for the good of the common.

To reduce all that has come to be known as good scientific explanation to the same category with myth, mysticism, belief, personal agendas, or common-sense notions of the world is not supported by history. While there may not be one inductive Scientific Method--the all-too-familiar lockstep list on chalkboards across America--there are unique aspects to what successful and good science must do to justify its explanations that involve reasoning and public discourse, testing and retesting, trying to confirm or falsify with rigor, making results public using rational arguments, or using mathematics and statistics when appropriate to support or weaken. As Lewis Wolpert points out in his book, *The Unnatural Nature of Science* (1993) these ways did not come easily to humans.

So much of what we call early science or the results of early science were in fact pure technology, which is much older than science. The first bridges and magnificent cathedrals turn out to have been built by practical experience and common sense--the principle at work being if they stood for more than five minutes they would stand forever (Wolpert, p. 29). Sometimes experimental models were built first to see if they would work, but these were not based on existing theory, nor were generalizations really attempted. Even when a series of experimental models were used to establish a design, no theory for why some worked was offered. There was, in effect, no real attempt to understand or to generalize. If devices or explanations of the universe worked, they were accepted as best or true.

It really took the Greeks--with their unique approach of vigorous discourse, highly valued reasoning, and requirements for justification of explanations to set the seeds for modern science. Even then, according to Wolpert, scientific thinking was so unnatural that it took hundreds of years for "modern" science to emerge. Today, this notion of the "unnatural nature of science" is supported by the novice-expert and conceptual change research (Larkin, et al, 1980; Reif & Larkin, 1991; Songer & Linn, 1991) and by those writing of scientific literacy (Hazen & Trefil, 1991). It may also help justify the arguments of those who encourage a kind of *scientific* literacy based on a practical Science, Technology, and Society approach--limiting learning of scientific explanations to those most necessary for survival and good citizenship (McFadden and Yager, 1993).

There has been a revival of interest in studies dealing with student, teacher, and scientists' views of the nature of science and to what extent they are congruent with current conceptions. Whereas earlier studies relied less on postmodern interpretations, they did try to get at some of the human aspects of science (Kimball, 1967-68; Carey and Stauss, 1970; Rubba & Anderson, 1978). Recent work has reassessed those earlier studies to include more postmodern perspectives (Lederman, 1992; Travis, 1993).

Several recent studies are illustrative, however, of the tendency to force people into one of two philosophical categories, sometimes through Likert-scaled (empirical) instruments of dubious validity or reliability. Here questions promote either/or categories or do not adequately address the many dimensions of science. Results tend to reduce complex formalist and contextualist interpretations of science to either positivist or constructivist. In the Pomeroy (1993) study, for example, since traditional views are the "bad guys" and promoting postmodern, naturalistic, anti-realist views of science more desirable, naturalistic methods of inquiry in science education, and not reductionist, "positivist," empirical ones should have been the author's choice. Interviewing the same people who were characterized as traditional or non-traditional by the Likert-scaled questionnaire would have helped give a more complete picture. Pomeroy suggests for follow-up using naturalistic methods which I suggest should have been incorporated from the beginning.

While we in the science education community condemn one-dimensional characterizations of scientists as white, male nerds in lab coats, too much of current work on the nature of science reduces philosophers of science who consider themselves realists and rationalists to the same fate, while everyone else seems to fall under the spell of Thomas Kuhn (to the discomfort of Kuhn). This is not unlike the old western movies where the good guys were on white horses and/or were dressed in white and the bad guys were on black horses and always had a "five-o'clock shadow"--to emphasize their shadiness. Thomas Kuhn does not attend meetings on a white horse. This is a reminder of Glymour's dilemma (in Part 1) about having to be identified as either a positivist or an English professor. He is, in fact, a meticulous philosopher of science who has dealt in great detail with how evidence bears on theory (1980)--taking what he considers the best from the logico-deductive arguments of Carl Hempel and modifying them. He is worth reading and does not ride a black horse.

Dubious Extrapolations And Neglect Of Primary Works

One concern I have with what I sometimes read and hear is that philosophies intended for one domain are "extrapolated" to explain others, often to the dismay of the philosopher. More philosophers of science have had backgrounds in physics, a substantially different domain, say, from evolutionary biology. As one philosopher pointed out recently, "While physicists look for unification, biologists look for diversity." (Allen, 1993). Of course we do have a few philosophers who acknowledge that theirs is, say, "a philosophy of biology " (Mayr, 1988).

Playing loose with a scholar's interpretations about a particular issue in a certain domain can result in misuse of work in both science and other disciplines. One can latch onto a philosophy of science and extrapolate from what the philosopher had intended to domains like applied science, social science, literary criticism, etc. Kuhn is often quoted by those in sociology (Barnes and Edge, 1982) and literary criticism (Fish, 1980) as two examples. The reverse has also occurred. Kuhn himself acknowledges that his philosophy of science in *SSR* was influenced by Gestalt psychology, Piaget's child developmental theories, and linguistic effects on world view (1970, p.vi). Some in science education are adapting postmodern interpretations in literary criticism and social sciences (Piaget, Bruner and others on how individuals construct meaning and learn) and applying them in a parallel fashion to the "natural" sciences of physics, chemistry, geology and biology--and how the best explanations come about for the way the world works. This tends to lessen the distinction between best theories for why earthquakes occur and why D.H. Lawrence's portrayal of females in *Women in Love* is so moving, so accurate, so insightful. Joseph Schwab, in an edition of his classic lectures entitled *Science, Curriculum and a*

Liberal Education (1978), suggests two truths, "poetic truths" and "scientific truths." The poetic truths are without evidence or argument and are best described here as constructions generated, perhaps, with a great deal of insight. The "truth" distinction has become increasingly muddled as we end the 20th century.

In the case of the writings found most commonly outside of the discipline for which they were intended--those of Thomas Kuhn--one notices very few biology examples, let alone any in the social sciences or humanities. Yet his notions of the naturalistic, anti-realist aspects of science and its best theories (what works best for a particular community, instead of a reasonably objective search for truth), and the way history affects theory choice are used repeatedly in both social science and the humanities--most often in terms of politicizing meaning making. It is ironic that the philosopher of science who is best known for his concept of the incommensurability of rival theories--and the concomitant difficulty (or impossibility) of meaningful communication between scientists of two opposing theoretical camps--would have his philosophical theories used in a variety of fields seemingly "incommensurable" with physics. It is not to say that some of what Kuhn wrote is not applicable to other fields, but rare is the use of Kuhn accompanied with the necessary qualifiers.

Many more quote Kuhn than those who have read beyond his famous book, *The Structure of Scientific Revolutions* (1962). Some even ignore the second edition (1970) where he grappled with heavy criticism of his many uses of the term "paradigm" from the first edition. Criticism of Kuhn led to descriptions of his "fuzzy" philosophy--often by other postmodernists like Larry Laudan (1990, 1984, 1977), once a Kuhnian, now distinctive in his separate contributions about such things as how methods, aims, and theories interact in different scientific settings. Others like Dudley Shapere (1984) disagreed with Kuhn's, Stephen

Toulmin's and Paul Feyerabend's more relativistic notions of the role of "global presuppositions" or "high-level background theory" as predominating in the decision-making processes of science. Shapere, for example, is a realist, believing that theories either a) correspond to reality (which is not the same as knowing they are true); b) are pragmatic (able to succeed and to serve in prediction); or c) are coherent (useful in explaining order and are relevant to the real world). His truth is that which works best. Shapere is not, however, anti-realist or instrumentalist as Toulmin (1982)--who differs with Kuhn in seeing theories as evolutionary not revolutionary. Toulmin also feels comfortable with theories as mere instruments or models of rules for drawing inferences about phenomena. Ronald Giere (1988), would not fit into any traditional categories easily with his realist-contextualist philosophy. Certainly there are elements of postmodernism, as he spends his time watching and recording how scientists create models on paper from what is in their heads, say in a nuclear science facility. He does, however, see certain normative standards of good science at work, rather than deconstructing to a kind of private, political motive driving or at least affecting the laboratory activities observed, as was done by Latour and Woolgar (1979). All of these consider themselves postmodernists. [For a careful analysis of the attempts of Latour and Woolgar to deconstruct science, see Slezak (1995 a,b)].

Other evidence of subtle and not-so-subtle distinctions within what are often described as uniform categories is found in research suggesting many interpretations of the notion of constructivism--a postmodernist explanation for individual knowledge acquisition and, to some, a complete epistemological theory. Good (1990b) and Matthews (1992) discuss these distinctions. Taylor and Cambell-Williams (1993) describe differences in the theories of constructivism from the personal (Ausubel) to the radical (Piaget and Von

Glasersfeld) to the social (Vygotsky) and finally to the critical (Habermas). While each is shown to be useful in varying degrees, Taylor and Cambell-Williams' favor the constructivism of Habermas which allows the genesis of knowledge to be subjective or intersubjective, the status to be provisional, the interest to be emancipatory (based on critical reflection) and the epistemic framework to be visible. This version allows an emphasis on constructivism in explaining how students make meaning, but stops short of the radical claim that it is a complete and epistemologically sound argument for how scieince comes to its best explanations.

The Debate In The Context Of Other Disciplines

What about those English professors Glymour (1980) was complaining about in Part 1? What are the English professors who are postmodern literary theorists up to and how does it affect our arguments in science? First of all there is a whole vocabulary associated with postmodernism in the humanities and social sciences. I will leave out most of the names, except one Good (1993a) mentioned in his warning *JRST* editorial--Michael Foucault. Foucault is one of the French fathers of deconstruction. In essence, everything in literature is ideology. Whatever words the author puts down were put there cast in the shadow of reigning ideologies.

Unlike the modernist writers, postmodernists revived the pessimistic tradition of the naturalists like Emile Zola, Thomas Hardy, Theodore Dreiser, Frank Norris, and the neo-naturalists, like William Steinbeck that little beyond the senses can be known. This view naturally casts a pallor over life's possibilities. Partly, perhaps, because of the influence of Darwinian explanations of how we evolved, the naturalists' reigning thought was that we are a creation of our heredity and environment with little free will. Rather than a logocentric belief in a Godcentered society, humanism took over. That is the belief that we could know, albeit indirectly, something beyond the phenomena of everyday life.

While the structuralists saw texts as potential units of beauty of the written word, post-structuralists were busy deconstructing that harmony to reveal the verbal chaos (and later the political motivation) hidden in virtually everything we do. Their philosophy is jokingly paraphrased as: there is no meaning, except our meaning, which is that there is no one exclusinve meaning.

Extreme postmodernism has, for example, reduced art to a Campbell's soup can, and dance, via "early" Twyla Thorp and Merce Cunningham, to random movements with no music and, admittedly, no meaning. It is, by the way, quite interesting and maybe prophetic of things to come that the value of Andy Warhol's art has fallen in price drastically--much more than the last recession can explain--such that Sotheby's recently withdrew most works from the auction block rather than sacrifice them at low prices.

There is an expression, and a related phenomena, known in the western world as "Fin de Siècle," the end of the century. Scholars have noticed certain predictable trends as centuries come to an end and start over again. The degree of skepticism in fields from literature to scientific interpretations seems to move from romantic to naturalistic as the century ages. The rising influence of Darwin's, Freud's, and Herbert Spencer's work in the 19th century, for example, seemed to accompany a period of pessimism because the comforting belief in a benevolently ruled world was threatened.

While there was great upheaval in the early part of this century with Einstein's theories, the value of scientific rationalism, empiricism and faith in human objectivity endured through versions of philosohers of science like Rudolf Carnap, Carl Hempel, Karl Popper and others. Although a rationalist and a realist, Karl Popper (1982, 1983), who recently died after sixty years of being the "scientist's philosopher," really questioned the ability of science to verify or confirm--instead promoting the proliferation of bold theories and conjectures, and their subsequent downfall through vigorous testing and elimination by refutation. His former student, Paul Feyerabend (1975) abandoned the rationalist realist mode of his mentor, along with N.R. Hanson (1958), and, of course, Kuhn, further defining and directing the postmodern agenda in science. One result in all of the disciplines mentioned is that now, instead of just dialogue between two opposing views, like rival theories, we have a new term, "dialogic," which speaks of multitudes of voices, views and perhaps theories of how the world works. The extreme postmodern (relativist) view is that these multitudes are equally compelling.

It may be prophetic that the poet and literary critic, William Everson (now deceased) predicted in 1992 during a celebration I attended of the 100th anniversary of the poet Walt Whitman's death that a new romantic period is coming as we leave the 20th century behind--that the individual will come to matter more. Now it is the politics of the society and its ideologies that prevail and the individual is almost encouraged to feel helpless. Could this explain the malaise in our society regarding feelings of lack of individual responsibility? What effect might these various influences have on science education and our view of the nature of science?

Perspectives for Science Education

There is something of a dilemma back in the science classroom with the postmodern-positivist extremes. The teacher labeled "positivist" <u>might</u> lead students to think of a particular science as a series of many truths, more or less in their final form. On the other hand, they might be just as likely to emphasize the precision with which one should do measurement or the objectivity one must <u>attempt</u> in conducting observations in an experiment, or the logical deductions that can be made from carefully monitored experiments. All of these are part of the "positivist" credo. In other words, it is naive to think of "positivists" as anti-process and postmodernists pro-process--more of the good guy-bad guy stuff.

Where we start moving into deep "postmodern territory" is when the methods, aims, theories, and explanations may be either "up for grabs," and criteria used for judging the best of any of these become suspect to one's ideology. In effect, the extreme postmodernist might say, "How can you say that yours is a better interpretation of the data than mine, or that your methodology yields more "fruit," or that yours are more valid questions than mine? It's all relative."

Good (1993a) makes an important point that extreme postmodern interpretations are not very useful in describing how a science like geology has arrived at its current best explanations for natural phenomena--and why they are the best. These interpretations have limited value in the world of science education where the mission still seems to be to bring the uninformed to understand and value the processes and best explanations of "good" science. Are we in the process of changing that mission? Perhaps *that* is at the heart of the issue.

While this debate about universal versus relativist notions of science and its knowledge goes on, the science education community is aiding and encouraging teachers to have students do more problem solving, deeply contextualized reading, critical thinking, designing and carrying out whole problems, justifying evidence with cogent writing, articulating orally with others, and spending more time on fewer things but delving more deeply and more creatively. It seems that--*despite* greed, politics, tenure, jealousy, and other human dimensions--this is essentially what *good scientists* around the world still *try* to do.

But if we turn around and say that alternative frameworks are okay too--and should be given equal time--such as explanations based on myth, folklore, belief-

-where will the multitudes of frameworks stop? Of course, it is fascinating to learn how the Mono Indians of California lived and worshipped. They lived with the land so exquisitely. But their explanation of how the Sierra Nevada formed is based on observation of little mounds of earth everywhere which are formed by pocket gophers. So the legend of song and story is that big pocket gophers over many years succeeded in lifting the Sierra to where it is today--a wonderful story--but just a story.

How much time can we spend in science class telling stories versus using a strong science *storyline*---a concept in Arons (1989) and promoted in the *California Science Framework*---based on important concepts like the current explanations for the composition of the earth, how plates move, and different ways mountains can form. It takes time to implement activities that engage students to make meaning out of these concepts. It might involve "chocolate chip geology" or "peanut butter and jelly" mountain building. But if the *storyline* is strong and the concepts revisited in numerous ways, we have evidence that meaningful learning can occur. This, I believe is still the goal of science education as we enter the 21st century. It certainly seems to be the goal in Project 2061's *Benchmarks for Scientific Literacy* (AAAS, 1993)) and in the *Draft National Science Education Standards* (NRC, 1994).

It seems to me we cannot dilute the best of what *good* science is to a fuzzier discipline which may limit students to having good feelings, playing a little, and appreciating the interpretations of their neighbor. Is that enough for a good week in science class? Where will that put our students in the international science and technology "pipeline." Philosophical arguments are a necessary part of any discipline, and whatever direction the research agenda of the science education community takes into the 21st Century, it should be based on a well-informed, balanced philosophy of the nature of the scientific enterprise. That includes encouraging teachers to be sensitive to and appreciative of alternative frameworks, belief systems, and world views. As Cobern (1991,1995) shows us, students' alternative frameworks need to be valued and may need to be addressed before some conceptual understanding can take place. There is, however, enough difference between beliefs and the best explanations in science--at least in the many disciplines not still depending on "fringe" theories.

Need for Philosophy of Science in Science Education

Roth (1992) was right when she said "It's not enough to do or relate" in science class. She was referring to the need for students to go beyond hands-on activities to meaningful conceptual understanding. For science educators it suggests developing strong conceptual understandings about what all science might have in common, what the distinct, hard-fought philosophical differences are on various topics related to doing science, and how different disciplines within science, or the people who engage in them, come to their best explanations of how the natural world works.

Many thoughtful works (Martin, 1972; Matthews, 1991, 1994; Duschl, 1990; Duschl & Hamilton, 1992; Aikenhead, 1986) have shed light on the relationship between philosophy of science and science education. [Also see important editions from the International History, Philosophy and Science Teaching Group meetings (Herget, 1989; Hills, 1992)]. It may be advisable, however, to spend a little time reading some key philosophers themselves. Just as we really need to see people doing science, read their primary work, and do a little bit ourselves before appreciating the "humaneness" of the enterprise, so, too for philosophers of science.

The Scientific Theory Profile (Loving, 1991, 1992) might at least get the reader started knowing who to read or how they may relate to each other in terms of being at a more formalist or postmodern position. The "STP" was an attempt to show twelve varied approaches to judging and representing theories. No claims are made that it is complete, but all studied are well known in the field. They are representative of what is out there. I invite readers to add to it and to debate positions.

In the end we are confronted with the question, what and how do we teach in our science classes and what philosophies do we ourselves carry into the classroom? Is it an unplanned philosophy, a naive philosophy born of limited exposure, or one that has been crafted after careful reading and reflection? Just as we are asking science teachers not to teach science in its final form, let us not oversimplify the nature of science--and what it takes philosophers, along with historians and sociologists of science to get where they are on their views of how science is done--or how it should be done.

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