Opinion Piece: Thomas Kuhn and Science Education: A Troubled Connection

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Publications here.
A pen-picture here.

Thomas Kuhn was the most influential historian of science in the twentieth century. His impact was felt in all academic fields and beyond the academy into society and culture. 'Paradigm' is a commonplace in newspaper editorials, political speeches, and much else, 'incommensurability' is oft heard in religious debate, and 'theory dependence' is everywhere in social science, 'alternative realities' is part of indigenous science discourse. Kuhn published a great deal[1], but his global reputation was based upon one work: *The Structure of Scientific Revolutions* first published sixty years ago (1962). Kuhn's impact on science education has been immense, and is testified to in the opening sentence of a very contemporary 2022 article written by David Treagust, Australia's foremost science educator[2]:

> Perhaps one of the major influences on our understanding of how scientific research and scientific knowledge evolves and develops was the publication of Thomas Kuhn's (1962) *The Structure of Scientific Revolutions*. This small book really changed the way we look at the enterprise that is science. (Treagust 2022, p.16)

The first edition of *Structure* appeared in 1962 in the obscure *International Encyclopedia of Unified Science* (Volume 2 Number 2) edited by the logical empiricist Otto Neurath and read by a small coterie of philosophers. Its argument lay dormant until the enlarged second edition was published as a book in 1970.

The second edition precipitated the Kuhnian tsunami. It was quickly translated into 16+ languages and sold over a million copies. In Australia's *Arts and Humanities Citation Index*, it was the most cited book on any subject through the 1970s and 1980s. Doubtless it held much the same position in comparable indexes in most countries, both English-speaking and otherwise. The above

[1] A 2000 listing of his publications, beginning in 1945, runs to ten pages (Conant & Haugeland 2000, pp.325-335).
[2] David Treagust has 36,000 citations. He is not a minor or peripheral figure. His quoted words indicate how normalised Kuhn's 'picture of science' has become among educators.

listed July centenary conference celebration of his life and work is witness to the enduring interest in Kuhn's work.

Pedagogical Origins of Structure

What is oft overlooked is that Kuhn's 'revolutionary' account of science had educational origins, namely his teaching in James Conant's Harvard General Education Programme. And the early formulation of Kuhnian theory was fuelled by reflection and research on science pedagogy.

In the Preface of Kuhn's first book, *The Copernican Revolution*, which arose from his lectures in Conant's programme, he writes:

> Work with him [Conant] first persuaded me that historical study could yield a new sort of understanding of the structure and function of scientific research. Without my own Copernican revolution, which he fathered, neither this book nor my other essays in the history of science would have been written (Kuhn 1957, p. xi).

Kuhn had a serious and deep interest in how students learn science, and how they come to give meaning to basic scientific concepts such as mass, acceleration, field and so on. His 1959 address 'The Essential Tension' (Kuhn 1959) was his first substantial discussion of the characteristics of effective pedagogy and the cognitive mechanisms involved in learning scientific concepts. He maintained this interest to the end of his career.

Kuhn's pedagogical interest is well displayed in his little-known 1990 essay 'On Learning Physics'[3]. The essay is a careful and valuable contribution to the science of learning. It gives due recognition to science teachers in the communicating, embedding and development of science:

> The vocabulary in which the phenomena of a field like mechanics are described and explained is itself a historical product, developed over time, and repeatedly transmitted, in its then-current state, from one generation to its successor. (Kuhn 1990/2000 p.11)

He gives detailed attention to five factors that are required for the learning of 'force', 'mass' and 'weight'. Students are adding to their everyday vocabulary, to their lexical repertoire. But the Newtonian concepts 'can only be acquired together with the theory itself' (p.11). He supposes there are only two ways the Newtonian concepts can be learnt:

> One that stipulates the second law and finds the law of gravity empirically; another that stipulates the law of gravity and discovers the second law empirically. (p.16)

A consequence is that individual speakers 'will differ about the epistemic status of generalisations that the community members share' (p.16). This is fine, provided there are not anomalies between theory and expected observations. But when there are anomalies, for example 'between celestial observations and the motion of the lunar perigee,' then problems arise.

> Scientists who had learned Newtonian 'mass' and 'weight' along the first of my two lexical-acquisition routes would be free to consider altering the law of gravity as a way to remove the anomaly. On the other hand, they would be bound by language to preserve the second law. On the other hand, scientists who had acquired 'mass' and 'weight' along

[3]The essay is contained in 'Dubbing and Redubbing: The Vulnerability of Rigid Designation' in C.W. Savage (ed.) *Scientific Theories*, University of Minnesota Press, 1990, pp.302-308. And reproduced in *Science & Education* 9(1-2), 2000.

> my second route would be free to suggest altering the second law but would be bound by language to preserve the law of gravity. (Kuhn 1990/2000 p.16)

For explication he directs readers to his paper 'Commensurability, Comparability, Communicability' (Kuhn 1982). And in a footnote he writes:

> Despite my critics, I do not think that the position developed here leads to relativism, but the threats to realism are real and require much discussion. (Kuhn 1990/2000 p.19)

It was no accident that, very early in Kuhn's career and certainly by 1966, he sought to collaborate with Jean Piaget, the internationally renowned developmental psychologist. Kuhn popularized Piaget's 'cognitive ontogeny recapitulates scientific phylogeny' thesis among historians and philosophers of science. Piaget had written:

> The fundamental hypothesis of Genetic Epistemology is that there is a parallelism between the progress made in logical and rational organisation of knowledge and the corresponding formative psychological processes. (Piaget 1970, p.13)

Piaget was, in turn, repeating the Hegel-Darwin informed opinion of Herbert Spencer that was expressed in the latter's 1861 *Essays on Education and Kindred Subjects*:

> If there be an order in which the human race has mastered its various kinds of knowledge, there will arise in every child an aptitude to acquire these kinds of knowledge in the same order... Education is a repetition of civilization in little.

This opinion had underwritten the popular history-based *Genetic* approach to curricula structure. The connection was well expressed by J.C. Hogg in his 1938 chemistry text:

> The historic development is a logical approach. The slow progress of the early centuries was owing to a lack of knowledge, to poor technique and to unmethodical attack. But these are precisely the difficulties of the beginner in chemistry. There is a bond of sympathy between the beginner and the pioneer. (Hogg 1938, p.vii)

It was no idle claim of Kuhn's when he wrote:

> 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).

It is of some interest to those studying the growth of disciplines that Kuhn recognised how accidental and serendipitous was his discovery of Piaget's work:

> A footnote encountered by chance [reading Merton's thesis] 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 this development exhibited discontinuities, played a role in Kuhn's characterisation of scientific development[4]. In any *rational*, cleaned-up reconstruction of Kuhn's theory this input would be important, but its realisation was accidental.

Conversely, Alexander Koyré, the historian of science, averred that it was Aristotle's physics that

[4] See Kitchener (1986), Siegel (1982, 1985) and Rowell (1993).

taught him to understand Piaget's children. Summarising the two-way interaction, the philosopher Philip Kitcher believed 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 historians and philosophers of science can learn from the experimental results and analyses of the child psychologists (Kitcher 1988).

Given that learning, cognitive apprenticeships, transmission of basic concepts, and mastering of methodologies, were important components of the establishment of a paradigm, it is not surprising that Kuhn was engaged by such questions regarding human learning[5]. 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 advocates of the 'new philosophy of science'.

In one of the rare studies of Kuhn and education, Hanne Andersen points out:

> Kuhn's early interest in science education centers around two claims: (1) the empirical claim that science education as it actually takes place does lead to convergent thought, and (2) the normative claim that the development of convergent thought through rigorous training is necessary for the progress of science. (Andersen 2000, p.91)

Educators' Early Neglect of *Structure*

The first edition of Structure (1962) had little impact on anything and zero impact on science education. John Robinson's *The Nature of Science and Science Teaching* (Robinson 1968) was the very first book to link together philosophy of science and science teaching. Kuhn is nowhere mentioned in its 150 pages (Matthews 1997).

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*. Kuhn is not mentioned.

In 1969, Hans O. Andersen published *Readings in Science Education for the Secondary School* (Andersen 1969). It was a collection of 60 research papers informed by commitment to the principle:

> Science instruction should be based on a series of principles selected for their value in projecting science as a process of inquiry designed to discover new facts, improve quantitative descriptions of known facts, and organize these facts into conceptual schemes which more adequately describe the phenomena of the universe and beyond. (Andersen 1969, p.2)

And:

> The only way to succeed in increasing science enrollments [sic] without a subsequent loss of positive attitude is to make the course offerings so interesting and so valuable to the student that he will demand more. (Andersen 1969, p.2)

The discipline was 'calling out for Kuhn' but *Structure* sat unread. Kuhn is not mentioned in the anthology's 430 pages and 60 readings.

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

Nor did Kuhn inform the widespread post-Sputnik curriculum debates of the 1960s. In the US, 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 UK, the various Nuffield schemes were launched at the same time. It has been oft 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 by science educators. Yehuda Elkana 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' (Elkana 1970, p.8).

Elkana lamented that Kuhn's *Structure* and Joseph Schwab's 'The Teaching of Science as Inquiry' (Schwab 1960) were published at the same time, 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.

A few years later, Michael Martin, the Boston University philosopher, surveyed the same literature 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 (EPC 1966), and charted the myriad ways in which it, and other curricula as well, reproduced simplistic inductivist understanding of scientific inquiry (Martin 1972, 141-147).

The homely inductivism of *Education and the Spirit of Science* had the imprimatur of the highest office in US education. It was published eight years after Norwood Russell Hanson's *Patterns of Discovery* (Hanson 1958) which received wide philosophical attention for its 'theory dependence of observation' thesis, seven years after Popper's anti-inductivist work *The Logic of Scientific Discovery* (Popper 1934/1959) was translated into English and also given wide philosophical attention, and four years after the publication of Feyerabend's essay 'Explanation, Reduction, and Empiricism' that shook the foundations of inductivist accounts of science (Feyerabend 1962).

In 1974 Martin opined:

> a great deal has been written on the philosophy of science; perhaps even more has been written in science education. However, surprisingly little has been written on the relation between the two areas. (Martin, 1974, 293)

The unfortunate divide of the time between HPS and science education was well documented in a study by Richard Duschl titled 'Science Education and Philosophy of Science: Twenty-five Years of Mutually Exclusive Development' (Duschl 1985)[6]. Though, given what educators were subsequently to make of their discovery of Kuhn, the delay perhaps was no bad thing. Had Kuhn been discovered along with his critics, it would have been a good thing. It would have informed and lifted educator's understanding of the scientific endeavour. That did not happen.

Delayed Embrace of Kuhn

Publication of the second edition of *Structure* (1970) changed things dramatically: Kuhn was enthusiastically taken into education and into much else. In 1985, Derek Hodson published a review of research on 'Philosophy of Science, Science and Science Education' in which he ascertained that of 22 articles published, and theses submitted, in the period 1974-1984, fourteen 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 thirteen-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, incommensurability, authenticity of textbooks, the social components of science, and also the philosophical comparison of Kuhn and other methodologists of science (Loving & Cobern 2000). They commented that the science education community had become a Kuhnian cheer-squad.

The embrace is laid out in one of the first science education articles to engage with Kuhn's theory, namely Ted Cawthron and Jack Rowell's '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:

> 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. (Cawthron & Rowell 1978, p.45)

The most influential article in conceptual change research was one written by George Posner and colleagues 'Accommodation of a Scientific Conception: Toward a Theory of Conceptual Change' (Posner, Strike, Hewson & Gertzog 1982)[7]. It is explicitly based on Kuhn's analysis of paradigm change in science. One of the authors of that study noted this dependence and itemised 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.

[6] Some of this history of separate development is discussed in Matthews (1994, chap.2). An exception to 'silo' research was the work of Harvey Siegel (1978, 1979, 1989).

[7] Having 9,100+ citations (April 2022).

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).

Despite their explicit entreaty, the bulk of conceptual change research, or research on the learning of science, which followed Posner and Strike's paper ignored philosophy. Hardly surprising as philosophy is not part of science teacher education and is rarely part of education graduate programmes. Educational research did not, and more seriously could not, engage with what might constitute 'rational' conceptual change: philosophical competence was needed to identify rational conceptual change. There has been a very deep cleavage between serious epistemology and psychology in educational research. For example, the supposed study, by educators, of knowledge acquisition is really just the study of changing beliefs: if beliefs develop, then knowledge develops. Psychologists and educational researchers are indifferent to whether the change is rational, irrational or anything else.

Kuhnian Philosophy and Constructivism in Education

Kuhnian philosophy[8], more particularly his epistemological relativism and his ontological idealism, had enormous impact on educational theory and curriculum. Kuhn is front and centre in constructivism which for nearly forty years has dominated educational research and theorising[9]. All leading constructivists acknowledge Kuhn as the fount of their relativist and idealist view of science.

Derek Hodson wrote:

> 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).

Ernst von Glasersfeld, in the opening sentences of a much-cited paper, said that Kuhn's *Structure* '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 Structure '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 research programme on children's alternative conceptions (Novak 1998, p.6). Nancy Davis and colleagues used 'Thomas Kuhn's (1970) work as a basis to support change in guiding epistemological

[8]The term 'Kuhnian' rather than 'Kuhn's' is deliberate as it is notorious that a great many positions were advanced in Kuhn's name that he did not recognise as his own.

[9]For an account of the influence of constructivism in science education, see Matthews (2000, 2015 chap.8). For wider views of the matter, see contributions to Phillips (2000).

paradigms' whereby they endorse constructivism and reject objectivism (Davis et al. 1993, p.627).

Cathleen Loving's and William Cobern's review of Kuhn's influence on science education research noted that 'there is not a single critical voice; the science education community has turned into an admiration society for Thomas Kuhn' (Loving & Cobern 2000, p.199).

But these Kuhnian connections with education have not received the attention they deserve. Characteristically, the programme of the Kuhn centenary conference makes no reference to the pedagogical origins of Kuhnian thought, or to its impact on educational theorising.

To give greater exposure to Kuhn's engagement with pedagogy, and his impact on educational theorising, a thematic issue of *Science & Education* on 'Kuhn and Science Education' (Vol.9 Nos.1-2, 2000) was organised and I contributed a ten-page Editor's Introduction. There were contributions from philosophers, scientists, historian, educators and psychologists: Alexander Levine, Stephen Brush, Steve Fuller, Berry van Berkel, Wobbe de Vos, Adri Verdonk and Albert Pilot, Hanne Andersen, Cathleen Loving and William Cobern, Robert Nola, Harry Shipman, Stellan Ohlsson, Mick Nott, and Howard Sankey. Included in the issue was Kuhn's 1990 essay 'On Learning Physics'. Following this up, two years later I published a long article in Science Education on 'Kuhn and Education' (Matthews 2004).

Kuhn and Philosophy: A Fraught Relationship

Kuhn's central philosophical ideas were not novel; something Kuhn oft acknowledged[10]. Many elements of his philosophy of science were extant when the first edition of *Structure* was published in 1962. The intellectual ground for the Kuhnian 'revolution' had been well prepared. Simple empiricist and logical positivist understandings of science had been challenged on many fronts.

Marx's 1852 *Eighteenth Brumaire of Louis Bonaparte* could have been, and by a few was, appealed to by doubters of the orthodox empiricist account of science. Marx memorably wrote:

> 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)

This is a harbinger of the sociology of knowledge, and was acknowledged as such by Karl Mannheim, the founder of that discipline (Mannheim 1936/1960). Marx's observation was consistent with Kuhn's programme, though I am not aware that Kuhn referred to it.

In the 1930s, Ludwik Fleck wrote on the social construction of facts and on the necessity of an historical component for understanding (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). In the 1940s, R.G. Collingwood elaborated how particular periods in the history of science had dif-

[10] See repeated acknowledgements of scholars in his autobiographical interview (Baltas, Gavroglu & Kindi 1997).

ferent 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 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, Norwood Russell Hanson wrote on the theory dependence of observation and on the contested nature of the facts in scientific disputes (Hanson 1958). Michael Polanyi 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).

Few, if any, of these 'unsettling' positions found their way into the comfortable orthodox empiricist/inductivist understanding of science that dominated education research and the preparation of science teachers.

Kuhn's *Structure* brought all these contra-empiricist elements together in a way hitherto not seen and, for whatever combination of philosophical, sociological and cultural reasons, gave them enormous exposure. The time was ripe for Kuhnianism. In the 1970 Postscript to Function he famously, or infamously, said that truth was irrelevant to judgements 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).

This formulation fails to distinguish *fallibilism* which characterises good scientific understanding from *absolutism* which is irrelevant to science. Further, Kuhn simply rejected 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. Ernan McMullin recognised that: 'The radical challenge of *Structure* is directed not at rationality but at realism' (McMullin 1993, p.71).

Kuhn's 'novel' ideas were taken out of the philosophy corridor and let loose in the marketplace. The Kuhnian wave broke over philosophy departments, and in quick succession other humanities, social science and education departments.

My own first exposure to Kuhn was fifty years ago (1973) as a University of Sydney philosophy student after some years of science teaching. It was a fourteen-week, final-year honours seminar[11]. It was devoted to a detailed reading of the second edition of *Structure*, along with the essays in the related Imre Lakatos and Alan Musgrave edited *Criticism and the Growth of Knowledge* (Lakatos & Musgrave 1970). It was a precious learning experience. Wal Suchting was the convenor, there were ten or so students. Staff members Michael Devitt, Alan Chalmers, David Stove, and others, week-by-week contributed.

While a good many, if not most, scholars around the world were impressed, if not 'bowled over' by Kuhn, the Sydney philosophers in 1973 were not. They were under-impressed with Kuhn's philosophical arguments; indeed, they thought there were barely any such arguments. Wittgenstein,

[11] Sydney philosophy, and the Kuhn seminar, are described in Matthews (2021, pp.75-82).

Braithwaite, Polanyi, Whewell, Popper, Goodman and Hanson are the only philosophers cited in the first edition of *Structure*. And these, with the notable exception of Hanson, are mentioned only 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. What arguments there were, amounted to empiricism in new clothes: theory dependence of observation still took observation as an epistemological fundamental. Suchting maintained, for instance, that debate about the theory dependence, or otherwise, of observation was just an in-house empiricist family-squabble. As he later expressed the matter:

> The central deficiency of empiricism is one that it shares with a wide variety of other positions, namely, all those that see objects themselves, *however they are conceived*, as having epistemic significance *in themselves*, as inherently determining the 'form', as it were, of their own representation. (Suchting 1995, p.13)

David Stove subsequently wrote of Kuhn:

> 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).

Alan Chalmers believed that *Structure* contained two incompatible strands: one *relativist*, that was developed by sociologists of knowledge; the other, *objectivist* that could have been, but was not, advanced by Kuhn (Chalmers 2013, p.115).

The Sydney department was also under-impressed with Kuhn's historical analyses, especially his pivotal account of Galileo's physics which they thought was plainly mistaken. The department was a hold-out against the Kuhnian enthusiasm that swept through the academy in the 1970s, washing out ideas of truth, objectivity and universality from nearly all humanities and social science departments, and a good many philosophy departments.

The department's honour's seminar proved to be an inoculation against the irrationalism that would soon sweep education faculties and teacher-education institutions. Having page-by-page read *Structure*, with a group of serious scholars, it was jarring to see the completely cavalier, uncritical Kuhnianism that emerged in education circles, and elsewhere, in the 1970s and '80s (and through to the present day).

Sydney philosophers were not the only holdouts. Mario Bunge recounts in his autobiography that in 1966 he attended an influential colloquium on causality convened in Geneva by Jean Piaget. Kuhn was a participant. Bunge observed:

> Kuhn's presentation impressed no one at the meeting, and it confirmed my impression that his history of science was second-hand, his philosophy confused and backward, and his sociology of science non-existent. (Bunge 2016, p.181)

This is too harsh a call on Kuhn's historical work, at least of his careful studies of the history of quantum theory (Kuhn 1978).

Israel Scheffler, who had joint appointments in the Harvard Philosophy and Education departments, responded to the first edition of *Structure*, arguing 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).

John Searle, a philosopher, observed:

> …the remarkable interest in the work of Thomas Kuhn on the part of literary critics did not derive from a sudden passion in English departments to understand the transition from Newtonian Mechanics to Relativity Theory. Rather, Kuhn was seen as discrediting the idea that there is any such [objective] reality. If all of 'reality' is just a text anyway, then the role of the textual specialist, the literary critic, is totally transformed. (Searle 1994, p.38)

Jan Golinski, an historian, wrote:

> I see Kuhn as having little positive influence on philosophers and almost none (directly) on historians. His most significant influence within science studies was mediated by sociologists, whose reading of his work he specifically repudiated (Golinski 2012, p.15).

Alexander Bird concluded a sympathetic appraisal of Kuhn with the qualification:

> 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. (Bird 2000, p.ix)

Abner Shimony, a Boston University physicist and philosopher, said of the key Kuhnian move of deriving 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)

Wolfgang Stegmüller, an Austrian philosopher, opined that the crux of Kuhn's theory of science was 'a bit of musing' of a philosophical incompetent (Stegmüller 1976, p.216).

Stegmüller's was a harsh judgement, but Kuhn was candid in admitting that he had no training in philosophy and was an 'amateur' (Kuhn 1991/2000, p.106). And, to a point, he thought that having no formal training was advantageous: He was not schooled in 'old thinking', he did not develop a certain 'cast of mind'. The Sydney philosophers valued their cast of mind – write clearly, avoid purple passages, pay attention to evidence, develop arguments, value scholarship, be consistent, know the tradition, and so on. They thought it was the educational task of the philosophy discipline to forge such a 'cast of mind' in students.

It is noteworthy that his long, and charming, 1997 autobiographical interview with Aristides Baltas, Kostas Gavroglu and Vasso Kindi is, significantly, titled: 'A Physicist who became a Historian for Philosophical Purposes'[12]. Kuhn relates:

> I had made that attempt to investigate going into philosophy immediately after the war when I first came back and got into [Harvard] graduate school and I decided I wasn't going to go back to fulfill undergraduate philosophy. And in certain respects I'm extremely glad I didn't, because I would have been taught things that would have given me a cast

[12] The interview originally appeared in a Greek philosophy of science journal and was then reproduced in Conant & Haugeland (2000).

> of mind which would have, in many ways, helped me as a philosopher, but they'd have made me into a different sort of philosopher. So, I had decided, when I applied to the Society [Harvard Society of Fellows], to do history of science. My notion was, and my application indicated, that there was important philosophy to come out of it; but I needed first to learn more History. (Baltas, Gavroglu & Kindi 1997, p.166)

Did Kuhn ever learn philosophy? A moot question. After Harvard, he went to University of California, Berkeley, with beginning teaching appointments in both the History and the Philosophy Departments. At tenure time, the Acting Chancellor called him in and relayed:

> The recommendation for your promotion has now gone all the way through, it's favourable, and I have it on my desk. There is just one thing. The senior philosophers voted unanimously for your promotion – in History. (Baltas, Gavroglu & Kindi 1997, p.182)

For Kuhn:

> I was extraordinarily angry ...and very deeply hurt, I mean that's a hurt that has never altogether gone away. (Baltas, Gavroglu & Kindi 1997, p.182)

He did consciously try to make up lost philosophical ground and this can be seen in his 1993 'Afterwords' in the Paul Horwich volume devoted to his work (Kuhn 1993). Other physicists did do 'the hard yards' in philosophy – Abner Shimony and Mario Bunge come immediately to mind and so better served 'philosophical purposes'[13].

Kuhn and Science-Technology-Society (STS) Studies

Naomi Oreskes, an historian and philosopher[14], surveyed Kuhn's work and gave a very constrained account of his contribution to HPS:

> Scholars generally agree that the largest impact of Kuhn's work – besides adding the term *paradigm shift* to the general lexicon – was in helping to launch the field of science studies. (Oreskes 2020, p.66)

Oreskes is correct in this assessment. Three STS scholars acknowledged Kuhn as the founder of their discipline, and went on to say in their Editorial Introduction to an STS anthology:

In the wake of STS research, philosophical words such as truth, rationality, objectivity, and even method are increasingly placed in scare quotes when referring to science – not only by STS practitioners, but also by scientists themselves and the public at large. (Brante, Fuller & Lynch, 1993, p.ix)

Bruno Latour and Stephen Woolgar, the Kuhn-influenced sociologists of scientific knowledge, wrote in their influential *Laboratory Life*[15]:

> The "out-there-ness" [of the external world] is the consequence of scientific work rather than it cause ... 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. (Latour & Woolgar 1986, pp.182, 184)

[13] For Shimony, see Myrvold & Christian (2009); for Bunge, see Matthews (2019).

[14] She is co-author of *Merchants of Doubt: How a Handful of Scientists Obscured the Truth on Issues from Tobacco Smoke to Global Warming* (Oreskes & Conway 2010) and currently a regular contributor to *Scientific American*.

[15] The book is widely, and approvingly, cited in science education. Peter Slezak provides a withering, but rarely cited, critique (1994b).

Glen Aikenhead, a leading Canadian science educator, informed readers that contemporary social studies of science (STS), reveal science as:

> mechanistic, materialist, reductionist, empirical, rational, decontextualized, mathematically idealized, communal, ideological, masculine, elitist, competitive, exploitive, impersonal, and violent. (Aikenhead 1997, p.220)

Obvious questions of: Why study science? Why trust science? Has science produced knowledge? How has science been so successful in identifying and dealing with disease?were passed over.

Two cultural studies researchers in education assert:

> Recent scholarship in science studies [STS] has opened the way for more thoughtful science education discourses that consider critical, historical, political, and sociocultural views of scientific knowledge and practice ldots Increased attention to the problematic nature of western science's claims to objectivity and universal truth has created an educational space where taken-for-granted meanings are increasingly challenged, enriched, and rejected …Thus, science's long accepted claim to epistemological superiority has now become bound to the consideration of cultural codes, social interests, and economic imperatives. (Bazzul & Sykes 2011, p.268)

Although the power-house social-constructivist Edinburgh Programme was convincingly criticised by many (Bunge 1991, 1992, Slezak 1994 a,b) confidence about universal science was universally diminished. Relativism and agnosticism concerning knowledge of the natural, social, cultural, and moral worlds became the academic and public, norm. And, depressingly, the science education norm[16].

Of course, Kuhn is more cited than read; the mere citation of Kuhn is considered to constitute an argument, or to provide evidence, for some philosophical view. Marilyn Fleer, a professor at Monash University, writes:

> In recent years, the rational foundations of Western science and the self-perpetuating belief in the scientific method have come into question …The notion of finding a truth for reality is highly questionable. (Fleer 1999, p.119)

Typically, no evidence is adduced for this sweeping claim except an unpaginated reference to Kuhn. The practice of having an unpaginated Kuhn citation substitute for evidence, or argument, is widespread. It became the disciplinary norm in science education. Merely putting the name 'Kuhn' in brackets after some claim was, and in places still is, regarded as sufficient warrant for making the claim, no matter how outrageous and ill-supported it might be. And, importantly, no attention is paid to the arguments of numerous others that refute the claim.

Conclusion

Kuhn deserves full praise for recognising the centrality of education in the growth of science, for investigating the cognitive processes involved in the learning of scientific concepts and, of course, for so powerfully putting philosophy of science into

[16] The unhealthy reach of relativism and idealism in science education is described in (Matthews 2015, chap.8, 2021, chap.7)

[17] There have been hundreds, just in English, of substantial philosophical books and anthologies devoted to Kuhn and Kuhnian themes. Many praising, many condemning.

the academic and public domains[17]. As claimed by David Treagust, and cited at the beginning of this Opinion Piece, for academics and the public, Kuhn's *Structure* 'really changed the way we look at the enterprise that is science'.

But, as usual, along with the upside there was a downside. Kuhn admitted in 1997 that his treatment of the orthodox philosophical tradition was 'irresponsible' (Conant & Haugeland 2000, p.305). And elsewhere he confessed: 'I should never have written the purple passages'. And he is surprised at their impact:

> 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. (Kuhn 1993, p.314)

Dismay? Did Kuhn not read his own text? Did he not take his writing seriously? At the risk of introducing a purple passage: Should Donald Trump have been surprised that his followers stormed the US Capital after his January 6 speech on the White House Ellipse?

Kuhn's is a too easy a *mea culpa*: A philosopher writing a purple passage is akin to a mechanic not putting oil in a serviced car: for both, it is a culpable error. The mechanic has to pay for the damage done; unfortunately, the philosopher does not pay for the damage done to students, schoolteachers, faculty and the public who uncritically read their 'irresponsible' text.

Philosophers cannot be entirely responsible for their followers, and Kuhn did disown much of the relativism, idealism and subjectivism that was being promoted in his name (Kuhn 1991/2000). Nevertheless, given the deleterious impact of Kuhn-inspired philosophy in educational constructivism, in STS studies, in fuelling post-modernism, and in important debates about inclusion of indigenous science in science programmes——he should have been more considered, restrained, and careful in his writing. A more orthodox philosophical 'cast of mind' would have done no harm and, doubtless, would have done good. Though there would have been fewer sales of Structure and not as many translations made.

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