Thomas Kuhn's Impact on Science Education: What Lessons Can Be Learned?

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Received 14 May 2001; revised 15 March 2002; accepted 8 June 2002

ABSTRACT: Thomas Kuhn has had an impact in all academic fields. In science education, Kuhnian themes are especially noticeable in conceptual change research, constructivist theorizing, and multicultural education debates. Unfortunately the influence is frequently compromised by researchers having a limited understanding of Kuhn's original ideas, little exposure to the tradition of philosophical opposition to Kuhn's theories, and minimal appreciation of how Kuhn progressively qualified his initial "irrationalist" views of scientific development. One lesson to be learnt is that the science education community should more seriously and effectively engage with on-going debates and analysis in the history and philosophy of science. This is the same lesson that was learnt from the science education community's wholesale embrace of logical empiricism during the 1950s and 1960s. Another lesson is that there are powerful disciplinary, institutional, and subcultural barriers that mitigate against science educators seriously engaging with historical and philosophical scholarship. *Sci Ed* **88:**90–118, 2004; Published online in Wiley InterScience (www.interscience.wiley.com). DOI 10.1002/sce.10111

Thomas Kuhn has arguably been the most influential historian of science in the twentieth century. His impact has been felt in all academic fields. By the mid-1990s, his landmark *Structure of Scientific Revolutions* (first edition 1962, second edition 1970, hereafter *SSR*) had sold over one million copies in 16 languages. It was the most cited single twentieth century book in the Arts and Humanities Citation Index in the period 1976–1983; even 40 years after its publication, as the twentieth century closed, there were 400 references to the book in the 1999 Social Science Citation Index. Kuhn's epistemology, his account of the

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nature of science, and especially his views on theory change and incommensurability in the history of science, have been exhaustively examined.[1]

Kuhn had a cultural impact. Inevitably, the influence of one million readers, and the hundreds of thousands of students taught by those influenced by Kuhn, would surface in newspaper articles, government reports, popular journalism. As Freud made the term "unconscious" part of everyday speech, Kuhn did the same for "paradigm." The word is as likely to be used in a newspaper editorial, a government report, or a film review, as it is in a sociology paper, a theology text, or a science education article. Kuhn's *SSR* opened with the now famous claim that "history, if viewed as a repository for more than anecdote or chronology, could have produced a decisive transformation in the image of science by which we are now possessed" (Kuhn, 1970, p. 1). He goes on to say that his aim in writing the book is to use the historical record to "sketch a quite different concept of science" (p. 1). He did create a seemingly new image of science, and he succeeded, beyond his own and his publisher's wildest imaginations, in bringing this image to the attention of the educated public.

Not surprisingly Kuhn influenced science education theory and research. It is instructive to see how the science education community responded to Kuhn, because the responses can be used as a further case study of the engagement of science education with the history and philosophy of science. Some lessons have been drawn from the community's embrace of logical empiricism in the 1950s and 1960s[2]; further lessons can be learnt from the community's embrace of Kuhnian ideas in the closing decades of the twentieth century.

KUHN'S PHILOSOPHICAL POSITION: AN INTRODUCTION

Paradoxically, Kuhn the historian of science has had far less impact than Kuhn the philosopher of science.[3] It is noteworthy that his philosophy, not his history, should have the major impact because Kuhn had no training in philosophy, and was a self-described "amateur" in the discipline (Conant & Haugeland, 2000, p. 106). The philosophical impact of *SSR* is out of all proportion to its philosophical content: the book lacks any extended philosophical argument, and makes scant reference to philosophical literature.

[1] Noteworthy among the anthologies devoted to Kuhn's work are *Criticism and the Growth of Knowledge*, which contains the proceedings of a 1965 seminar devoted to the first edition of Kuhn's *SSR* (Lakatos & Musgrave, 1970); *Paradigms and Revolutions*, which contains a number of the major reviews of *SSR* (Gutting, 1980)*; World Changes*, where contributors examine Kuhn's mature views on the nature of science (Horwich, 1993); *Incommensurability and Related Matters*, where issues of realism, incommensurability, and conceptual change are addressed (Hoyningen-Huene & Sankey, 1993); and *Thomas Kuhn*, where contributors range widely over Kuhnian themes (Nickles, 2003). Two systematic elaborations of Kuhn's philosophy of science are Hoyningen-Huene (1993) and Bird (2000).

A long and delightful autobiographical interview, first published in 1997 in the Greek philosohy of science journal *Neusis* (Baltas et al., 1997) and subsequently republished in the posthumous *The Road Since Structure* provides a rich account of Kuhn's personal, educational, and professional history (Conant & Haugeland, 2000, pp. 253–324).

[2] See Matthews (1997) and Duschl (1987).

[3] Kuhn only ever had two or three PhD students (they included John Heilbron, Paul Forman and, as an undergraduate, Zed Buchwald). He recognizes this paucity of students (Conant & Hugeland, 2000, p. 303), and says, even of them, that they do not "push along" his own program for the history of science. In a review of Kuhn's impact on the practice of history of science, Stephen Brush (who did a Harvard course with Kuhn in 1954) writes "I have been disappointed by the hostility and indifference to Kuhn's work displayed by many historians of science" (Brush, 2000, p. 39). Brush also lists 27 recent (1990–1997) physics texts and concludes that Kuhn had little, or no, impact on their contents.

Wittgenstein, Braithwaite, Polanyi, Whewell, Popper, Goodman and Hanson are the only philosophers cited in the first edition of *SSR*[4]; and these, with the notable exception of Hanson, are mentioned just in passing. There is no prolonged analysis of any philosophical argument, excepting a brief analysis of arguments about perception and what contributions the observer makes to the object as perceived.

Kuhn did read philosophy in his post-*SSR* phase. He saw the whole scholarly endeavor of his last 10–20 years as a philosophical one, and addressed audiences as a philosopher, but even here a recent sympathetic appraisal concluded that (Bird, 2000, p. ix)

> Kuhn's treatment of philosophical ideas is neither systematic nor rigorous. He rarely engaged in the stock-in-trade of modern philosophers, the careful and precise analysis of the details of other philosopher's views, and when he did so the results were not encouraging.

Kuhn admitted in 1997 that his *SSR* treatment of the orthodox philosophical tradition was "irresponsible" (Conant & Haugeland, 2000, p. 305): a judgment that his critics would endorse (Caneva, 2000; Friedman, 2002).

It is worth drawing attention to this peculiarity at the outset as, overwhelmingly, it is Kuhn's (real or imagined) philosophy that had the biggest impact on science education. Amateurs can stumble on to truths, even deep ones, but a certain caution is sensible when entertaining their views. This caution was rarely exercised by members of the science education community who were moved by Kuhn's philosophical pronouncements; in the words of one review, the community became an "admiration society for Thomas Kuhn" (Loving & Cobern, 2000, p. 199).

Protagoras Revived: The Restatement of Relativism

Kuhn was notoriously cavalier (in his words "irresponsible") and imprecise in stating his philosophical position. On many occasions he had to go out of his way to distance himself from "Kuhnians," and to separate himself from prevalent interpretations of his position.[5] Thus one needs to refer to Kuhn's "real or imagined" philosophy. But there is agreement that, whatever else he did, Kuhn articulated a seemingly new—if one can be allowed to use the term—epistemological paradigm, or theory of scientific knowledge. More accurately he gave a modern, scientifically informed, philosophical legitimacy to much older relativist and skeptical traditions in epistemology.

The ancient sophist Protagoras, in the fourth century BC, famously (or infamously for Plato and the realist and rationalist traditions in philosophy), said that "man is the measure of all things, of things that are that they are, and of things that are not that they are not"; and consequently that "whatever seems just to a city is just for that city so long as it seems so".[6] Many readers of Kuhn resurrected the Protagorean thesis, and simply substituted "paradigm" for "man": paradigms became the measure of all things. There was no "Grand Narrative" in science, just a number of local stories.

According to modern Protagoreans, Kuhn showed that judgments of truth in science were intratheoretic, and that no rational decision could be made between competing theories; they

[4] In the Postscript to the second edition (1970), written after the 1965 London conference convened on his work, he does mention a good many other contemporary philosophers of science, and some of this early critics.

[5] That he could write so ambiguously is itself indicative of his lack of philosophical training. A concern with clarity and avoidance of ambiguity is one of the, hoped for, first fruits of philosophical education.

[6] The arguments of Protagoras, the Sophist of Abdera in Thrace, have entered Western philosophy principally through Plato's influential epistemological dialogue that bears his name. Guthrie's is the standard English translation (Guthrie, 1956).

believed that Kuhn established that different scientific theories were incommensurable and that scientists working in different paradigms saw different things (an ontological claim), not merely saw the same thing differently (an epistemological or semantic claim). If this was so for science, the very model of a rational enterprise, it was then easy to assert that no rational decision can be made between competing accounts in politics, religion, art, social science, ethics, or education. Indeed, Kuhn frankly admits that he merely applied to the history of science, the commonplace methodological and conceptual terms found in good histories of art, literature, music, and politics (Kuhn, 1970, p. 208). After Kuhn, many more people felt comfortable in saying "what's true for you, need not be true for me." He appeared to give intellectual underpinning to individual and cultural relativism.

Protagoras Refined: The Abandonment of Truth

It is not that Kuhn merely offered a different account of truth, or allowed different and contradictory truths to coexist, he basically undermined the traditional idea of truth, especially in science. He famously said in the 1970 Postscript to *SSR* that truth was irrelevant to judgments of scientific progress: "Does it really help to imagine that there is some one full, objective, true account of nature and that the proper measure of scientific achievement is the extent to which it brings us closer to that ultimate goal?" (Kuhn, 1970, p. 171). Other things might have been happening as physics moved from Aristotelianism to Newtonianism to Relativity Theory, and as biology moved from Special Creation to Lamarckianism to Darwinism, but becoming more truthful, or better approaching the truth, was not, for Kuhn, one of them. He goes on to say that the idea of a match between the theoretical posits of increasingly better theories and the ontology of the world is "illusive in principle" (p. 206). There is, for Kuhn, "no theory-independent way to reconstruct phrases like 'really there' " (p. 206).

His 1970 Postscript makes explicit the antirealism of *SSR*. Kuhn simply rejects, and continued to reject, realism in philosophy of science. He denied that the theoretical terms of *any* scientific theory successfully refer to objects in the world; not just that contingently they have so far been unsuccessful or false, but rather in principle they cannot so refer. The world in itself is unknowable. As Ernan McMullin recognized "The radical challenge of *SSR* is directed not at rationality but at realism" (McMullin, 1993, p. 71).

Many sociologists of science well express the new scholarly view of science; some maintaining that after Kuhn (Brante, Fuller, & Lynch, 1993, p. ix)[7]

> . . . philosophical words such as *truth, rationality, objectivity*, and even *method* are increasingly placed in scare quotes when referring to science.

THE EARLY NEGLECT OF KUHN BY SCIENCE EDUCATORS

The first edition of *SSR* appeared in 1962 as a number (Volume 2, Number 2) in the rarely seen *International Encyclopedia of Unified Science* edited by the logical empiricist Otto Neurath. To all intents and purposes its argument lay dormant[8] until the enlarged second

[7] Kuhn would be content with this judgment of Brante and colleagues, except that he did try to rescue a reinterpreted form of rationality for science, and complained that critics who accuse him of denying the rationality of theory change in science have grossly misread him (Kuhn, 1970a, p. 199).

[8] For example, in 1967 a collection was published titled *Science and Contemporary Society* (Crosson, 1967). Among other papers were one by the Chicago philosopher Richard McKeon titled "Scientific and Philosophic Revolutions," and one by the Minnesota philosopher Herbert Feigl titled "Contemporary Science and Philosophy." In neither paper is Kuhn mentioned. The single most important acknowledgment of

edition was, in a stroke of marketing genius, published as a separate book by The University of Chicago Press in 1970. Then the Kuhnian wave broke over philosophy departments, and in quick succession other humanities and social science departments. However, the Kuhnian impact on science education was delayed. For instance, the very first science education book to deal with the place of philosophy of science in science teaching was published in 1968. This was John Robinson's *The Nature of Science and Science Teaching* (Robinson, 1968). Kuhn is nowhere mentioned in its 150 pages. Robinson's book was entirely predicated upon the, then dominant, logical empiricist analysis of science, not even Popper, whose antipositivist *The Logic of Scientific Discovery* appeared in 1959, is mentioned (Matthews, 1997).[9]

In 1968 there was an important panel discussion on "Philosophy of Science and Science Teaching" at the annual US National Association for Research in Science Teaching conference. Contributors included John Robinson, Michael Connelly, and Marshall Herron. The papers were published the following year in Volume Six of *the Journal of Research in Science Teaching* (Connelly, 1969; Herron, 1969; Robinson, 1969). Again Kuhn is not mentioned.

Nor did Kuhn inform the widespread post-Sputnik curriculum debates of the 1960s. In the United States, twenty-eight curricular projects were being supported by the National Science Foundation in 1975. During this period the "alphabet" curricula were conceived, born, and raised: PSSC, CBA, BSCS, CHEMS, IPS, ESCP, and so on. In the United Kingdom, various Nuffield schemes were launched at the same time. It has been often, commented upon that these forward leaps were, philosophically speaking, more backward than forward. The science education community was not engaging with, nor learning from, developments in the history and philosophy of science. Michael Connelly commented, in 1974, of the post-Sputnik curricular boom, that

> while this activity began with philosophical concerns for knowledge and for enquiry, it was largely dominated by the works of a few psychologists, notably, Bruner, Ausubel, Gagne, Piaget. (Abimbola, 1983, p. 182)

A few rare commentators in the 1960s, who were familiar with both the philosophical and the educational literature, noted this neglect of "new" philosophy of science by science educators. Yehuda Elkana, a philosopher of science, observed that science education from the 1950s to Sputnik was formed in the image of "inductivist-realist" philosophy of science (Elkana, 1970, p. 3). He said of post-Sputnik PSSC and BSCS curricula and teaching material that they "reflect the positivistic-Instrumentalist philosophy of science [logical empiricism], which was at the height of its influence in the early days of space travel" (p. 8).

Elkana noted, and lamented, that Kuhn's *SSR* and Joseph Schwab's "The Teaching of Science as Inquiry" (Schwab, 1960) were published in the same period yet share no common literature. They were "two very important books, both highly influential in their own fields, both relying on two traditions and two bibliographies which completely ignore each other" (Elkana, 1970, p. 15). Elkana sketched out the "practical implications for the teaching of science" that Kuhn's new philosophy of science generated.

the first edition of Kuhn's *SSR* was the 1965 conference on his work held in London, the Proceedings of which were subsequently published in 1970 (Lakatos & Musgrave, 1970), the year that the second edition of *SSR* appeared.

[9] It should be remembered that Robinson's book was the delayed publication of a PhD thesis that was completed some years prior to 1969. Nevertheless, it contains no hint that any part of the Logical Empiricist picture of science had been challenged by historians or philosophers.

In 1972 another philosopher, Michael Martin, surveyed the same material as Elkana, paying particular attention to the rush of "inquiry" and "discovery" curricula and recommendations put into Western educational orbit by Sputnik. He drew attention to the important 1966 Educational Policies Commission document, *Education and the Spirit of Science*, and charted the myriad ways in which it, and other curricula as well, reproduced simplistic inductivist understanding of scientific inquiry (Martin, 1972, pp. 141–147).

> For instance, the *Spirit* document of 1966 asserted that:
>
> data and generalization are the forms which [scientific] knowledge takes. Generalisations are induced from discrete bits of information gathered through observation conducted as accurately as the circumstances permit. (Martin, 1972, p. 142)

This homely piece of inductivism had the *imprimatur* of the highest office in the US educational landscape. It was published 8 years after Norwood Russell Hanson's *Patterns of Discovery* (Hanson, 1958), which received wide philosophical attention for its "theory dependence of observation" thesis, 7 years after Popper's anti-inductivist work *The Logic of Scientific Discovery* (Popper, 1959) was translated into English and also given wide philosophical attention, and 4 years after the publication of Feyerabend's essay "Explanation, Reduction, and Empiricism" that shook the foundations of inductivist accounts of science (Feyerabend, 1962).

Elkana and Martin were correct in saying that in the 1950s and 1960s, the communities of history and philosophy of science and of science education basically ignored each other. Kuhn, in 1962, had published the first edition of *SSR*, others were also shaking the logical empiricist tree, but little, if any, of the philosophical debate impacted on the science education community. The unfortunate divide of the time was well documented in a landmark study by Richard Duschl titled "Science Education and Philosophy of Science: Twenty-Five Years of Mutually Exclusive Development" (Duschl, 1985).[10]

CATCHING UP: THE BELATED RECOGNITION OF KUHN BY SCIENCE EDUCATORS

After a late start, Kuhn's impact on education, and specifically science education, research was considerable. Kuhnian notions of "paradigm," "incommensurability," and "theory dependence" became the stock-in-trade of most educational researchers. For the most part, science educators interpreted these terms in the prevalent relativistic and antirealist manner; they did not attempt to give a fallibilist and realist rendering of the terms. Perhaps there is no better example than the influential work of Yvonna Lincoln and Egon Guba. In their major publication, *Naturalistic Inquiry* (Lincoln & Guba, 1985), they draw on the work of Hesse, Heron, Patton, and a few other writers inspired by Kuhn, to claim that "paradigms represent a distillation of what we *think* about the world (but cannot prove)" (p. 15), "Since all theories and other leading ideas of scientific history have, so far, been shown to be false and unacceptable, so surely will any theories that we expound today" (p. 16), "people are not so much *compelled* by the logic of a situation as they are *persuaded* to accept a new set of values . . . the value shift is crucial; without it, rational movement cannot occur," and, finally (pp. 83–84),

> There is, in this ontological position, always an infinite number of constructions that might be made and hence there are multiple realities. Any given construction may not be (and

[10] Some of this history of separate development is discussed in Matthews (1994, ch. 2).

> almost certainly is not) in a one-to-one relation to (or isomorphic with) other constructions of the same (by definition only) entity.

In this pronouncement, Kuhnian-inspired notions of paradigms and incommensurability are rolled together to produce unadulterated ontological idealism: our theoretical terms (constructions) create their corresponding realities! Phlogiston apparently came into the world when Georg Stahl coined the term in 1718, and it began disappearing from the world sixty years later as Antoine Lavoisier's oxygen construction gained currency. Neptune was added to our solar system only in 1846—just what had been causing the deviations of Uranus from its orbit before that date is, on the Lincoln and Guba account, a mystery, as without the concept of Neptune there was no reality. Their view echoes the biblical claim that "In the beginning was the Word and the Word made the world." Would that reality, especially educational reality, was so obedient to words, speeches and theorizing. In later work Guba and Lincoln cite Kuhn, and attribute their embrace of constructivism to his influence (Guba & Lincoln, 1989, p. 84).

Although science education missed the first, post-1962, Kuhnian wave, the community did not miss the second, post-1970, wave. In 1985, Derek Hodson published a review of research on "Philosophy of Science, Science and Science Education" in which he determined that of 22 articles published, and theses submitted, in the period 1974–1984, 14 addressed Kuhnian themes (Hodson, 1985). In 2000, Cathleen Loving and William Cobern conducted a citation analysis of two major science education journals, *Science Education* and *Journal of Research in Science Teaching*, for the 13 year period 1985–1998 and, not surprisingly, found that there were numerous citations of Kuhn covering such Kuhnian themes as paradigms (30 articles), conceptual change theory, constructivist epistemology, incommensurablity, authenticity of textbooks, the social components of science, and also the philosophical comparison of Kuhn and other methodologists of science (Loving & Cobern, 2000).

Kuhn and Conceptual Change Research

Research on conceptual change in children is one field where Kuhn has had extraordinary influence. This is understandable as Kuhn, alone among major philosophers of science, explicitly dealt with pedagogical matters and with problems in learning science. Indeed his whole new view of the history of science, of scientific change, led him to consider the processes of conceptual change in scientists; his account of macrochange in science led him to comment on the processes of micro change in individual scientists, in other words, conceptual change or the learning of science.

Kuhn's first substantial discussion of the cognitive mechanisms and pedagogical conditions for the learning of scientific concepts and theories was contained in his 1959 address "The Essential Tension" (Kuhn, 1959). He maintained this interest in the theory and practice of learning to the end of his career, as indicated by a 1990 paper devoted to "The Learning of Physics" and republished in 2000 (Kuhn, 2000). Given that learning, cognitive apprenticeships, and transmission of basic concepts and methodologies were important components of the establishment of a paradigm, it is not surprising that Kuhn was engaged by such questions.[11] Nor is it surprising that these features of Kuhn's corpus made it attractive and accessible to science educators. Psychology, learning, cognition, perception all provided a natural bridge between the research of science educators and the champion of the "new philosophy of science."

[11] For a wide-ranging discussion, with numerous references to research literature in cognitive science, see Nersessian (2003).

This psychological bridge into Kuhn's work is well displayed in one of the first science education articles to seriously engage with Kuhn's theory, namely Ted Cawthron and Jack Rowell's 1978 article "Epistemology and Science Education" (Cawthron & Rowell, 1978). They drew parallels between Piaget's theory of knowledge and his psychological account of the constructive knowing subject, and what they found in Kuhn. For them, Kuhn established that (p. 45)

> we see things not just as they are but also partly as we are, and this is not due simply to differences in interpretation of otherwise stable facts or data. The "objective" real world becomes merged with its "subjective" interpretation and the Cartesian Dichotomy is replaced by a dialectic epistemology with distinctly relativistic implications

One of the most influential articles in recent conceptual change research is by George Posner and colleagues "Accommodation of a Scientific Conception: Toward a Theory of Conceptual Change". It is explicitly based on Kuhn's analysis of paradigm change in science (Posner et al., 1982). One of the authors of that study noted this dependence, and itemized how Kuhn's analysis was transferred to the study of individual conceptual change (Hewson, 1981, p. 387). The authors proposed that, for individual conceptual change or learning to take place, four conditions had to be met:

1. There must be dissatisfaction with current conceptions.
2. The proposed replacement conception must be intelligible.
3. The new conception must be initially plausible.
4. The new conception must offer solutions to old problems and to novel ones; it must suggest the possibility of a fruitful research program.

Strike and Posner, in retrospect, describe their original conceptual change theory as "largely an epistemological theory, not a psychological theory . . . it is rooted in a conception of the kinds of things that count as *good* reasons" (Strike & Posner, 1992, p. 150). They say that their original theory is concerned with the "formation of rational belief" (p. 152); it does not "describe the typical workings of student minds or any laws of learning" (p. 155).

Strike and Posner recognize clearly that ascribing reasons for some belief is a purely descriptive or psychological claim; identifying which reasons are "good" or "rational" is no longer psychology but philosophy; it is no longer descriptive but normative. This qualification is important, and it will feature in the following discussion about Kuhn's equivocation between descriptive and normative aspects of learning, an equivocation that is also rampant in theories of learning, and in research on children's conceptual change. In educational research, the move between children's *beliefs* and children's *knowledge* routinely occurs without comment; indeed "belief" and "knowledge" are regarded as synonyms.[12]

Strike and Posner are right to stress this important distinction between psychological and normative dimensions of conceptual change, but it opens the question of how, on Kuhnian grounds, rational beliefs and reasonable belief change can be identified? This is a crucial area for philosophical analysis. If their original paper depends on Kuhn's analysis of scientific change, and if it is furthermore meant to be an account of an individual's *rational* conceptual change, then it would seem to require that Kuhn's analysis be of rational change in science.

[12] For discussion of some of the issues involved in the belief/knowledge distinction, see Southerland, Sinatra, and Matthews (2001).

This can be attempted—as will be seen in a subsequent section—but it does fly in the face of the common "irrationalist" reading of Kuhn's theory of scientific change.

Kuhn's Recapitulation Thesis

Kuhn popularized Piaget's "cognitive ontogeny recapitulates scientific phylogeny" thesis among historians and philosophers of science, saying: "Part of what I know about how to ask questions of dead scientists has been learned by examining Piaget's interrogations of living children" (Kuhn, 1977, p. 21). In *SSR*, Kuhn remarked on how accidental was his discovery of Piaget, saying that "a footnote encounted by chance led me to the experiments by which Jean Piaget has illuminated both the various worlds of the growing child and the process of transition from one to the next" (Kuhn, 1970, p. vi). It is easy to accept that Piaget's view that the conceptual development of children was stage-like, and that it exhibited discontinuities, played a central role in Kuhn's characterization of scientific development.

The recapitulation thesis underlies Piaget's Genetic Epistemology program (Piaget, 1970; Piaget & Garcia, 1989), as Piaget says: "The fundamental hypothesis of genetic epistemology is that there is a parallelism between progress made in the logical and rational organization of knowledge and the corresponding psychological processes" (Piaget, 1970, p. 13). Conversely, the historian of science, Alexander Koyré, observed that it was Aristotle's physics that taught him to understand Piaget's children. The philosopher Philip Kitcher has affirmed that developmental psychologists can gain insights into the linguistic advances of young children by studying the shifts that have occurred in the history of science; and that historians and philosophers of science can learn from the experimental results and analyses of the child psychologists (Kitcher, 1988).

Cawthron and Rowell's above mentioned 1978 article is one of the first in science education to link Kuhn's ideas on the noncumulative, discontinuous growth of scientific knowledge with Piaget's views of the staged development of individual cognition. In their words (Cawthorn & Rowell, 1978, p. 46)

> This view of man [Kuhn's] as an active agent, constructing his own reality, is also at the core of Piaget's developmental psychology or "genetic epistemology" . . . As man constructs his reality he must also experience it. Thus both the world view and the developmental level of an individual are determined by a dialectic process whereby a dynamic equilibrium is maintained between responses to environmental stimuli (accommodation) and changes in the intruding stimuli due to the existing cognitive internal structure of the human agent. As in the Kuhnian scheme, the equilibrium is not static and new cognitive structures—or systems of expectations, as they may be interpreted—evolve through the dialectic process (known as equilibration in Piagetian terminology).

Interestingly, they observe that "it is not surprizing that some researchers have, in fact, started to use the term 'paradigm shift' to indicate the transition from one Piagetian developmental stage to the next" (Cawthron & Rowell, 1978, p. 47). Indeed the title of a deservedly famous article from the late 1970s is "Pupils and Paradigms: A Review of Literature Related to Concepts Development in Adolescent Science Students" (Driver & Easeley, 1978).[13]

[13] Harvey Siegel wrote on this topic (Siegel, 1982). Nussbaum (1983) provided an early review of the relevant science education literature dealing with individual learning and the history of science. Subsequent contributors to this area of research have included Gauld (1991), Nersessian (1989), Carey (1988, 1992), Karmiloff-Smith (1988), Gopnik (1988, 1996), Thagard (1992, pp. 250–263), and Levine (2000).

Kuhn and Constructivism

For 20 years, constructivism, in one form or another, has dominated theoretical debate in science education.[14] Most leading constructivists acknowledge Kuhn as the fount of their relativist epistemology and their constructivist, antirealist, view of science. Derek Hodson wrote that "it has been argued earlier that Kuhnian models of science and scientific practice have a direct equivalent in psychology in the constructivist theories of learning. There is, therefore, a strong case for constructing curriculum along Kuhnian lines" (Hodson, 1988, p. 32). The opening sentences of a much-cited paper by Ernst von Glasersfeld said that Kuhn's *SSR* "brought to the awareness of a wider public" the professional crisis "of faith in objective scientific knowledge" (Glasersfeld, 1989, p. 121). David Hawkins, in an article on the history of constructivism, wrote that *SSR* "provided 'constructivist' justification" for "philosophies of relativism and subjectivism" (Hawkins, 1994, p. 10). Joseph Novak acknowledged Kuhn as instrumental in the development of his own constructivist epistemology that underscores the children's alternative conceptions research program (Novak, 1998, p. 6). Nancy Davis and colleagues used "Thomas Kuhn's (1970) work as a basis to support change in guiding epistemological paradigms" whereby they endorse constructivism and reject objectivism (Davis et al., 1993, p. 627). The first sentence of Kenneth Tobin's anthology *The Practice of Constructivism in Science Education* rings with a Kuhnian allusion: "Currently there is a paradigm war raging in education" (Tobin, 1993, p. ix); and at least one contributor to the anthology listed Kuhn's *SSR* as "one of the main constructivist sources of influence on my thinking" (Taylor, 1993, p. 268).

Cathleen Loving and William Cobern's review of the influence of Kuhn on science education research noted that "there is not a single critical voice; the science education community has turned into a 'admiration society for Thomas Kuhn" (Loving & Cobern, 2000, p. 199).[15] This suggests a real danger of the community repeating the mistake of the 1960s when it simply embraced, wholesale, the then dominant logical empiricist account of science. This in turn raises questions about how the community can avoid these kinds of mistakes. Both considerations will be taken up after outlining some of the problems with Kuhn's philosophy, problems that ought to have given pause to the science educator's embrace.

HOW NOVEL WAS KUHN'S PHILOSOPHICAL POSITION?

Kuhn was a key figure in the demise of the long dominant logical empiricist program in philosophy of science. The program was initiated by Ernst Mach in the late nineteenth century, and contributed to by such influential philosophers as Morris Schlick, Otto Neurath, Rudolf Carnap, Carl Hempel, Herbert Feigl, Fredrick Ayer, Hans Reichenbach, Ernst Nagel, and countless less famous others.[16] Largely through Kuhn's efforts, philosophy of science took an historical turn in the 1970s—it was simply no longer acceptable for philosophers of science to discuss issues of methodology, explanation, values, theory structure, and so on, without reference to how these matters are manifest in the history of science. Rudolf Carnap might have proudly said of himself that he was "as an unhistorically minded a person as one could imagine" (Suppe, 1977, p. 310) but, after Kuhn's impact on the field, such confessions

[14] For an account of the influence of constructivism in science education, see Matthews (2000a); for wider views of the matter, see contributions to Phillips (2000).

[15] One minority voice was that of Harvey Siegel. Although he published in science education, he was not part of the science education community. His articles of 1979 and 1985 are critical of Kuhn's relativist epistemology, and of educational implications derived from it (Siegel, 1979, 1985).

[16] The standard survey of this logical empiricist tradition, the "Received View" in mid-twentieth century philosophy of science, is that of Suppe (1977). See also the brief summary by Ian Hacking in the "Introduction" to his *Scientific Revolutions* Hacking (1981).

were a rarity. A marriage, if somewhat uneasy, was enacted between philosophy and history of science.[17]

As with most revolutions, Kuhn's was not *ex nihilo*. His central ideas were by no means novel. The intellectual ground for the Kuhnian revolt was prepared by a variety of "prophets." What Kuhn observed of transformations in science applies to his own transformation in philosophy of science: "new theories . . . in the mature sciences are not born *de novo*. On the contrary, they emerge from old theories and within a matrix of old beliefs" (Kuhn, 1977, p. 234).

Science educators should not have been entirely surprised by Kuhn's putative revelations about science. All the elements were there before Kuhn so publicly announced them. And, as frequently happens in revolutions, Kuhn unconsciously absorbed a great deal of what he was reacting against. Political revolutions frequently change personnel, but maintain structures. Likewise with intellectual revolutions: Recall Marx's acerbic comment about opponents of Hegel who merely turned him on his head. So to with Kuhn: He assumed a great deal of the empiricist problematic against which he was reacting.[18]

The empiricist understanding of science had been challenged on many fronts. In the 1930s, Ludwik Fleck wrote on the social construction of facts, on the necessity of an historical component for understanding, and on the dominance of the "thought collective" or *Denkkollectiv* for the gestalt perceptions of individual scientists (Fleck, 1935/1979). At the same time Gaston Bachelard wrote on epistemological ruptures in the history of science, and on the impact of epistemological obstacles on cognition (Bachelard, 1934/1984, 1940/1968). In the 1940s, R.G. Collingwood elaborated how particular periods in the history of science had different metaphysical presuppositions which were fundamental assumptions about the constituents of the world and their properties that were not given directly in experience (Collingwood, 1940, 1945). In the early 1950s, Stephen Toulmin wrote on how discoveries in the physical sciences consisted, in part, of finding fresh ways of looking at phenomena, and advocated the importance of history for the philosophy of science (Toulmin, 1953). At the same time Willard van Orman Quine published his famous "Two Dogmas of Empiricism" essay (Quine, 1953) which seemingly demolished the core empiricist distinctions between synthetic and analytic truths, and the between theoretical and observational terms. As Quine remarked, "One effect of abandoning them is .... a blurring of the supposed boundary between speculative metaphysics and natural science" (Quine, 1951, p. 20). In 1952, Michael Oakeshott wrote pointedly on how scientific method is an abridgement of scientific activity, and that crucial to the latter is a certain nonreductive connoisseurship for knowing when to apply methodological prescriptions (Oakeshott, 1962). Michael Polanyi developed these same themes in the late 1950s, when he wrote on the place of tacit knowledge in science, the corrective function of the scientific community, and the importance of initiation into accepted methodologies and practices for the conduct of science (Polanyi, 1958). A few years later, Norwood Russell Hanson wrote on the theory dependence of observation and on the contested nature of the facts in scientific disputes (Hanson, 1958). And in the same year as the first edition of Kuhn's *Structure*, Paul Feyerabend published a long essay, "Explanation, Reduction, and Empiricism" that challenged the empiricist assumption of meaning invariance during scientific theory change, and suggested the notion of incommensurability between competing theories (Feyerabend, 1962).

Thus many elements of Kuhn's philosophy of science were extant when he published his landmark *SSR*, and science educators of the 1950s and 1960s could easily have availed

[17] Kuhn addresses the relationship between history and philosophy of science in the lead essay of his *The Essential Tension* (Kuhn, 1977). Useful discussions of the marriage of history and philosophy of science can be found in Hacking (1992), Lakatos (1971), McMullin (1970, 1975), Shapere (1984), and Wartofsky (1976).

[18] For a penetrating discussion of this matter, see Bird (2000).

themselves of these minority views and arguments. However, Kuhn's book brought these elements together and assembled them in a novel way: the bricks might have been lying about the scholarly terrain, but not the building. Kuhn put the bricks together in such a way that the edifice attracted immediate and widespread comment: one did not have to be a philosopher to see the edifice, or to read about it. Each of the individual ideas, the bricks of the new philosophy, were engaging and illuminating. Kuhn did an enormous service to scholarship to give them prominence. What his training, or more correctly lack of it, prevented him from doing was to scrutinize the bricks, to recognize from what intellectual traditions they were constituted, to appraise the structural coherence of the new edifice, and to see whether he had, in fact, been building according to a very old plan.

THE "PURPLE PASSAGES": KUHN'S EARLY IRRATIONALISM

With good reason, the first edition of *SSR* set off alarm bells in the history and philosophy of science community. This was because Kuhn made both truth and rationality an *intra*-paradigm matter. The book was filled with comments such as: "What occurred [when paradigms changed] was neither a decline nor a raising of standards, but simply a change demanded by the adoption of a new paradigm" (Kuhn, 1962, p. 107); transition to a new paradigm occurs "not by deliberation and interpretation, but by a relatively sudden and unstructured event like a gestalt switch" (p. 150); "the competition between paradigms is not the sort of battle that can be resolved by proofs . . . in these matters neither proof nor error is at issue" (pp. 147, 150); consequently paradigm change is a matter of "techniques of persuasion, or about argument and counterargument in a situation in which there can be no proof" (p. 152); "the man who embraces a new paradigm at an early stage must often do so in defiance of the evidence provided by problem solving . . . A decision of that kind can only be made on faith" (p. 158); and, "we may have to relinquish the notion, explicit or implicit, that changes of paradigm carry scientists and those who learn from them closer and closer to the truth" (p. 169).

There were critics of Kuhn's "new" philosophy of science. David Stove, for instance, wrote that "his entire philosophy of science is actually an engine for the mass-destruction of all logical expressions . . . [he] is willing to dissolve even the strongest logical expressions into sociology about what scientists *regard as* decisive arguments (Stove, 1982, p. 33). Israel Scheffler, in *Science and Subjectivity*, argued that Kuhn's charge of irrationality in paradigm choice "fails utterly, for it rests on a confusion. It fails to make the critical distinction between those standards or criteria which are internal to a paradigm, and those by which the paradigm is itself judged" (Scheffler, 1966, p. 84). Dudley Shapere, concluded a review of the second edition of SSR with the words "I have tried to show that the arguments by which Kuhn arrives at this conclusion are unclear and unsatisfactory" (Shapere, 1971, p. 709).

Other prominent early critics were Karl Popper, who accused Kuhn of promoting the relativist and clearly mistaken "myth of the framework" (Popper, 1970, p. 56); Imre Lakatos, who accused Kuhn of denying rational explanations of crises in science, and thus substituting "mob psychology" for scientific appraisal (Lakatos, 1970, p. 178); Paul Feyerabend, who thought that Kuhn was intentionally opaque on the crucial question of whether he was offering a *descriptive* or *prescriptive* account of science, and who said that "normal science is not even an historical fact" let alone anything with normative import (Feyerabend, 1970, pp. 198, 207); John Watkins, who claimed that Kuhn misunderstood normal science and failed to recognize the centrality it gives to critical discourse (Watkins, 1970, pp. 36–37); and Stephen Toulmin, who thought that the very distinction between normal and revolutionary science could not, on Kuhn's terms, be made (Toulmin, 1970, pp. 39–41).

But the critics were swamped in the general enthusiasm, certainly outside of philosophy departments—recall that after *SSR* was written, Kuhn was denied tenure in the Berkeley philosophy department[19]—for Kuhn's new account of science and its history. For the most part the critics were voices in the academic wilderness; in Kuhnian terms, they were practitioners hanging on to an old paradigm whose conceptual apparatus rendered them incapable of seeing the realities that the new paradigm was unearthing. The arguments of these early critics certainly did not impact on the science educators who launched themselves on the second wave of Kuhnian enthusiasm.

Very little of the relativist patina changed in the second edition of *SSR* where he insists that changes of paradigm are "conversion experiences" that can "only be made on faith" (Kuhn, 1970, pp. 151, 158). Minimally on this view, evidence, reasons, and rational theory choice might, arguably, be part of normal science, but they can play little role in major theory changes, and consequently in the growth of scientific knowledge. Consequently, psychology, and then sociology, were invoked to explain the development of science.

Four centuries ago, Galileo warned against this option. Against Kuhn-like relativists of his day, Galileo maintained in his famous *Dialogue* that (Galileo, 1633/1953, p. 53)

> if what we are discussing were a point of law or of the humanities, in which neither true nor false exists, one might trust in subtlety of mind and readiness of tongue and in the greater experience of the writers, and expect him who excelled in those things to make his reasoning more plausible, and one might judge it to be the best. But in natural sciences whose conclusions are true and necessary and have nothing to do with human will, one must take care not to place oneself in the defense of error; for here a thousand Demostheneses and a thousand Aristotles would be left in the lurch by every mediocre wit who happened to hit upon the truth for himself.

Many contemporary readers clearly believe that Kuhn dissolved Galileo's distinction between science and nonscience, and that "subtlety of mind and readiness of tongue", along with reputation and power, are decisive in scientific theory choice. For example, the Kuhn-influenced sociologists of scientific knowledge, Bruno Latour and Stephen Woolgar, wrote in their enormously influential *Laboratory Life* that (Latour & Woolgar, 1986, p. 184)

> . . . science is a form of fiction or discourse like any other, one effect of which is the "truth effect," which (like all literary effects) arises from textual characteristics.

and that " 'out-there-ness' [the external world] is the consequence of scientific work rather than it cause" (p. 182).

Some quickly seized on Kuhn's argument to say that there was no "God's Eye" view or, for those more agnostically inclined, there was no "view from no where"; others announced that "all knowledge is local knowledge." Protagorean relativism was well and truly resuscitated after seemingly being given a knockout blow by Galileo, Newton, and other champions of the Scientific Revolution, and their philosophical interpreters. Absolutism was in full academic retreat. Fallibilism, unfortunately, suffered collateral damage; few people stopped to inquire about how fallibilism differed from absolutism, both were swept away.

TAKING STOCK: KUHN'S INITIAL RETREAT

Kuhn, at least, did attend to his critics, and consequently began to back away from the more extreme of his early claims concerning canons of rationality being entirely

[19] This episode is nicely discussed in Kuhn's autobiographical interview (Kuhn, 2000, pp.300–302).

intraparadigmatic, the essential irrationality of theory choice, the theoretical dependence and subjectivity of observation, and the incommensurability of theories. Paradigms were not such all-enveloping and defining constraints on observation, methodology, and theory choice as they first appeared to be in 1962.

The beginning of Kuhn's retreat is found in the 1970 Postscript to the second edition of *SSR*, and in his essays "Reflections on My Critics" and "Logic of Discovery or Psychology of Research" also published in 1970 in the Lakatos and Musgrave collection (Lakatos & Musgrave, 1970, pp. 231–278 and pp. 1–24 respectively). In these works Kuhn acknowledged the gravity of the complaints of relativism leveled against him. In response he either gave up a lot of the ground he had earlier marked out, including abandoning the idea of paradigm in favor of "disciplinary matrix" which is constituted by a mixture of exemplars, laws, models, and values; or he offered the "I did not say that" defense. In the Postscript he says that there are still good reasons for theory choice, and that these are the old familiar ones "usually listed by philosophers of science: accuracy, simplicity, fruitfulness, and the like" (Kuhn, 1970, p. 199). Where he now departs from the orthodox tradition is in maintaining that such reasons or criteria "function as values and that they can thus be differently applied, individually and collectively, by men who concur in honoring them" (Kuhn, 1970, p. 199). People might, for instance, share the value of "doing good," but be led to very different conclusions about what to do in concrete cases: euthanasia and abortion are striking examples.

Kuhn resorted to different "puzzle solving" capacities to differentiate, compare, and rank theories and paradigms (Kuhn, 1970, pp. 205–206). He says that "this is not a relativist's position, and it displays the sense in which I am a convinced believer in scientific progress" (p. 206). But "The conversion experience that I have likened to a gestalt switch remains, therefore, at the heart of the revolutionary process" (p. 204).

Kuhn took stock of his early retreat in his 1973 Machette Lecture, "Objectivity, Value judgment, and Theory Choice"[20] where he claims that most of his critics "manifest total misunderstanding" of his position (Kuhn, 1977, p. 321).[21] He says that he had always held that good scientific theories were characterized by at least five features: accuracy, consistency (internally, with other theories, and externally, with scientific metaphysics and worldviews), scope, simplicity, and finally, fruitfulness (suggesting new phenomena and relationships). Kuhn says that these are the same criteria that the orthodox philosophy of science tradition uses to judge good theories. What he does insist upon is that their application to any particular theory, or choice of theories, is problematic and equivocal; the different criteria might imply contradictory evaluative outcomes—theories that score high on simplicity might rate low on accuracy (p. 322). Thus judgments about the worth of any particular theory is intrinsically problematic. The criteria function more as "values" in theory appraisal, rather than rules (p. 331). There is an inescapable "subjective" element to the appraisal of theories; there is a gap between evidence and outcome of applying evaluative criteria.

RETREAT TO WHERE? PHILOSOPHY OR SOCIOLOGY AS GROUND FOR METHODOLOGY

Although Kuhn retreated, he by no means left the philosophical battlefield. In particular he reiterated his claim that "if I am right, then 'truth' may, like 'proof,' be a term with only

[20] Republished in Kuhn (1977).

[21] He singled out Imre Lakatos who famously said that Kuhn reduced scientific change to a matter of "mob psychology" (Lakatos, 1970, p. 178).

intratheoretic applications" (Lakatos & Musgrave, 1970, p. 266). This did not satisfy critics such as Wolfgang Stegmüller, who thought that the crux of Kuhn's theory of science was "a bit of musing" of a philosophical incompetent (Stegmüller, 1976, p. 216). Another critic, Stephen Toulmin, said of Kuhn's 1970 retreat that "On his latest reinterpretation, Kuhn's account of 'scientific revolutions' rests on a logical truism and—as such—is *no longer a theory of conceptual change at all*" (Toulmin, 1972, p. 117). Alan Musgrave echoed this view, saying that Kuhn's qualifications to his 1962 position: "left me feeling a little disappointed. I find the new, more real Kuhn who emerges in it but a pale reflection of the old, revolutionary Kuhn . . . Sociological puzzle-solving will not be subversive of our basic philosophical commitments [about the nature of science]" (Musgrave, 1980, p. 51).

David Stove was a trenchant critic of Kuhn Mark One, and Kuhn Mark Two. He regarded Kuhn's theory of rationality to be "not only false but the exact reverse of the truth" (Stove, 1982, p. 39). Stove thought that Kuhn's irrationalism "stems from the conflation . . . of the descriptive with the prescriptive: from his steady refusal to distinguish the history or sociology of science from the logic or philosophy of science" (p. 4).

Stove has a point, if not more than a point. For instance in his Machette Lecture—"Objectivity, Value Judgment, and Theory Choice"—Kuhn notes, as an historical and sociological fact about science, that theory choice is not only a matter of the application of values, or shared criteria, "but also idiosyncratic factors dependent on individual biography and personality" (Kuhn, 1977, p. 329). This is clearly quite true, but it does not, necessarily, bear upon the rationality of theory choice. Nazi scientists chose hematological theories that fitted Nazi ideology; Stalinist scientists chose genetic theories that fitted historical materialism; Creation scientists chose evolutionary theories that fitted with fundamentalist theology; cold fusion scientists chose theories that would get them maximum publicity and grant money. The list is almost endless. These are all facts about science, but we can call them, and would want to call them, unfortunate or undesirable facts about science. Here the exercise of idiosyncratic factors violated methodological norms. But to make this judgment you cannot identify norms with practice. Kuhn, at best, equivocates on this point.

Stove believes that the key mistake made by both Popper and Kuhn, is the Humean mistake of identifying rationality with deductivism. Stove (1982, p. 86) writes.

> Recent irrationalist philosophy of science [Popper, Kuhn, Lakatos, Feyerabend] is therefore to be ascribed (insofar as it can be ascribed to intellectual causes at all) to acceptance of the thesis of deductivism. What has been decisive in leading these authors to conclude that there can be no reasonable belief in a scientific theory, and *a fortiori* that there has been no accumulation of knowledge in the last few centuries, is a certain extreme belief, by which their minds are dominated, about what is required for one proposition to be a reason to believe another.

If only deductively valid arguments are considered sound arguments, then no amount of finite evidence can make for a sound argument from evidence to a universal conclusion. And as scientific theories are universal (typically "All A are B"), then there are no sound (deductively sound) inferences to the truth of a theory.

Apart from idiosyncratic factors, Kuhn says that scientists bring five values (accuracy, consistency, scope, simplicity, and fruitfulness) to bear upon the appraisal of theories, but he equivocates about whether this is meant in a *descriptive* or *prescriptive* sense; whether he is offering a psychological account of what scientists *actually* do, or a *philosophical* account what they *should* do. He is aware of the difference, and the legitimacy of the question. For what reasons should scientists act in the way *prescribed* by Kuhn? His answer is to derive the prescriptive from the descriptive: the ought from the is. In his 1970 Postscript he says "In the absence of criteria able to dictate the choice of each individual . . . what better

criterion could there be than the decision of the scientific group" (Kuhn, 1970, p. 170). He repeats this answer in his 1973 Machette lecture. If the scientific group has chosen T, then that constitutes, ultimately, the best reason for also choosing it. If the group is split in its theoretical allegiance (revolutionary science) then there is no convincing reason for the choice of any T from the pool of Ts.

Does Kuhn's system allow him to identify rational changes in science? Kuhn was at pains to say that the identification of "good" reasons in theory choice was elusive. In replying to critics of the first edition of *SSR,* Kuhn was adamant that a theory of rationality, or of scientific methodology (deductivism, inductivism, falsificationism, etc.) could not be manufactured prior to the study of scientific change. He says: "To suppose, instead, that we possess criteria of rationality which are independent of our understanding of the essentials of the scientific process is to open the door to cloud-cuckoo land" (Lakatos & Musgrave, 1970, p. 264). So his theory of rationality *derives* from his account of the history of science.

This is an important point and it needs to be handled carefully. In part Kuhn is correct to assert that criteria of rationality have to take account of the history of science, that methodological lessons are learnt in the practice of inquiry as well as factual lessons, that we learn how *best* to inquire (methodology) by the *actual* conduct of inquiry, that we learn about rationality *a posteriori*, not (just) *a priori*. The highway of science is littered with the corpses of *a priori* systems of rationality that were supposed to pass judgment upon the rationality of science.

THE DEFENSE OF METHODOLOGY

But if the point about the interplay of history and philosophy in the derivation of methodological principles is not carefully made, then it opens his account either to charges of circularity or question-begging. Why should the scientific community be so privileged as to be the one that defines what rational decision making is? And where there are alternative scientific communities (Western Science, Christian Science, Islamic Science, Nazi Science, Voodoo Science) how, without recourse to *extra*-paradigmatic criteria, do we pick out Western Science as the practice from which we derive methodological lessons? The reduction of philosophy of science to the sociology of science has these awkward consequences.

Abner Shimony was charitable in saying of Kuhn's derivation of methodological lessons from scientific practice that "his work deserves censure on this point whatever the answer might turn out to be, just because it treats central problems of methodology elliptically, ambiguously, and without the attention to details that is essential for controlled analysis" (Shimony, 1976, p. 582). Less charitably, David Stove wrote this is "the reason why Kuhn can, and must, sentence all present and future philosophers of science to the torments of the damned: that is, to reading the sociology of science" (Stove, 1982, p. 19).

Kuhn does feed Stove's hellish vision. To recognize that ideas have a historical and social dimension, that concepts do not just drop out of the sky, and that people (including scientists) are products of their time—is all to the good. However, the world did not have to await Kuhn to learn this truism: Recall Marx's *Eighteenth Brumaire of Louis Bonaparte* where he writes that "Men make their own history, but they do not make it just as they please . . . they make it under circumstances directly found, given and transmitted from the past. The tradition of all the dead generations weighs like a nightmare on the brain of the living" (Tucker, 1978, p. 595). But for Kuhn, Marx, or anybody else, to confuse these historical, psychological, and sociological matters with normative and logical ones is a major mistake.

For Kuhn to describe *how* and *why* scientists fail to embrace new theories is an interesting enough historical and sociological lesson; for him then to claim that their recalcitrance and

holding out was *justified* is an entirely different matter. The second is a *normative* judgment that depends (explicitly or implicitly) upon methodological criteria. Toulmin well advised on this point that (Toulmin, 1972, p. 117)

> indeed, the more keenly one is aware of the interdependence of concepts and their contexts, the more indispensable certain distinctions become: for instance, that between the intrinsic authority of ideas and the magisterial authority of books, men and institutions, or that between the methodical acceptance of concepts whose merits have been demonstrated and the dogmatic acceptance of concepts whose merits are unproved.

Contrary to Kuhn there are aspects of methodology that are prior to, or independent of, the practice of science. There are, to start with, logical rules, probability theory, and ethical norms. These certainly do not constitute the full complement of methodological directives, and the balance needs to be teased out from engagement with, and reflection upon, the history and practice of science. But it is a *methodological* picture that that is being created here; Kuhn has not really abandoned the orthodox methodological pursuit of philosophy of science.[22] Indeed some commentators see his position as being a variant of standard subjective Bayesianism (Salmon, 1990).

KUHN AND THE STRONG PROGRAM IN SOCIOLOGY OF SCIENCE

While Kuhn was trying to blunt the irrationalist charge, other philosophers, most famously Paul Feyerabend in his *Against Method* (Feyerabend, 1975), were proudly embracing it. And sociologists of science, especially adherents of the Edinburgh "Strong program," were premissing their whole "strong program" upon its validity. Barry Barnes in his *Interests and the Growth of Knowledge* wrote that "recent historical studies, however, in particular those of T.S. Kuhn (1970), effectively undermine this faith; they demonstrate that fundamental theoretical transitions in science are not simply rational responses . . . it cannot be said that there is less of reality to be explained after such a transition" (Barnes, 1977, p. 22). In his *T.S. Kuhn and Social Science*, Barnes remarked that (Barnes, 1982, p. 65)

> as far as the decision between paradigms is concerned, logic and experience alone no more suffice than they do in normal science. There is no appropriate scale available with which to weigh the merits of alternative paradigms: they are incommensurable. To favor one paradigm rather than another is in the last analysis to express a preference for one form of life rather than another—a preference that cannot be rationalised by any non-circular argument.

The strong program's efforts to make the fact of scientific theory choice irreducibly sociological, and their claim that a sociological veil falls between evidence and rational theory choice, has been examined critically by many authors, including Slezak (1994a, 1994b), Suchting (1997), and Kragh (1998). For present purposes, it is worth noting that Kuhn himself was, toward the end of his career, at pains to distance himself from his sociological champions. In his Robert and Maurine Rothschild lecture at Harvard University in 1991, he appraised the sociological turn in the history and philosophy of science, acknowledging that it was "emphasized and developed by people who often called themselves Kuhnians" (Kuhn, 1991/2000, p. 3), but added that "I think their viewpoint damagingly mistaken, have been pained to be associated with it, and have for years attributed that association to misunderstanding" (p. 3). He recognized the merit of detailed sociological examinations

[22] On this point see Shimony (1976, pp. 582–586) and Nola & Sankey (2000).

of proximal and distal causes that affected scientific decision making, but thought that "their net effect, at least from a philosophical perspective, has been to deepen rather than to eliminate the very difficulty they were intended to solve" (Kuhn, 1991/2000, p. 7). Kuhn was happy to recognize that "interest, politics, power, and authority undoubtedly do play a significant role in scientific life and its development" (p. 8) but he adds that "the form taken by studies of "negotiation" has, as I've indicated, made it hard to see what else may play a role as well" (p. 8). He concluded that (Kuhn, 1991/2000, p. 9)

> I am among those who have found the claims of the strong program absurd: an example of deconstruction gone mad. And the more qualified sociological and historical formulations that currently strive to replace it are, in my view, scarcely more satisfactory.

KUHN'S POSITIVISM

The key epistemological and ontological claims of classical empiricism, and of its twentieth century positivist grandchild, are fairly well known. John Locke in his *Essay* wrote: "The mind, in all its thoughts and reasonings, hath no other immediate object but its own ideas, which it alone does or can contemplate" (Locke, 1690/1924). Locke's formulation of the problem of knowledge was used by Berkeley to support idealism and relativism. Berkeley's argument in his *Treatise* was simple but devastating: "As for our senses, by them we have the knowledge *only of our sensations*, ideas, or those things that are immediately perceived by sense, call them what you will: but they do not inform us that things exist without the mind, or unperceived". Rudolph Carnap, one of the most influential twentieth-century postitivists, stated that the crux of positivism was the claim that (Carnap, 1969, p. 108)

> we have to proceed from that which is epistemically primary, that is to say, from the "given" i.e. from experiences themselves in their totality and undivided unity . . . The elementary experiences are to be the basic elements of our constructional system. From this basis we wish to construct all other objects of prescientific and scientific knowledge.

Elsewhere I have indicated that the key philosophical elements of constructivism in science education are, paradoxically, the core commitments of the old-style positivism that constructivists thought they were surplanting; modern constructivism is a case of old philosophical wine in new bottles (Matthews, 1992a, 1992b, 1994, ch. 7, 2000a). The key constructivist epistemological commitments are as follows:

(i) We have no unmediated access to reality.
(ii) Objective knowledge of reality is impossible.
(iii) Sense experience is the foundation of scientific knowledge.
(iv) There is a sharp distinction between observational terms that pick out sense experience and theoretical (nonobservational) terms in scientific theory.
(v) Our sense experiences are the only things of which we have unmediated knowledge.
(vi) Sense experience is both the fount and the test of knowledge claims.
(vii) The object of knowledge is not reality, but experience.
(viii) The bearer of knowledge is the experiencing or cognising subject.
(ix) The test of knowledge is not truth, in the sense of correspondence, but utility.

Additionally there are idealist ontological commitments that are stated more or less explicitly:

(x) The world has no structure, structure is derived from the world being perceived.

And in extreme forms

(xi) There is no world apart from experience.

Various of these eleven commitments can be seen in representative, constructivist-inspired, science educators' claims that

> the constructivist epistemology asserts that the only tools available to a knower are the senses. It is only through seeing, hearing, touching, smelling, and tasting that an individual interacts with the environment. With these messages from the senses the individual builds a picture of the world. Therefore, constructivism asserts that knowledge resides in individuals. (Lorsbach & Tobin, 1992, p. 5)

> In this view [constructivism], the world, as such, has no preestablished form and hence does not admit of direct perception or knowledge . . . the visible world does not exist as such but assumes a form when it is constructed by the eye' (Pepin, 1998, p. 175).

> constructivism, like idealism, maintains that we are cognitively isolated from the nature of reality . . . Our knowledge is, at best, a mapping of transformations allowed by that reality (Bettencourt, 1993, p. 46).

> constructivists choose to consider knowledge as an internally coherent system that we actively build up from within for our own purposes, coping with the world of our individual experience (Staver, 1998, p. 505)

What needs to be recognized is that Kuhn himself did not divest himself of the central philosophical planks of positivism while he was waging his war against it. This is a not uncommon circumstance in all kinds of revolutions: the new incorporates and mimics a good deal of the old. More specifically in his epistemology there would be individualism, an experiential base, and ontological doubt.

Individualism. Kuhn regarded the bearer of knowledge claims, the possessor of knowledge, as the individual scientist. This is an orthodox positivist, and more generally empiricist, tenet: individuals confront the world, have sense impressions and cogitate, then arrive at mental states that are then deemed either knowledge or nonknowledge. In *SSR* all of the Hanson-inspired talk of seeing ambiguous figures, having theory dependent observations, and experiencing gestalt switches is predicated upon an *individual* knowing subject. It is the individual who is the subject, or bearer, of these sensations, observations, and experiences. There is, admittedly, a tension, if not confusion, on this matter in Kuhn's work. This is something that he acknowledges, when in 1986 he says that historians and philosophers need to make a "systematic attempt to separate the concepts appropriate to the description of groups from those appropriate to the description of individuals" (Kuhn, 1986/2000, p. 89), and that "communities do not have experiences, much less gestalt switches" (p. 88). This tension between individuals and groups as the bearer of knowledge is something that Kuhn's underlying positivist assumptions really do not allow him to satisfactorily resolve.

Experiential Base. Kuhn's *SSR* is littered with perceptual examples and discussions. In the Preface he thanks a colleague who first directed him to papers in the psychology of perception from where he learnt of Gestalt phenomena, he also mentions Benjamin Whorf's thesis about the dependence of perception on language. The most cited philosopher in *SSR* is Norwood Russell Hanson, who's *Patterns of Discovery* is replete with "theory dependence of observation" cases (Hanson, 1958), cases that Kuhn discusses in his chapter on "Revolutions as Changes of World View" (Kuhn, 1970, ch. X). Kuhn bolsters Hanson's argument by

recourse to psychological literature, including the gestalt studies of ambiguous figures and the famous Jerome Bruner and Leo Postman studies on expectation and perception (Bruner & Postman, 1949). Kuhn distils these arguments as: "What a man sees depends both upon what he looks at and also what his previous visual-conceptual experience has taught him to see" (Kuhn, 1970, p. 113). These theory-dependent observations serve as a foundation or touchstone for science: "The scientist can have no recourse above or beyond what he sees with his eyes and instruments" (Kuhn, 1970, p. 114).

This whole concern with individual observation and perception is an empiricist concern. Bacon long ago, in his discussion of "Idols of the Mind" (Bacon, 1620/1960) recognized that what we see depends on interests and language. As an empiricist Bacon advised recognizing and correcting these distorting influences. Intelligent empiricists ever since, and right up to the positivists, have recommended the same. The epistemological assumption was that observations could, in principle, be veridical and thus provide a ground for certainty in knowledge and science more particularly. Observations could separate competing knowledge claims. Kuhn is part of this tradition, he is working within the empiricist epistemological problematic or paradigm (if one wishes) excepting that he (and Hanson and other theory-dependence-of-observation proponents) maintain that observations cannot, in principle, be objective. The foundations of science are not unequivocal.

This is another crucial point where one wishes that Kuhn might have elaborated his argument. He says that "what a man sees depends upon what he looks at and also what his previous visual-conceptual experience has taught him to see." "Seeing" is, presumably, some kind of experience the scientist/observer/subject has. A lot hinges on two things: one, whether seeing is the experience, or the report of the experience; two, how much the world (external, objective nature) contributes to the experience. Kuhn doesn't explicate these matters.

It is useful to distinguish *object* perception from *propositional* perception. This is sometimes stated as the distinction between "seeing as" and "seeing that." The former is an observational experience that subjects have but which they do not, or need not, articulate. They confront an object and have whatever perceptual experiences are generated by that object. For example, newly born infants have a multitude of experiences as their eyes range around their cot; adults, as they drive a car along a road, have all manner of perceptual experiences without, necessarily, any of them registering or being noticed. These are cases of *object* perception. Traditionally, there have been *strong* and *weak* interpretations of object perception. To have object perception in the *strong* sense means that there is an external object that is causally responsible for the experience. Babies have whatever experience they have of seeing a hanging mobile in virtue of the hanging mobile being there to create or generate the experience. To have object perception in the *weak* sense means that the subject has whatever experience they have, and no well founded inference can be drawn about the existence of anything external to the subject that causes the experience. That is to say that some subject's experience is a *strong* object perception is to be committed to there being an external object apart from the subject; to say that some subject's experience is a *weak* object perception means there is no commitment to the existence of an external object causing the experience.

Propositional perception, or seeing that, means that the subject articulates or describes what they see. They see that something or other is the case. It is usually expressed as "The subject sees that p," where "p" is some proposition stating a fact about the world. Thus "Harry sees that the prime minister is walking by," or "Jane sees that the pendulum is isochronous." At a less specific level, these statements could be "Harry sees that a person is walking by" and "Jane sees that a pendulum is swinging." Propositional perception is veridical if "p" is true, it is non-veridical if "p" is false. If "p" is false, then the above statements are rendered as "Harry thought he saw that the prime minister was walking by" and "Jane thought she was that the pendulum was isochronous."

Clearly propositional perception depends on the subject having appropriate concepts. You cannot see "that p," unless you have the concepts for expressing whatever p might be. Thus, perception of this kind is, by definition, dependent upon participation in a language community. There is no mystery about theory dependence of propositional observation; it is a tautological claim. Two people might have identical propositional perceptions about pieces on a chess board (they both can recognize a chess board and the array of chess pieces), but to further see "that the black bishop is threatened by the white castle" requires a good deal of internal knowledge and concepts relating to the rules of chess, that maybe only one of them has, and no amount of more concentrated looking by the other will allow them to have the second propositional perception. What is needed for the second is knowledge and experience of chess.

Propositional perception involves truth claims: one can only be said to "see that p" if "p" is a true proposition. You cannot see "that the black bishop is threatened by . . . ." unless the white castle is in fact so threatening it. But the latter circumstance is a circumstance about the world (the placement of the pieces on the board plus the rules of chess).

Now Kuhn is in an awkward position. He wants perception as the foundation for science, indeed he wants (without saying it clearly) propositional perception (as this is the kind of perception relevant to science), but he does not want truth. Indeed Kuhn is at pains to provide a "truth-free" account of science and its progress (see the first section above of this article). But without a robust (correspondence or neo-correspondence) account of truth, then propostitional perception is on a slippery slope toward relativism and ontological idealism. And there is plenty to suggest Kuhn and Kuhnians make the trip right to the bottom.

Ontological Doubt. Kuhn shares the positivist, antirealist, view that we cannot know reality, nor have any direct access to it. Again one wishes that he would spell out more exactly just what his ontological claims are, but instead of doing so, he muddies the ontological waters. When discussing the discovery of oxygen (or the creation of the concept of oxygen, as some might say), Kuhn says that "Lavoisier saw oxygen where Priestley had seen dephlogisticated air," and that "after discovering oxygen Lavoisier worked in a different world" (Kuhn, 1970, p. 118). Once more there is excruciating ambiguity here: Lavoisier did do different theoretical and practical work after discovering oxygen, but that is a commonplace and hardly controversial, or worth saying; but on any decent realist view of things, the world itself did not change; there was no ontological shift in the constitution of the world. Before and after 1779 the earth's atmosphere contained about 20% of oxygen.

When discussing Aristotelian and Galilean accounts of pendulum motion, Kuhn says that "Pendulums were brought into existence by something very like a paradigm induced gestalt switch" (Kuhn, 1970, p. 120). Again, yes and no. Galileo recognized (or saw, if we wish to speak loosely) that pendulum motion, inclined plane motion and free fall were all just one kind of motion, and could be analyzed accordingly (Matthews, 2000b, ch. 5). But, as Kuhn acknowledges, bodies were swinging on the ends of cords long before this intellectual achievement of Galileo. Galileo's achievement did not bring anything apart from, most importantly, a new theory into existence; it relabeled what was already in front of everyone's eyes.

Kuhn in his 1991 lecture on "The Trouble with Historical Philosophy of Science" does address some of these ontological puzzles—he says "it's as a philosopher that I speak this afternoon" (Kuhn, 1991/2000, p. 106). He asserts that "In the absence of a rigid Archimedean platform . . . it's hard to imagine . . . what the phrase 'closer to the truth' can mean (p. 115). Whatever truth might be, it "cannot be anything quite like correspondence to reality" (p. 115). Further, "no sense can be made of the notion of reality as it has ordinarily functioned in philosophy of science" (p. 115). Truth has no role to play in the crucial task of rationally selecting between competing scientific research programs,

rather "what evaluation aims to select is not beliefs that correspond to a so called real external world, but simply the better or best of the bodies of belief actually present to the evaluators at the time their judgments are reached" (p. 119). In the place of a "mind- and culture-independent external world" Kuhn proposes a "variety of niches . . . which both create and are created by the conceptual and instrumental tools with which their inhabitants practice" (p. 120).

Kuhn's notion of a "niche" is of the utmost epistemological importance with immense flow-on effects for culture, science, philosophy, and science education. On the account given above, judgments about propositions and theoretical claims are always made within a niche and always without recourse to truth, as normally understood. Indeed to claim truth in the normal sense is simply, for Kuhn, to be deluded, there is no such thing; even the idea of approximate truth is ruled out. Thus Kuhn, and those affected by him, standardly resort to putting "truth" in scare quotes, along with "reality," "know," "discover," "rational," and so on. Such quotation marks used once designate the words enclosed within them; now they indicate that the writer is, supposedly, an epistemological sophisticate who recognizes that although the old words are being used, they do not have the old meaning.[23]

Niches, and what goes on within them are, for Kuhn and Kuhnians, protected from intellectual and scientific appraisal. If you are not in the niche, you cannot appraise; and even if you are in the niche, or loop, appraisal by reference to reality is not allowed. Little wonder that practitioners of Islamic, Hindu, and Creationist science embraced Kuhn.[24] And, if they had lasted long enough, practitioners of Nazi and Soviet science might have done the same. In all cases, adherents can simply claim to be working in a niche that they create with their conceptual and instrumental tools, and that any attempt to appraise their claims by reference to how the world is constituted, is simply a philosophical folly. And, of course, there is ample evidence to suggest that once given a niche, practitioners take a cave. There has been an explosion of epistemological niches with most of them now occupying academic and institutional corridors: queer theory, black theory, white theory, hundreds of indigenous knowledge theories, and so on. Multiple approaches to understanding natural and social reality is laudable; what is lamentable is when these approaches insulate themselves from criticism and appraisal by invoking "special niche privileges"—as when, for example, indigenous groups claim that they alone can investigate their origins.

CONCLUSION: LESSONS FOR THE SCIENCE EDUCATION COMMUNITY

In 1993, when responding to commentators on his mature philosophical position, Kuhn reflected on the reception of *SSR*, saying (Kuhn, 1993, p. 314)

> To my dismay, . . . my "purple passages" led many readers of *Structure* to suppose that I was attempting to undermine the cognitive authority of science rather than to suggest a different view of its nature.

[23] A nice example is provided by the feminist philosopher Elizabeth Grosz, who in describing the work of another feminist, Luce Irigaray, writes (Grosz, 1993, p. 209, quoted in Radcliffe-Richards, 1997, p. 385)

> Irigaray's work thus remains indifferent to such traditional values as "truth" and "falisity" (where these are conceived as correspondence between propositions and reality). . . . She both combats and constructs, strategically questioning phallocentric knowledges without trying to replace them with more inclusive or more neutral truths.

[24] On Hindu science see Nanda (1997, 1998), on Islamic science see Hoodbhoy (1991), on Creation science see Scott (1997), and on Feminist science see Radcliffe-Richards (1997).

Although he did not deny writing the "purple passages," he nevertheless lamented that scholars were misappropriating his work, saying that many were "retrieving from my work ideas that had no place there" (Hoyingen-Huene, 1993, p. xi)—this does leave one wondering why the ideas were written there. Philip Kitcher agreed with Kuhn's lament, saying that much of what popularly passes for Kuhnian analysis is just a caricature of his views (Kitcher, 1982, p. 168). The physicist-philosopher, Abner Shimony, wrote of Kuhn's work that "the great value of these insights has been debased by drawing from them relativistic and subjectivistic epistemological conclusions" (Shimony, 1991, p. 96).

The science education community is as guilty as any other of the charge of misunderstanding Kuhn, and drawing relativistic and subjectivistic epistemological conclusions. There has been very little prolonged engagement with his writings, and even less prolonged critical engagement. As Loving and Cobern stated in their review "None of the articles examined . . . offered any real critique of Kuhn's positions," and the science education community has become a "admiration society for Thomas Kuhn" (Loving & Cobern, 2000).

Up to the Sputnik era, and beyond, the science education community swore allegiance to the then dominant logical empiricist view of science, and this allegiance impacted heavily on the major Sputnik-inspired curricular reforms of the 1960s (Duschl, 1985). Having belatedly discovered the new philosophy of science, in particular Thomas Kuhn's new philosophy, the community swore a new allegiance. Despite realist views being robustly asserted by prominent philosophers of science, many science educators, for instance, believed that "constructivist views . . . hold sway among current philosophers of science" (Benson, 1989, p. 342). While others disarmingly asserted that constructivism is "the most mature epistemological commitment" (Roth & Roychoudhury, 1994, p. 28), thereby consigning realist philosophers of the stature of David Armstrong, Michael Devitt, Wallis Suchting, Ernan McMullin, Ian Hacking, and Clark Glymour (to name just a few) to the philosophical kindergarten.[25]

It appears that the lesson to be learnt here is the old lesson: the science education community should more effectively engage with on-going debates and analyses in the history and philosophy of science; and although members of the education community should be lauded for taking and arguing positions in the history and philosophy of science, these positions should be held with full acknowledgment that most of them are disputed.

But this simple lesson is not quite the end of the story. Karl Marx said of philosophical critique that it was insufficient to simply point out errors, rather it is necessary to explain how the errors came to be. As far as the science education community's shifting allegiances in history and philosophy of science is concerned, they are probably explained by the following factors:

First, for the most part science educators are not trained in the history and philosophy of science (HPS). Standardly they complete a first or higher degree in science, then do postgraduate studies in education, then take a job, then get on the academic tenure and promotion treadmill. For all sorts of reasons they need to address, in their teaching and writing, matters concerning the nature of science (NOS), but they lack the training to do this in an informed and diligent manner. They basically look, without much discernment, to the history and philosophy of science community to provide answers to their NOS questions. Science educators pretty much have to take what appears the most popular NOS position. Prior to the 1960–70s this was logical empiricism; after the second edition of Kuhn's *Structure* appeared in 1970, Kuhnianism was the most popular view of the NOS. And if they do not take the most popular, they frequently take whatever fits with, and reinforces,

[25] For a critical appraisal of constructivism as a theory of science and as an epistemology, see contributions to Matthews (1998).

their own proto-philosophical tendencies or prejudices. And Kuhn certainly reinforced a lot of constructivist-inspired relativism and subjectivism in the science education community.

Second, it is hard for science educators to make up the shortfall in HPS training on the job. The hot-house pressure induced by tenure committees means that very little time can be spent in the library, engaging in scholarship, or even in thinking. The demands to publish, to attend conferences, to engage in teacher development activities, to write grant proposals, and to develop new courses are so great that finding time to carefully read a book such as Kuhn's, much less to read the source material that it is built on (the texts of the Galilean revolution, for instance), or the critical literature that flowed from it is nigh well impossible. Conference presentations, in-service courses, publications can all appear on a CV. Books carefully read do not appear on CVs. So if the first consideration leads to Kuhn, this second consideration means that he is not seriously engaged with, or criticized.

Unfortunately these problems that the science education community has with history and philosophy of science are not singular. The community also, perforce, has to teach, write, and research on children's learning without benefit of much training in learning theory or cognitive science. Similarly for topics in the philosophy of education that also have to be taught and researched, again without benefit of formal training in the area. It is not surprising that across all three areas superficial popularism is so prevalent in science education literature. It is hardly any individual's fault, the system conspires against genuine scholarship and deep command of the disciplines.

Three small steps that might mitigate the current unfortunate situation are as follows:

(i) Make the completion of a HPS course a requirement of the PhD program in science education.
(ii) Require the involvement of a historian or a philosopher on the doctoral committee for all science education theses dealing with Nature of Science themes.
(iii) Encourage joint appointments between science education and HPS departments.

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