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ARTICLE

Thomas Kuhn and Science Education

Learning from the Past and the Importance of History and Philosophy of Science

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Abstract
Beginning 60 years ago, Thomas Kuhn has had a significant impact across the academy and on culture more widely. And he had a great impact on science education research, theorising, and pedagogy. For the majority of educators, the second edition (1970) of his *Structure of Scientific Revolutions* (Kuhn, 1970a) articulated the very nature of the science, the discipline they were teaching. More particularly, Kuhn's book directly influenced four burgeoning research fields in science education: Children's Conceptual Change, Constructivism, Science-Technology-Society studies, and Cultural Studies of Science Education. This paper looks back to the Kuhnian years in science education and to the long shadow they cast. The discipline of science education needs to learn from its past so that comparable mistakes might be averted in the future. Kuhn's influence was good and bad. Good, that he brought HPS to so many; bad, that, on key points, his account of science was flawed. This paper will document the book's two fundamental errors: namely, its Kantian-influenced ontological idealism and its claims of incommensurability between competing paradigms. Both had significant flow-on effects. Although the book had many positive features, this paper will document how most of these ideas and insights were well established in HPS literature at the time of its 1962 publication. Kuhn was not trained in philosophy, he was not part of the HPS tradition, and to the detriment of all, he did not engage with it. This matters, because before publication he could have abandoned, modified, or refined much of his 'revolutionary' text. Something that he subsequently did, but this amounted to closing the gate after the horse had bolted. In particular, the education horse had well and truly bolted. While educators were rushing to adopt Kuhn, many philosophers, historians, and sociologists were rejecting him. Kuhn did modify and 'walk back' many of the head-turning, but erroneous, claims of *Structure*. But his retreat went largely unnoticed in education, and so the original, deeply flawed *Structure* affected the four above-mentioned central research fields. The most important lesson to be learnt from science education's uncritical embrace of Kuhn and Kuhnianism is that the problems arose not from personal inadequacies; individuals are not to blame. There was a systematic, disciplinary deficiency. This needs to be addressed by raising the level of philosophical competence in the discipline, beginning with the inclusion of HPS in teacher education and graduate programmes.

Keywords Thomas Kuhn · Relativism · Idealism · Constructivism · Philosophy

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1 Introduction

Thomas Kuhn's impact on science education has been immense. This is reflected in the opening sentence of a 2022 article by David Treagust, one of Australia's foremost science educators:[1]

> 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)

Treagust spoke for the science education research community. In Peter Fensham's landmark study of the discipline of science education, leading science educators repeatedly identify Kuhn's *Structure* as the main influence on their understanding of science (Fensham, 2004). Richard Duschl, in 1990, described *Structure* as 'the most acclaimed book in history of science' (Duschl, 1990, p.36).[2]

The immediate educational impact of *Structure* was on educators' understanding of the nature of science (NOS) which is, increasingly, a stand-alone inclusion in worldwide national and provincial science curricula.[3] An explicit or implicit view of NOS informs curricular decisions, pedagogical practices, and wider national policies about the extent, duration, and funding of science in schools and beyond. It is widely agreed that students learning science should learn what science is, how it works, and what it accomplished. In brief, they should learn the nature of science. This is a truism, but its implication, namely, that science students and teachers need to learn the history and philosophy of science (HPS), has been less recognised and followed through. The seldom faced question is: How is it possible to learn about NOS without knowledge of HPS?

Many educators who, after the publication of the second edition of *Structure* in 1970, did look for history and philosophy of science, saw just Kuhnianism, more particularly Kuhn-inspired constructivism. It is important to distinguish Kuhn's understanding of NOS from the many versions of Kuhn-inspired NOS. At many points, Kuhn was at pains to separate his views from those being advanced in his name. 'Kuhnianism' is an appropriate inclusive label for the latter. It includes Kuhn's genuine views but also those mistakenly advanced in his name. In education, Kuhnian NOS not only informed, but it also underpinned Science-Technology-Society (STS) curricula and Cultural Studies in Science Education research and framed the narrative about inclusion of indigenous science within school science programmes.

Kuhn's *Structure* not only dealt with revolutions in the history of science but also painted a revolutionary account of the nature and practice of science. But did Kuhn give a correct account? Many in HPS thought he did not. For the million-plus who bought the

[1] David Treagust has 100+publications, 36,000 citations, and has supervised scores of doctoral theses. He is not a minor or peripheral figure in international science education. His quoted words indicate how normalised Kuhn's 'picture of science' has become among educators.

[2] A collection of educational responses to Kuhn, and associated philosophical, educational, and psychological issues, can be seen in Matthews (2000b).

[3] For an overview of the history and rationales of NOS in science education, see McComas and Clough (2020) and contributions to Flick and Lederman (2004) and McComas (2020). Also references in Khishfe (2022).

book,[4] reading *Structure* was a Rorschach test. Kuhn recognised this. In 1969, at a large Frederick Suppe-organised symposium on 'The Structure of Scientific Theories' Kuhn, reflecting on the reception of the first edition, wrote:

> I have sometimes found it hard to believe that all parties to the discussion had been engaged with the same volume. Part of the reason for its success is, I regretfully conclude, that it can be too nearly all things to all people. (Kuhn, 1977c, p.459)

He regretted the book's 'excessive plasticity', particularly blaming his casual introduction of the term 'paradigm' (ibid). He should, more to the point, have regretted writing unclear and sloppy sentences about vital subjects. Philosophers, above all, need be cognisant of the importance of clarity in writing.

The argument of this paper is that the most important lesson to be learnt from the problems of science education's wholesale embrace of Kuhnianism is the pressing need to raise the level of philosophical competence in the discipline, beginning with the inclusion of both history and philosophy of science, and philosophy of education, in science-teacher education programmes, and in education doctoral programmes.

2 Kuhn's Status

It is oft claimed that Thomas Kuhn was the twentieth century's most influential historian of science. This can hardly be disputed. He was a Harvard-trained physicist[5] who published a great deal over a 55-year span.[6] However, his global reputation was based upon one book—The Structure of Scientific Revolutions—which was first published in 1962 as a monograph in a little-read Vienna Circle-inspired *International Encyclopedia of Unified Science*,[7] and then, eight years later, republished as a stand-alone second edition by University of Chicago Press (Kuhn, 1970a). The second edition precipitated the Kuhnian tsunami. *Structure* was quickly translated into two-dozen 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. In the USA, the *Social Science Citation Index* listed 4970 Kuhn citations in just the decade 1971–1981 (Brush, 2000, p.54). Doubtless, it held much the same position in comparable indexes in other countries, both English-speaking

[4] *Structure* sold over a million copies in 24+languages. And they were just the authorized, or recorded, sales. So, talk of 'millions of readers' is not hyperbolic or rhetorical. And if the Edinburgh sociologists misinterpreted Kuhn, assuredly others also did.

[5] Kuhn was awarded physics degrees at Harvard: B.S. (1943), M.A. (1946), and Ph.D. (1949). His dissertation thesis was 'The Cohesive Energy of Monovalent Metals as a Function of Their Quantum Defects', supervised by J.H. Van Vleck, and subsequently a Nobel laureate. He quickly published two thesis-related papers in *Physical Review*.

[6] A 2000 listing of his publications, beginning in 1945, runs to ten pages (Conant & Haugeland 2000, pp.325–335).

[7] The encyclopedia was founded in 1937 by scientifically minded, left-wing, US and European émigré philosophers and published by University of Chicago Press. From the beginning, like its Vienna Circle forebears, it was enveloped in political and philosophical controversy. Two days after the commencement of WWII, James Conant, the Harvard President, hosted the group's conference at the university. He shared the group's conviction that science was central to a free and democratic society and to this end, he did his utmost in the following decades to advance both science and science education. See Reisch (2019, chap.2) and Rudolph (2002).

and otherwise. Google Scholar, in October 2022, listed 71 versions of the book, having 143,303 citations.

For a combination of philosophical, sociological, and cultural reasons, the book had stratospheric sales and influence; sales way beyond that of almost any other HPS book published in the twentieth century; and probably beyond all HPS books. David Hull observed: 'All the wrong people seemed attracted to his book for all the wrong reasons' (Hull, 1988, p.112). There have been efforts to give a sociological or naturalist account of the explosion of Kuhnianism: Why was the book such a huge publishing success, and why did the constellation of ideas it contained spread so widely and quickly while its components had sat in isolation for decades?[8]

Some regard Kuhn as 'the most influential philosopher writing in English since 1950, even the most influential academic' (Sharrock & Read, 2002).[9] For others, he is 'one of the historically most significant philosophers of the twentieth century' (Bird, 2000, p.vii). Kuhn's vocabulary ('paradigm', 'paradigm shift', incommensurability', 'gestalt') and thought-to-be Kuhnian ideas (scepticism, relativism, subjectivism, science as power play) have become a part of everyday culture. As recently as 8 October 2022, a correspondent writing on a local political issue, in *The Age*, a major Australian newspaper, confidently related that:

> Thomas Kuhn's *The Structure of Scientific Revolutions* … exposed the influence of inertia, interests and the irrational on scientific explanation and understanding. (p.34)

And this is 60 years after the book's publication. Countless readers of the newspaper would be nodding their heads. Most of them not asking: What is the extent and degree of such influences? Were the influences determinate, or otherwise, of scientific explanations and understandings? Were they were brought to light and corrected? Science educators also nodded their heads, and very few followed through with the obvious questions. Gerald Doppelt gave an accurate, and neutral, account of Kuhn's impact:

> Putting the merits of Kuhn's philosophical claims to one side, it is still undeniable that his work has reshaped the terms of debate, and much research, in philosophy. In short, his work had given a new centrality and relevance to the history of science, and the examination of specific scientific practices, for philosophers. (Doppelt, 2001, p.160)

This paper does not put Kuhn's philosophical merits to one side: It appraises them, recognising the positives, but detailing its key demerits and their deleterious educational, and cultural, influence.

There are many informative accounts of Kuhn's personal, educational, and academic life.[10] And, of course, there are many more accounts of his achievements, real and contested. Separating Kuhn's real from his imagined achievements has, for 60 years, engaged legions of scholars.[11] In 2012, the 50-year anniversary of publication of *Structure* was

8 For some explanations, see Fuller (2000b) and Reisch (2019, chap.16).

9 Gary Gutting's 1980 book has a partial bibliography that lists, by academic discipline—philosophy, history of science, sociology of science, sociology, political science, economics, psychology, history, theology, art, literature, and education—320 works on Kuhn: (Gutting 1980, pp.321–339).

10 See at least the Introduction to Nickles (2003) and the interview with Kuhn in Baltas et al. (1997) reproduced in Conant and Haugeland (2000, pp.255–321).

11 Just in English, there have been hundreds of substantial philosophical books and anthologies devoted to Kuhn and Kuhnian themes. Many praising and explicating him, others criticising.

marked by numerous celebratory conferences in many countries.[12] A large international centenary conference celebration of Kuhn's life and work, held in July 2022, is witness to his enduring interest among historians, philosophers, and other scholars.[13]

Kuhn's philosophy, his account of the nature of science, and the conditions for, and mechanisms of, theory change in science have been exhaustively appraised by historians and philosophers.[14] Richard Duschl and Richard Grandy are of the opinion:

> The most recent movements in philosophy of science can be seen as filling in some of the gaps left by Kuhn's demolition of the basic tenets of logical positivism. (Duschl & Grandy, 2008, p.8)

This is an orthodox, majority account of Kuhn's influence. And it is generous. An alternative reading would be to describe recent movements as 'correcting mistakes in' or 'clarifying ambiguity about', rather than 'filling in gaps', and further, pointing to how much he, and more generally Kuhnians, shared, rather than demolished, some of the basic tenets of logical positivism.[15]

3 Different Structures: Ernest Nagel's *Structure* (1961) and Thomas Kuhn's *Structure* (1962)

In successive years, two HPS books part-titled *Structure* were published. Ernest Nagel's (1901–1985) 600+page *The Structure of Science: Problems in the Logic of Scientific Explanation* was published in 1961 (Nagel, 1961). Kuhn's 170+page *The Structure of Scientific Revolutions* was published in 1962. Beginning with his choice of title, Kuhn took aim at Nagel's book and largely displaced it from academic discussion. Kuhn's *Structure* opened a new chapter in the history of HPS. It sold a million-plus copies in at least 18 languages; Nagel's sold the smallest fraction of that in a handful of languages. Kuhn sold to the masses and Nagel to captive philosophy students, including the current author. The Nagel/Kuhn contrast is an informative background for the arguments of this paper.

Nagel's was a widely adopted philosophy text giving a detailed exposition of the logical empiricist 'picture' of science. This encapsulated the dominant philosophical, cultural, and educational understanding of science of the era.[16] The book was the received view's manual. Its Preface encapsulates post-war, progressive society's hopes for science. Nagel

[12] Proceedings of some major English celebratory conferences are Devlin and Bokulich (2015), Kindi and Arabatzis (2012), and Richards and Daston (2016). In reviewing the published proceedings of one such conference, the philosopher Howard Sankey commented 'Still, I am not prepared to ignore the critical objections of an earlier generation of commentators on Kuhn. … the inclusion of dissonant voices would have strengthened the collection and provided a more balanced treatment of Kuhn' (Sankey 2014, p.47).

[13] Kuhn was born in 1922 and *Structure* was published in 1962. So, the conference was both a centenary and a 60-year celebration. The conference was jointly sponsored by the Aristotelian Society and the British Society for the Philosophy of Science. There were, disappointingly, no presentations on Kuhn and science education.

[14] See major studies of Kuhn by Hoyningen-Huene (1993) and Bird (2000, 2013), and contributions to Gattei (2003), Horwich (1993), Mizrahi (2017), and Nickles (2003). And contributions to the celebratory conferences mentioned above.

[15] See minimally Matthews (1992, 1993, 1994 chap.7).

[16] Variously labelled the 'Standard View' (Grandy 2003) or the 'Received View' (Suppe 1977).

speaks of science as an 'institutionalized art of inquiry' that has yielded precious fruit. Foremost among these are:

> The achievement of generalized theoretical knowledge concerning fundamental determining conditions for the occurrence of various types of events and processes; the emancipation of men's minds from ancient superstitions in which barbarous practices and oppressive fears are often rooted. (Nagel, 1961, p. vii)

He proceeds down a long list of social and cultural achievements, concluding:

> Despite the brevity of this partial list, it suffices to make evident how much the scientific enterprise has contributed to the articulation as well as the realization of aspirations generally associated with the idea of a liberal civilization. (ibid)

This was a restatement of core Enlightenment convictions. The book was to be the first of three volumes laying out Nagel's philosophical reflections on science.[17] He rightly observed that: 'there are few notable figures in the history of Western philosophy who have not given serious thought to problems raised by the sciences of their day' (Nagel, 1961, p. viii).[18] He recognises there are many problems occasioned by science that warrant philosophical attention, but:

> … the present book is controlled by the objective of analyzing the logic of scientific inquiry and the logical structure of its intellectual products. (Nagel, 1961, p.viii)

Nagel's *Structure* had a very low profile, if any, in science education. However, the discipline enthusiastically embraced Thomas Kuhn, especially after publication of the 1970s edition of *Structure*. But, as shown in the following sections, Kuhnianism was more embraced than appraised by educators. This indicates a fundamental deficiency in the discipline: The failure to incorporate history and philosophy of science into teacher education or graduate programmes.

4 Unheralded Birth of Kuhn's *Structure*

The first edition of *Structure* appeared in 1962 in the Vienna Circle-inspired, *International Encyclopedia of Unified Science* (Volume 2 Number 2). It had been founded by Otto Neurath and subsequently edited in the USA by Rudolf Carnap and Charles Morris. The *Encyclopedia* was the post-war flagship of logical empiricism; it had an almost entirely philosophical readership. The first edition was hardly noticed outside of philosophy departments, and not at all outside the academy.

An exception to the general neglect was a famed panel discussion titled 'Criticism and the Growth of Knowledge' at the July 1965, London International Colloquium for the Philosophy of Science. The contributors were Thomas Kuhn, John Watkins, Stephen Toulmin, L. Pearce Williams, Karl Popper, Margaret Masterman, Imre Lakatos, and Paul Feyerabend. This was a sort of 'HPS fights back' event. The papers were published as a book in 1970, and enjoyed huge sales, becoming a basic text in upper-level philosophy of science

[17] For then-contemporary appraisals of Nagel's work, see contributions to Morgenbesser et al. (1969). For current appraisals of his work, see contributions to Neuber and Tuboly (2022).

[18] This point is developed at length, with texts, in Matthews (1989).

courses (Lakatos & Musgrave, 1970). It was published the same year as the second edition of *Structure*.

The first edition was not noticed by science educators. John Robinson's *The Nature of Science and Science Teaching* (Robinson, 1968) was the first book whose title brought 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 enrolments 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)

Kuhn does not appear in the anthology's 430 pages, and 60 readings. In 1974, Michael Connelly commented 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 (1934–2012) observed that science education during the 1950s, and leading up to the Sputnik era, 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 two important books—Kuhn's *Structure* and Joseph Schwab's *The Teaching of Science* (Schwab, 1960)—were published at the same time yet shared 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.

Michael Martin (1932–2015), a Boston University philosopher, a few years later surveyed the same literature as Elkana. He paid particular attention to the rush of 'inquiry' and 'discovery' curricula and recommendations put into Western educational orbit by Sputnik. Martin drew attention to the important 1966 Educational Policies Commission report, *Education and the Spirit of Science* (EPC, 1966), and charted the myriad ways in which it, and other curricula as well, reproduced simplistic, mistaken, 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.

Much had happened in HPS in the decade leading up to the EPC report. 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). Science educators, especially those at the highest levels advising Federal Government curricular and education-funding bodies, should have had an inkling of the shifting ground in HPS and should have recognised its relevance to its report on the 'Spirit of Science'.

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 era's unfortunate divide 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).[19]

5 Embrace of the Second Edition

The second edition (1970), unchanged except for addition of a Postscript,[20] took Kuhn to the world. Kuhn was enthusiastically taken into education and into nigh on all other academic disciplines. In 1985, Derek Hodson reported that of 22 research articles published, and theses submitted, in the field of 'Philosophy of Science, Science and Science Education', 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 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 (12 articles), 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).

Clearly, there were many Kuhn enthusiasts in the science education community. It was close to being a Kuhnian cheer-squad (Matthews 2004a).

The embrace of Kuhn is demonstrated 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:

[19] Some of this history of separate development is discussed in Matthews (1994, chap.2). An exception to 'silo' research in philosophy and education was the work of Harvey Siegel (1978, 1979, 1985, 1989).

[20] He writes in the second edition's Postscript: 'For this edition I have attempted no systematic rewriting, restricting alterations to a few typographical errors' (Kuhn 1970a, p.174).

> 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)

What they are identifying as Kuhn's 'dialectic epistemology with distinctly relativistic implications' is Kuhn's embryonic and unsophisticated, Kantianism, something he picked up in a Harvard General Education course.

6 *Structure*: An Outline

As it was Kuhn's *Structure*, and almost *Structure* alone, that impacted science education, that book will be the focus of this paper's argument. More particularly, the book's anti-realist and incommensurability claims will be examined as these had the most enduring impact in science education. They are still, for example, centre stage in important debates in Canada, Australia, New Zealand, and elsewhere, about whether indigenous science should be included in a science programme or a social science programme.[21]

Key elements of *Structure* can be seen in Kuhn's, 1951 Lowell Lectures delivered in the Boston Public Library—*The Quest for Physical Theory*. These 'adult education' or 'university outreach' lectures remained unpublished until 2021 when they were edited by George Reisch and published by the MIT Library (Kuhn, 1951/2021). In criticism of Karl Pearson's popular empiricist account of science which embodied the 'orthodox' logical empiricist view of the time (Pearson, 1892/1937), Kuhn averred:

> I should like to suggest that the impartial, dispassionate observation of nature is impossible, that there are no "pure facts" from which alone valid theories can be derived, and that the effort toward "self elimination" which Pearson proposes as the scientist's goal, would, in practice, result in the abolition of productive research. (Kuhn, 1951/2021, p.3)

The core argument of *Structure* is well known and could be summarised as follows.[22] All communities seek knowledge and understanding of nature. This can amount to pre-science, pseudoscience, protoscience, or science. *Normal* science is heralded by the appearance of a common *paradigm*, or exemplar, that dictates methods of inquiry and constrains the kind of entities, or *ontology*, that can be appealed to in scientific explanations. The paradigm provides theoretical and practical puzzles that scientists work away at solving. When some pressing practical or theoretical puzzles resist solution with tools and concepts provided by the paradigm, then science enters a *crisis* period, and either drastic renovation is done (maybe new rooms added or boarders taken in) or scientists embrace a new paradigm (move house). This is *revolutionary* science. Crucially, while decisions *within* a paradigm are rule regulated, as evidenced in processes of journal review, decisions about how extensive a renovation should be—whether a particular addition is permissible, or whether to pick up and move house—are *not* rule regulated.

[21] On the use of Kuhnian arguments in debate about inclusion of indigenous knowledge in science curricula, see Matthews (2022b).

[22] Succinct overviews are in Bird (2012, 2013).

For Kuhn, values and interests, including personal ones, determine those decisions; they are an unavoidable part of science; and values are embedded within paradigms. Productive paradigms, and theories within them, unavoidably embody standards (values) of accuracy, consistency, breadth, simplicity, and fruitfulness. These are qualities that scientists, with good reason, prefer for their theories (McMullin 2008). And these values are common across paradigms, even incommensurable ones. As Kuhn later elaborates:

> such values as accuracy, scope, and fruitfulness are permanent attributes of science …. (Kuhn, 1977b, p.335)

But he advises that:

> the relative weights attached to them have varied markedly with time and also with the field of application. (Kuhn, 1977b, p.335)

So subjectivity is built into science.

6.1 Purple Passages

In the first edition, and remaining unchanged in the second, there were many 'purple passages' as Kuhn later labelled them (Kuhn, 1970a). These were head-turning claims that threatened the orthodox logical empiricist, and widespread cultural, understanding of science, understandings that were codified by Nagel:

- '…once current views of nature were, as a whole, neither less scientific nor more the product of human idiosyncrasy, than those current today' (p.2).
- viewing all fields together science 'is a rather ramshackle structure with little coherence between its various parts' (p.49).
- 'Like the choice between competing political institutions, that between competing paradigms proves to be a choice between incompatible modes of community life' (p.94).
- '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' and 'it could be reversed' (p.108).
- 'I have so far argued that paradigms are constitutive of science. Now I wish to display a sense in which they are constitutive of nature as well' (p.110).
- 'after discovering oxygen Lavoisier worked in a different world' (p.118).
- transition to a new paradigm occurs 'not by deliberation and interpretation, but by a relatively sudden and unstructured event like a gestalt switch' (p.122).
- 'the competition between paradigms is not the sort of battle that can be resolved by proofs' (p.148), and … 'in these matters neither proof nor error is at issue'.
- 'the proponents of different paradigms practice their trades in different worlds … the two groups of scientists see different things when they look from the same point in the same direction' (p.150).
- 'The transfer of allegiance from paradigm to paradigm is a conversion experience that cannot be forced' (p.151).
- 'a 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).
- 'the phrases "scientific progress" and even "scientific objectivity" may come to seem in part redundant' (p.162).

- 'with respect to normal science … progress lies simply in the eye of the beholder' (p.163).
- '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.170).

And so on and on.

Kuhn would later regret writing some of these passages, but he held on to others. In 1969, he wrote: 'I now recognize aspects of its initial formulation that create gratuitous difficulties and misunderstandings' (Kuhn, 1970a, p.174). Steven Shapin generously called such passages 'Kuhn's sound-bites' (Shapin, 2015, p.11). He could equally have called them 'ill thought out', 'careless', or 'irresponsible' claims. In 1969, in the Postscript to *Structure*, Kuhn wrote 'I now recognize aspects of its initial formulation that create gratuitous difficulties and misunderstandings' (Kuhn, 1970a, p.174). A year later, in distancing himself from charges of 'irrationality', 'mob rule', and 'relativism', with which he had rightly been charged, Kuhn admitted that his 'own past rhetoric is doubtless partially responsible' (Kuhn, 1970b, p.260).

Kuhn does not regret his lack of training in philosophy; on the contrary, he thought the lack advantageous as it did not give him a certain, but not spelt out, philosophers' 'cast of mind'. But if the above passages had been written into a graduate, or even undergraduate, philosophy thesis in most decent programmes, they would have been struck out: 'this work cannot be presented, take it away and clarify'. Such guidance in 1960 would have made a seismic difference to subsequent philosophical and cultural thought.

7 Anticipations of *Structure*

The central philosophical ideas in *Structure* were not novel, something Kuhn often acknowledged.[23] Many elements of his philosophy of science were extant; indeed, many elements were in residence just along the Harvard corridors. But these were barely, if at all, acknowledged, much less engaged with when the first edition of *Structure* was published in 1962. The intellectual ground for the Kuhnian 'revolution' had been well prepared. Simple empiricist, individualist, logical positivist understandings of science—the knower confronting the world—had been challenged on many fronts; the time was ripe for a philosophical upheaval, if not revolution.[24]

Marx's 1852 *Eighteenth Brumaire of Louis Bonaparte* could have been, and by a few was, appealed to by opponents 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)

[23] See Kuhn's repeated acknowledgements of scholars-of-influence in his autobiographical interview (Baltas et al., 1997).

[24] On the development of logical empiricism, see Giere and Richardson (1996); for appraisals, see contributions to Parrini et al. (2003).

This is an early statement of the sociology of knowledge, and was acknowledged as such by Karl Mannheim, the founder of that discipline (Mannheim, 1936/1960). It denies all 'Robinson Crusoe', individualist, observer-confronts-the-world epistemologies. It lays out how the 'we think' determines, or at least constrains, the 'I think'. Marx's observation was consistent with Kuhn's epistemological programme and might be reformulated as 'the paradigms of all the dead generations weighs like ….'.

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, Cohen & Schnelle, 1986). 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 1930s and 1940s, Alexandre Koyré extensively documented the centrality of metaphysics in the science of Galileo and Newton (Koyré, 1957, 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).

James Conant (1893–1978), while President of Harvard and the director of Kuhn's own General Education course, had written popular books pointing to 'conceptual schemes' as the skeleton of science, and their transformation as the consequence of 'scientific tactics and strategy' (Conant, 1947, pp.104–111). Kuhn, with enthusiasm and gratitude, embraced Conant's 'conceptual schemes' as a means to elucidate the nature of science and its history (Wray, 2016).[25]

Philipp Frank (1884–1966), a Harvard physics professor and a major exponent of logical empiricism, had been since the 1930s publishing accounts of science that shared a great deal with Kuhnian views. In the opening chapter of his *The Law of Causality and its Limits* (1932), he observes:

> The more a physicist or biologist refuses to concern himself with 'philosophy', respectfully or contemptuously, the more we can be sure that he adopts the views of the oldest traditional scholastic philosophy in good faith, because he has not given careful thought to the fundamental concepts of his science. In the elementary textbooks of purely experimental physics, the most astonishing metaphysical claims can be found. (Frank, 1932/1998, p.25)

This is a constant theme in his work. In the Preface to his *Philosophy of Science*, he says 'the deeper we dig into actual science the more its links with philosophy become obvious' (Frank, 1957, p.iv). Further, he is well aware of the need for concrete historically based analysis, warning that:

> Presentations of this field have very often started from a concept of science that is half vulgar and half mystical. Other presentations have linked science with a philosophy that has actually been a mere system of logical symbols without contact with historical systems of philosophy. (Frank, 1957, p.iv)

The 'historical turn' in philosophy of science did not have to await the Kuhnian revolution. Consequently, Frank's *Philosophy of Science* contains analyses of the scientific and

[25] See Conant's autobiography (Conant 1970). For Conant's direct influence on Kuhn, see Wray (2016) and Reisch (2017, 2019 chap.4).

philosophical contributions of Aristotle, Bacon, Copernicus, Descartes, Einstein, Foucault, and pretty much down the rest of the alphabet to Whewell and Young. In all of his writing, Frank stresses the importance of extant conceptual frameworks for science and relentlessly examines grounds for choosing frameworks and for changing frameworks. Herbert Feigl provides an accurate summary of Frank's philosophical orientation, saying that it:

> combines informal *logical* analysis of the sciences with a vivid awareness of the *psychological* and *socio-cultural* factors operating in the selection of problems, and in the acceptance or rejection of hypotheses, and which contribute to the shaping of styles of scientific theorizing. In a sense this is a genuine sequel to the work of Ernst Mach. (Feigl, 1956, pp.4-5)

All of this was grist for the Kuhnian mill, but Frank's name appears in *Structure* only as the biographer of Einstein. Kuhn's failure to engage with the work of such an eminent Harvard colleague is the more surprising as Frank had explicitly written on education. In 1947, he published an article in the *American Journal of Physics* titled: 'The Place of Philosophy of Science in the Curriculum of the Physics Student' (Frank, 1947/2004).

Gerald Holton, a Harvard colleague and fellow contributor to the General Education programme, had in the 1950s written on the role of organising principles, or 'themata', in science (Holton, 1952/1984, 1973/1988).[26] Holton acknowledges Frank as the person who opened his eyes to the intimate connection of physics with philosophy:

> … the interaction between science and epistemology was at the center of attention in discussions over many years with Philipp Frank, that most humanistic and conciliatory of logical empiricists, and biographer of Albert Einstein. (Holton, 1973, p.25)

Holton's *Thematic Origins of Scientific Thought* (Holton, 1952/1984) was published in 1952, eight years before the first edition of *Structure*, but is not mentioned in either the first or second edition. Had Kuhn made clear the commonalities and differences between his paradigms (or disciplinary matrices, as he would relabel them in the 1970 'Postscript')[27]

In response to criticisms of his notion of paradigm, Kuhn changed terminology to 'disciplinary matrix'. A matrix had four components: symbolic mathematised generalisations and formulae; metaphysical interpretations of basic models and analogies; epistemic and non-epistemic values shared by all practitioners; and exemplars showing productive puzzle generation and solutions (Kuhn, 1970a, pp.182–187). These have obvious connections to Holton's themata. Science education would have gained if educators had paid as much attention to *Thematic Origins* as they did to *Structure*.[28]

Willard van Orman Quine (1908–2000), Harvard's most eminent philosopher, in 1951 published his famed 'Two Dogmas of Empiricism' (Quine, 1951). His argument was widely acknowledged to have dissolved the analytic/synthetic distinction so fundamental to the empiricist tradition with which Kuhn was battling. For Quine, theoretical statements never confront experience or experiment in isolation, but always as part of a constellation

[26] Holton travelled much the same early professional path as Kuhn but came to a very different philosophical destination. They were born the same year, 1922, both completed Harvard physics PhDs in 1947, both taught in Conant's General Education programme, and both produced important books arising from those experiences.

[27] and Holton's 'themata', it would have been a valuable contribution to philosophy, and informative for science educators.

[28] On Holton's reminiscences of Kuhn, and the latter's 'struggles' with philosophy, see Holton (2018).

including methodological and metaphysical commitments, and where adjustments can be made elsewhere to reconcile theory and observation.Beliefs were not isolated; they were always part of a web.[29]

Hilary Putnam (1926–2016), yet another distinguished Harvard colleague, recognised Kuhn as 'one of the most ingenious contemporary philosophers and historians of science' (Putnam, 1990, p.123). In 1962, Putnam, in a much-cited paper, engaged with, and criticised, Quine's supposed dissolution of the analytic/synthetic distinction. In doing so, he wrote:

> It has been necessary to consider problems connected with physical science (particularly the definition of 'kinetic energy', and the conceptual problems connected with geometry) in order to bring out the features of the analytic-synthetic distinction that seem to me to be the most important. (Putnam, 1962, p.358)

In defending analyticity as a genuine, realistic, and informative category in the science of energy, Putnam gives, in the publication year of *Structure*, an almost mirror account of a Kuhnian paradigm without using the word:

> If a physicist makes a calculation and gets an empirically wrong answer, he does not suspect that the mathematical principles used in the calculation may have been wrong (assuming that those principles are themselves theorems of mathematics) nor does he suspect that the law 'F = ma' may be wrong. Similarly, he did not frequently suspect before Einstein that the law '$E = 1/2mv^2$' might be wrong or that the Newtonian gravitational laws might be wrong (Newton himself did, however, suspect the latter). These statements, then, have a kind of preferred status. They can be overthrown only if someone incorporates principles incompatible with those statements in a successful conceptual system. (Putnam, 1962, pp.371-372)

Putnam and Kuhn, though in the same Harvard 'corridor', did not, in print, publicly engage. This is more than peculiar as one of Kuhn's most substantial historical studies was his 'Law of Energy Conservation' (Kuhn, 1959b). Kuhn saw himself as a philosopher and, outside Harvard, was widely seen to be one. But in writing *Structure*, he engaged very little with the arguments and analyses of the top-flight philosophers who were literally all around him. Putnam is not even in the index of *The Essential Tension* which was Kuhn's selection of his own HPS studies (Kuhn, 1977b), and he does not appear in the collection of Kuhn's 1970–1993 philosophical essays (Conant & Haugeland, 2000).

The foregoing amounted to Kuhn's missed local opportunities for philosophical engagement.[30] But sympathetic philosophers had been writing in many other places on topics germane to *Structure*. Stephen Toulmin (1922–2009) 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 when doing philosophy of science (Toulmin, 1953). A little later, Norwood Russell Hanson (1927–1964) famously wrote on the theory dependence of observation and on the contested nature of the facts in scientific disputes (Hanson, 1958). In Hanson's work, two of the foundations of logical empiricism were cut adrift. Ludwig Wittgenstein had made philosophers attentive to the distinctions between seeing and seeing as; between looking and noticing; and between object perception and propositional perception.

[29] Hence, his later co-authored book, *The Web of Belief* (Quine & Ullian 1970).

[30] There might well have been, of course, private corridor or common-room engagement, but if so, none of it made it beyond the Harvard Yard.

The last requires language and judgement and hence varying degrees of theory (Wittgenstein, 1958).

Michael Polanyi (1891–1976), in his book *Personal Knowledge* (Polanyi, 1958) and elsewhere, wrote on the place of tacit, assumed, 'subterranean' 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. In 1961, in a conference commentary on a pre-*Structure* paper of Kuhn's, Polanyi wrote:

> A commitment to a paradigm has thus a function hardly distinguishable from that which I have ascribed (Polyani, 1958, chaps.5,6,7) to a heuristic vision, to a scientific belief, or a scientific conviction. (Polyani, 1963, p.375)

Rom Harré (1927–2019) in the early 1960s wrote on the centrality and necessity of general conceptual schemes in science, their impacts, how they were modified, and on the ubiquity of metaphor and metaphysic in science (Harré, 1960, 1964). He recognised, as most did, that:

> Empirical investigations do not exhaust the kinds of question raised by a conceptual crisis. There are also questions of a more metaphysical kind. Empirical investigations are not immediately relevant to these, nor do empirical facts provide conclusive answers. (Harré, 1964, p.65)

Max Black (1909–1988) had written extensively on the role of metaphor and analogy in scientific thinking and had rejected the notion of language as a mirror of nature (Black, 1962). Mary Hesse (1924–2016), a historically informed philosopher, argued in different publications that in neither science nor history of science are there 'bare uninterpreted facts; all facts, whether experimental or historical, are interpreted in the light of some theory' (Hesse, 1961 p.v).

Kuhn's failure to engage with Rudolf Carnap's (1891–1970) philosophy is especially noteworthy.[31] In the mid-late 1950s, while Kuhn was teaching in the General Education Programme and when *Structure* was being penned, Carnap was at the height of his powers and academic standing (Schilpp, 1963). He was a doyen of logical empiricism, the 'system' that Kuhn's work was consciously directed against. He was co-editor of the *Encyclopedia of Unified Science* that commissioned Kuhn's *Structure* essay. In that editor's capacity, Carnap wrote to Kuhn in 1960 and again in 1962 commending the manuscript, and saying in 1962 when accepting the manuscript for publication, that:

> In my own work on inductive logic in recent years I have come to a similar idea: that my work and that of a few friends in the step for step solution of problems should not be regarded as leading to "the ideal system", but rather as a step for step improvement of an instrument. (Reisch, 1991, p.267)

And Carnap addressed in 1963 (originally in German in 1935) 'revolutionary' change in science when theories finally cannot be reconciled with observation. He writes that the scientist has two options:

[31] Kuhn's connections with Carnap, and the overlooked similarities of their positions, are laid out by Friedman (2002), Irzik and Grunberg (1995), Reisch (1991), and Uebel (2011). This has been called a 'revisionist' reading of the Carnap-Kuhn relation.

> a change in the language, and a mere change in or addition of, a truth-value ascribed to an indeterminate statement.... A change of the first kind constitutes a radical alteration, sometimes a revolution, and it occurs only at certain historically decisive points in the development of science. (Reisch, 1991, p.270)

Michael Friedman, in his article 'Kant, Kuhn and the Rationality of Science', wrote on the unexpected comparability of Carnap's and Kuhn's account of science:

> Thus Kuhn's central distinction between change of paradigm or revolutionary science, on the one side, and normal science, on the other, closely parallels the Carnapian distinction between change of language or linguistic framework and rule-governed operations carried out within such a framework. (Friedman, 2002, p.181)

John Earman, writing as 'a distant student of Carnap and a close student of Kuhn' (Earman, 1993, p.32), says:

> Although I am no apologist for logical positivism, it does seem to me that many of the themes of the so-called postpositivist philosophy of science are extensions of ideas found in the writings of Carnap and other leading logical positivists and logical empiricists. (Earman, 1993, p.9)

For Carnap, the choice of a particular linguistic framework is not driven by rules; but moves within the framework are so driven or governed. This is close to Kuhn's choosing between paradigms, and working within paradigms. But for science, nature comes into the picture. Here was common ground that could have been explored with Kuhn's own academic sponsor, but this did not happen.[32]

In Kuhn's biographical interview, he confesses that he:

> read a little bit of Carnap, but not the Carnap that people later point to as the stuff that has real parallels to me. … I have confessed to a good deal of embarrassment about the fact that I didn't know it. (Baltas et al., 1997/2000, p.305-306)

Disarmingly, he proceeds to say:

> On the other hand, it is also the case that if I'd known about it, if I'd been into that literature at that level, I probably would never have written *Structure*. (Baltas et al., 1997/2000, p.306)

This is likely a true conjecture, and what a momentous one it is: The post-1960 scholarly and cultural worlds would be very different if Kuhn had spent his time reading, understanding, and engaging with Carnap, and other extant literature, rather than writing *Structure*. His decision to write rather than read had a butterfly effect. An avalanche of publications and book sales followed upon the decision. Other physicists did do the 'hard yards' in philosophy and so they better served the long-term philosophical and educational purposes that Kuhn himself sought to serve, though they did not have his sales or citation figures.[33]

[32] Jonathan Tsou has argued for the deep-seated divergence between their two programmes in philosophy of science. For him, Carnap's being traditional top-down philosophy of science, Kuhn's being new bottom-up, case-study, or particularist philosophy of science (Tsou 2015).

[33] See, for instance, Abner Shimony, whose philosophy PhD was supervised by Carnap (Shimony 1947, 1963/1993, 1977, 1978); Mario Bunge (1959/1979, 1967a, b, 1969, 2016); Robert S. Cohen (1970, 1975); and Gerald Holton (1952/1985, 1973/1988). For appraisal of Shimony, see contributions to Myrvold and Christian (2009); for Bunge, see contributions to Matthews (2019c); for Cohen, see contributions to Gavroglu and Wartofsky (1995).

In brief, by 1960, much had been written on the intellectual complexity of science, on the challenging junction of science and metaphysics,[34] and on the 'embeddedness' of science in society and culture. Much of this had been written on under the heading of 'Internal versus External History of Science' (Basalla, 1968). A good deal of these noteworthy elements of *Structure* had been written about by Kuhn's own Harvard colleagues—Frank, Conant, Quine, Scheffler, Putnam, and Holton. In the first edition (1962), Kuhn recognised few of these antecedents and engaged with only a subset of those. There was some engagement in the Postscript of the second edition (1970), but not much. Engagement with philosophers picked up in his subsequent philosophical writing,[35] but only the smallest proportion of the million-plus readers of *Structure* read his later essays.

Alexander Bird, in his biography of Kuhn, gives an accurate, but understated, account of the degree to which Kuhn's positions had already been voiced:

> There are the seeds of Kuhn's own revolution in such historians and sociologists as Ludwick Fleck, Karl Mannheim and Robert Merton, as well as philosophers such as Toulmin and Hanson. (Bird, 2000, p.2)

To Kuhn's credit, his *Structure* brought all these contra-empiricist elements together within a seemingly coherent narrative that could be easily read, though not always understood, by many.

A good deal of what educators found attractive and engaging in *Structure* was out there and public in earlier books by other philosophers and historians. Unfortunately, there was no tradition, or expectation, that science educators, teachers, or students would read and engage with such literature. Philosophy, or HPS more specifically, was not part of teacher education or seen to be an essential part of a science teacher's professional responsibility. And HPS was not an expected part of an education-researcher's repertoire.

8 Interpreting Kuhn

Before appraising the claims and arguments of an author, the first task is the exegetical one of laying out just what are the author's claims and then determining if they are consistent over time or change with maturation and criticism. For Kuhn, exegesis is uncommonly difficult. He was not a disciplined writer, or at least not as disciplined as his subject matter—the history, processes, methods, and achievements of science—deserved. In a delightfully revealing and lengthy 1995 interview conducted in Athens less than a year before his death, Kuhn relates how he attended an undergraduate seminar at Princeton where, in response to audience enthusiasm for 'Kuhnian' ideas, he objected:

> I kept saying 'But I didn't say that! But I didn't say that! But I didn't say that! (Baltas et al., 1997/2000, p.308)

[34] On metaphysics in science, see at least Agassi (1964), Wartofsky (1968), Amsterdamski (1975), and Dilworth (1996/2006).

[35] See Kuhn's essays in his *The Essential Tension* (Kuhn 1977b) and essays included in *The Road Since Structure* (Conant & Haugeland 2000).

In his 1991 Rothschild Harvard lecture—'The Trouble with the Historical Philosophy of Science'—he said of the hugely popular Edinburgh-based sociology of scientific knowledge programme[36] that it:

> Frequently troubles me, not least because it was initially emphasized and developed by people who often called themselves Kuhnians. I think their viewpoint damagingly mistaken, have been pained to be associated with it, and have for years attributed that association to misunderstanding. (Kuhn, 1991/2000, p.106)

This refrain of 'I did not say that', or 'you have misunderstood me' is common. Kuhn does not pause to reflect on why senior scholars such as David Bloor, Harry Collins, Trevor Pinch, John Henry, and Barry Barnes, to say nothing of multiple hundreds of thousands, if not millions, of lesser scholars and students, could misunderstand what he wrote. He attributes this to their lazy reading, not to his careless writing. Slow, tedious, sometimes page-by-page exegesis is a bugbear, but a necessary one in analysis of Kuhn and Kuhnian argument. Philosophers resented having to do this; they expected clearer, less ambiguous writing.

The deficiency of Kuhn's 'soft-focus', or undisciplined, writing was shown in an early review of *Structure*. Dudley Shapere, who acknowledged the 'vast amount of positive value in Kuhn's book', focused, as so many did, on Kuhn's introduction of 'paradigm'. At the very outset paradigm was defined as:

> a set of "universally recognized scientific achievements that for a time provide model problems and solutions to a community of practitioners". (Kuhn, 1962/1970 p. viii)

Shapere went on to argue that Kuhn's truly revolutionary account of theory changes in the history of science:

> … is made to appear convincing only by inflating the definition of 'paradigm' until that term becomes so vague and ambiguous that it cannot easily be withheld, so general that it cannot easily be applied, so mysterious that it cannot help explain, and so misleading that it is a positive hindrance to the understanding of some central aspects of science; and then, finally, these excesses must be counterbalanced by qualifications that simply contradict them. (Shapere, 1964, p.393)

None of these considerations prevented 'paradigm' appearing, like Spring blossoms (to use a generous simile), across the scholarly, social, and cultural worlds after *Structure*'s publication in 1962. For Kuhn, Hume's 'slow, lingering method' of philosophical analysis was alien. It is something that requires schooling, and Kuhn repeatedly acknowledges that he did not have it.

Margaret Masterman, at the important 1965 London conference session on *Structure*, identified 21, 'or was it 23', different meanings of 'paradigm' in the book's first edition (Masterman, 1970). Kuhn was in the conference audience and, thirty years later, commented:

> And I sat there, I said, my God, if I had talked for an hour and a half, I might have gotten these all in, or I might not have. But she's got it right! And the thing I particularly remember, and I can't make it work quite but it's very deeply to the point: a

[36] For foundation texts, see Barnes (1974, 1977) and Barnes et al. (1996). For evaluations, see contributions to Brown (1984).

paradigm is what you use when the theory isn't there. And she and I interacted then, during the rest of my stay, quite a lot. (Baltas et al., 1997/2000, p.299-300)

Scholars do change, or refine, their positions in response to criticism and upon further reflection. This is commendable and Kuhn did so (Hoyningen-Huen, 2015). In 1993, he wrote:

Whatever I may have believed when I wrote the *Copernican Revolution* [1957], I would not now assume … 'that the simpler, the more beautiful [astronomical] models are more likely to be true'. (Kuhn, 1993, p.331)

He changed his mind on this point simply because he gave up altogether on truth as a realisable goal of science.

Abandoning truth was a seismic change. Less seismic was his discarding of 'paradigm' which soon enough became a mixture of 'lexical structure' plus 'exemplar' (Kuhn, 1993, p.326). This construction did not catch on; it did not have the ring of 'paradigm' which lived on enjoying a life of its own long after its creator abandoned it. Exegesis details where and when change occurs. Further attention, beyond exegesis, might reveal why the change occurs, and whether it was justified.

9 Philosophers on Kuhn

Philosophers did not entirely ignore the first edition of *Structure*. Most notably, and publicly, Paul Feyerabend, Imre Lakatos, Margaret Masterman, Stephen Toulmin, and Karl Popper engaged with it at the 1965 London Philosophy of Science Congress (Lakatos & Musgrave, 1970). Many other philosophers, including those mentioned below, rose to the Kuhnian challenge.

John Searle, a philosopher, points to the need for externalist, sociological, or naturalist, rather than internalist, epistemic explanations for the embrace of Kuhnianism:

… 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)

Kuhn's 'novel' ideas were taken out of the philosophy corridor and broadcast in the marketplace. The Kuhnian wave broke over philosophy departments, and in quick succession of other humanities, social science, and education departments. There have been many substantial monographs and collections on Kuhn's real and imagined philosophy.[37]

Kuhn cites just a handful of philosophers in the first edition: Wittgenstein, Braithwaite, Polanyi, Whewell, Popper, Goodman, Quine, Nagel, and Hanson. And these, with the notable exception of Hanson, are mentioned only in passing. Kuhn regarded *Structure* primarily as a contribution to philosophy. Yet only 10% of its sources are philosophical.[38] There is

[37] See at least Bird (2000), Devlin and Bokulich (2015), Gutting (1980), Horwich (1993), Nickles (2003), and Sharrock and Read (2002),

[38] Brad Wray has documented the citation patterns of *Structure* (Wray 2015).

no prolonged analysis or evaluation of any philosophical argument, excepting a three-page analysis of arguments about perception, and what contributions the observer makes to the object as perceived.

In *Structure*, what philosophical arguments there were amounted to empiricism in new clothes: Theory dependence of observation still took observation as fundamental for a theory of knowledge. But debate about the theory dependence, or otherwise, of observation was just an in-house empiricist family-squabble; it occurred within an empiricist 'paradigm', as some would say, or within an empiricist 'problematic' as others might say. Wallis Suchting (1931–1997) commented:

> 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)

Mario Bunge (1919–2020), a physicist and philosopher who published significant work in both fields (Matthews, 2019c), recounts in his autobiography that in 1966 he attended an influential colloquium on causality convened in Geneva by Jean Piaget. Kuhn, an admirer of Piaget, 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 a harsh judgement, but it was made in 1966 after the first edition of *Structure* (1962), but before publication of the second edition's Postscript (1970) and Kuhn's 'Response to My Critics' in the *Criticism and the Growth of Knowledge* collection (Lakatos & Musgrave, 1970). At the time, Kuhn's most widely known historical work was his book *The Copernican Revolution* (Kuhn, 1957). He acknowledged that this was entirely derivative and put together from secondary sources for the benefit of his Harvard General Education classes.[39]

David Stove (1927–1994), an Australian philosopher, 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 scientist's *regard as* decisive arguments. (Stove, 1982, p.33)

Stove maintained that Kuhn's confusion of sociology with epistemology 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)

This seems an 'over the top' charge, but, in its mitigation, David Bloor, who recognised Kuhn as his philosophical inspiration, published a piece titled: 'The Sociology of Reasons: Or Why "Epistemic Factors" are really "Social Factors"' (Bloor, 1984, 295–324).

[39] The derivative quality of the book was largely the reason the Harvard History department did not offer Kuhn tenure. Subsequently, he did conduct careful and archival studies of the early history of quantum theory (Kuhn 1978).

So logical expressions—'inconsistent with', 'entails', 'subset', 'contradiction', etc.—only function as such in as much as people believe them. The validity of an argument depends on people thinking it is valid. The effect of this is to replace logic by psychology; the latter substitutes for the former. A consequence is that the important psychological study of poor and aberrant reasoning cannot be conducted, as correct reasoning cannot be identified independently of convictions. Separating 'good' psychology from 'mob' psychology is otiose.

Israel Scheffler (1923–2014), 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).

Jan Golinski, a 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 philosophers' views, and when he did so the results were not encouraging. (Bird, 2000, p. ix)

Consequently, Bird stated:

> *Structure* is not primarily a philosophy text. Rather it is a work in what I call 'theoretical history'. (Bird, 2000, p.vii)

Abner Shimony (1928–2015), a Boston University physicist and philosopher with substantial publications in both fields (Myrvold & Christian, 2009), 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)

The 'controlled analysis', to which Shimony refers, is a controlled and competent 'philosophical' analysis.[40]

Wolfgang Stegmüller (1923–1991), an Austrian philosopher, opined that the crux of Kuhn's theory of science was 'a bit of musing by a philosophical incompetent' (Stegmüller, 1976, p.216). This was a harsh judgement, but Kuhn was candid in admitting that he had no training in philosophy and was an 'amateur' in the discipline (Kuhn, 1991/2000, p.106). And, to a point, he thought that having no formal training was advantageous. This because as he was not schooled in 'old thinking', he did not develop a certain 'cast of

[40] Shimony's first PhD was in philosophy at Yale with Rudolf Carnap as his advisor. His second PhD was in physics at Princeton supervised by Eugene Wigner.

mind' that characterised academic philosophy. This is a pity, as this cast of mind traditionally espoused clear and coherent writing; the cast was uncomfortable with 'purple passages' and endeavoured not to compose them.

Michael Devitt agrees with some of Kuhn's epistemology concerning theory-ladenness and revolutionary theory change, but does not think such agreement requires abandoning truth, or even abandoning the correspondence theory of truth. He regards Kuhn's 'semantic and vaguely ontological doctrines as largely, if not entirely, mistaken' (Devitt, 1991, p.155). And says later: 'Constructivism is *prima facie* absurd, a truly bizarre doctrine' (Devitt, 2001, p.147). The 'absurd' doctrine has traction because:

> Constructivists typically vacillate between talk of theories or experience and talk of the world. This vacillation is important to the appeal of their message. For, although it is false that we construct *the world* by imposing concepts on *the world*, it is plausible to suppose that we construct *theories of the world* by imposing concepts on *experience of the world*. The vacillation helps to make the falsehood seem true. (Devitt, 2001, p.148)

It is noteworthy that Kuhn's 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'.[41] 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 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 et al., 1997, p.166)

In the light of the philosophical critiques of the first and second editions of *Structure*, Kuhn did, in a number of important publications, attempt to 'walk back' and refine his claims;[42] hence, the accepted differentiation between Kuhn I and Kuhn II, or between Radical Kuhn and Mild Kuhn. For the most part, educators and social scientists did not attend to this walking back; they were not aware it had happened. After the blinding relativist and idealist flash of *Structure*, few recovered their philosophic vision. Although Kuhn's walk back was applauded by many philosophers, realists thought that he did not walk back far enough. Kuhn's reality was still too dependent upon the views of the scientist and scientific community; his ontology remains wedded to subjectivity.

Kuhn did not become a professional philosopher. When he was denied tenure in the Harvard General Education Department, there was no question that the Philosophy Department, in which Rawls, Quine, Putnam, and others were in residence, would give equal standing to someone untrained in philosophy. After Harvard, he went to University of California, Berkeley, and had appointments and teaching duties in both the History and the

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

[42] See, especially, Kuhn (1970b, 1977c, 1982, 1990, 1991, 1993).

Philosophy departments. At tenure time, the Acting Chancellor called him into his office, 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 et al., 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 et al., 1997, p.182)

Kuhn was deeply hurt, and intellectually troubled, telling interviewers in October 1995, less than a year before his death, that he was an 'anxious neurotic'. In the same interview, he recognised, perhaps with regret, that 'I've never directed a philosophy graduate student' (Baltas et al., 2000, p.319). He admitted in 1995 that his treatment of the orthodox philosophical tradition was 'irresponsible' (Conant & Haugeland, 2000, p.305). This was not a good admission for someone seeking a position in a philosophy department. And elsewhere he confessed: 'I should never have written the purple passages.' He was 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)

And it was not just the 'purple passages' that were irresponsible; at many points, he advanced ill-considered philosophical and historical arguments. For example, he dismissed Joseph Priestley as an 'elderly holdout who had ceased to be a scientist' (Kuhn, 1970a, p.159). This was an assertion that, unjustifiably, blackened Priestley's name for the million-plus readers of *Structure* who themselves who had never, and probably would never, read Priestley.[43] Many other philosophers and historians pointed to problems and errors in Kuhn's account of both normal science and the processes of revolutionary change in science.[44]

10 Kuhn's Reach and Overreach

Kuhn's impact was felt in nearly all disciplines—economics, sociology, psychology, cultural studies, education, and feminism, for starters—and beyond academia into society and culture.[45] But his disciplinary impact was in inverse relation to his training and qualifications; he pressed the right buttons and raised important questions, but he disastrously overreached. From the beginning, his impact on HPS puzzled him and should have puzzled many others.

Kuhn is rightly seen as putting 'paradigm' and 'paradigm change' into the philosophical and social science vocabulary. The word, and expression, is a commonplace in newspaper opinion pieces, political debates, sports reporting, and much else. Its occurrence in

43 On this, see Matthews 2021a, chap.8.

44 An informative review of the critics and their arguments is Sanbonmatsu and Sanbonmatsu (2017).

45 On the impact of Kuhn across academic, and other fields, see 'Introduction' and contributions to Gutting (1980).

the Google Book Ngram Viewer jumped 26-fold between 1960 and 2020.[46] Other now-commonplace words and concepts owe their currency to Kuhn: 'incommensurability' is oft heard in political and religious debate, 'theory dependence' is ubiquitous in social science, 'gestalt switch' is now as common in history of science as it has long been in psychology, 'conversion experience' moved out of Evangelical sermons into history of science debates, 'alternative reality' is not just part of modern US politics but underpins a great deal of educational debate about the teaching of indigenous science, and many other expressions moved out of their Kuhnian home into wider discourse.

There are significant disciplinary and cultural lessons to be learnt from the phenomenon of Kuhnianism. Kuhn had no training in history, philosophy, or sociology; he modestly described himself as an amateur in all three fields. As related in his 1991 Rothschild lecture:

> Though most of my career has been devoted to the history of science, I began as a theoretical physicist with a strong avocational interest in philosophy and almost none in history. Philosophical goals prompted my move to history; it's to philosophy that I've gone back in the last ten or fifteen years. (Kuhn, 1991/2000, p.106)

10.1 History of Science

Charles Gillispie (1918–2015), a major figure in the history of science community, reviewed *Structure* in 1962 and wrote: 'Thomas Kuhn is not writing history of science proper. His essay is an argument about the nature of science' (Gillispie, 1962, p.1251). But disarmingly, in as much as articulating the nature of science is a philosophical endeavour, Kuhn keeps saying he had no training in philosophy. Outside physics, Kuhn's education fell between disciplinary stools.

Thomas Nickles, a philosopher sympathetic to Kuhn's programme of historically informed philosophy, wrote:

> Kuhn was always something of an amateur, largely self-taught in philosophy and even in history of science. (Nickles, 2003, p.9)

Kuhn may not have taken a PhD in history of science, but he did take his own teaching of history seriously. Stephen Brush, who took a Harvard General Education course of Kuhn's, recalls:

> In Kuhn's seminar we learned that the history of science must be studied by careful reading of original sources. That means reading them in the original language, not relying on translations; it also means becoming aware of the precise meaning of technical terms by reading other works by the same author and works by other authors on the same subject at that time. One must be careful not to read modern meanings into older writings. (Brush, 2000, p.40)

[46] The Google Ngram is a measure of the proportion of a given word among the total of all words published in books during a year. 'Paradigm' was stable and very low between 1800 and 1950; it barely moved off the zero base line. It began to move up in 1960 and then dramatically took off the year the first edition of *Structure* was published. See here.

This is the pedagogy of normal science in a history classroom. There was a lot to learn and be mastered before inquiry and negotiation began. And, as with science, doing the latter without the former was whistling in the dark.

Kuhn's, 1978 *Black-Body Theory and Quantum Discontinuity* (Kuhn, 1978) is regarded by all commentators as his most substantial, archive-informed, and focussed historical work. The book, among other things, traces in detail Planck's initial resistance to the new discontinuity theory of atomic radiation and the quantum effects, advanced by Einstein and others. It documents the tenacity with which Planck held on to the classical, continuous view of radiating energy. Trevor Pinch (1952–2021) a prominent 'new wave' sociologist of science, praised Kuhn's 'penetrating analysis' of the relationship between Boltzmann's statistical mechanics and quantum theory but, tellingly, points out that the book makes no mention of *Structure*. Further, none of the historiographic 'apparatuses' of *Structure* are utilised. Pinch remarked: 'Kuhn has disregarded almost all the issues that grip current sociology and philosophy of science' (Pinch, 1979, p.440) and has provided a 'largely internal history of how discontinuity emerged at the turn of the century' (Pinch, 1979, p.437). Pinch suggests that 'it is quite possible' that Kuhn's indifference to his own ambiguous formulations meant that he was 'unaware' of the radical implications of *Structure*, and so could not carry them through in his serious historical study.

This is a problem for educators and others: The conceptual apparatus of the supposed blinding new Kuhnian light on our understanding of science is not even used by its author in his 'display' work. Further, Abner Shimony, the philosopher-physicist, in reviewing Kuhn's book, wrote:

> On the whole, the intellectual processes of the few physicists immersed in blackbody research seems to me to have been wonderfully rational. (Shimony, 1979, p.436)

10.2 Sociology of Science

Nickles, above, does not mention sociology which was at the core of *Structure*'s argument about the conduct of science and of scientific revolutions. From the beginning, Kuhn was sensitive to sociological factors in science. In 1952, he wrote to Philipp Frank:

> It would seem to me that for any sociologist of science, it would be more fruitful to example the ubiquitous role of the sociology of the professional group than to concentrate solely on those factors (like government, church, etc.) which at this time and place have relatively little impact upon decisions made by professional scientists about problems arising within their own sciences. (Reisch, 2017, p.242)

This sensitivity meant he was attentive to the formative, educational influences, including textbooks, on the professional group and how that group's dynamics bore upon research decisions. But this was sensitivity, not research. Arrestingly, Kuhn admitted that, when writing *Structure*, he 'knew very little about sociology' and further 'he proceeded to make up the sociology of [scientific] communities as he went along'.[47] In 1983, when receiving the John D. Bernal Award, he wrote:

[47] In the early 1950s, Kuhn had been invited by Philipp Frank to join a group researching the sociology of science (Reisch 2017, p.241). Some correspondence is cited above.

> *Structure* is sociological in that it emphasizes the existence of scientific communities, insists that they be viewed as the producers of a special product, scientific knowledge, and suggests that the nature of that product can be understood in terms of what is special in the training and values of those groups. (Kuhn, 1983, p.28)

But Kuhn continued:

> Having insisted upon those points, however, I proceeded to make up the sociology of such communities as I went along, or rather to draw it from my experience with the interpretation of scientific texts supplemented by my experience as a student of physics. (ibid)

And admits:

> That is an abominable way to do sociology, and it did not occur to me that its outcome would, *qua* sociology, have a claim on the attention of members of that profession. (ibid)

Kuhn made little, if any, effort to master extant literature in sociology of science. The argument of *Structure* seems not to have benefited from engagement with either J.D. Bernal's work (Bernal, 1939) or that of Robert Merton, the founder of sociology of science (Merton, 1938/1973, 1942/1973). These fundamental works had been published 20 years before *Structure*. Merton's, 1942 claim about the universality of scientific knowledge is assuredly something that Kuhn might have, for the benefit of everyone, profitably engaged with:

> The cultural context in any given nation or society may predispose scientists to focus on certain problems … But this is basically different from the second issue: the criteria of validity of claims to scientific knowledge are not matters of national taste and culture. Sooner or later, competing claims to validity are settled by universalistic criteria. (Merton, 1942/1973, p.271)

Kuhn neither conducted nor oversaw empirical research studies on laboratory practice or any other scientific practice. His repeated admissions of 'little training' are striking and exemplify the argument of this paper: Kuhn consistently made claims about matters of which he, confessedly, knew little. Kuhn simply did no empirical sociology of science; he did not examine laboratories, research funding, political constraints or support, or control of journals. Barry Barnes, a founder of the powerful Edinburgh school of sociology of science, pointed to Kuhn's superficial grasp of the practice of science:

> In general, Kuhn's work reveals little sensitivity to the highly differentiated structure of science and the importance of competition and mobility between different 'schools' or specialities. It leaves us unprepared for the finding that a combination of the skills of several specialties led to the elucidation of the structure of DNA and hence the creation of a new basic model for biological investigators. (Barnes, 1974, p.95)

A psychologist and a biologist, in a co-authored paper, drew attention to Kuhn's distance from 'coal face' science and his subsequent misconceptions and underestimations of normal science:

> Although Kuhn was trained as a physicist, he writes in the most general and vague terms about the scientific process and conveys little familiarity with the nuts and bolts of conducting research. Consequently, much of Kuhn's analysis amounts to

> abstract speculation that bears little relation to how scientists normally think and what scientists normally do. (Sanbonmatsu & Sanbonmatsu, 2017, pp.134-135)

Some have thought that Kuhn's account of normal science missed so much of its creativity, value commitments, and personal judgement that he was writing about something better described as 'sub-normal' science (Mody, 2015, p.99).

10.3 Science Classrooms

If Kuhn spent little time researching laboratory practice, he spent even less time researching science classrooms where, supposedly, the paradigms of science were being incubated and reproduced. Kuhn had been a student in physics classrooms and had taught in General Education classrooms, and so had inklings and intuitions about the impact of such instruction, but no education 'research data'. Few sociology journals would publish any claim about purported scientific practice if there was no supporting empirical evidence. Yet founders of the Edinburgh Strong Programme in sociology of science consistently, as shown below, cite 'evidence-free' Kuhn as their inspiration, indeed their authority on the subject. Just the unpaginated citation 'Kuhn' was deemed sufficient to establish many contentious points.

10.4 The Limits of Overreach

That Kuhn had no immediate empirical evidence for his claims about the conduct of normal science, or for the processes whereby revolutions in science were initiated and accepted, does not mean that his claims are without warrant. Speculation in advance of evidence is standard scientific practice, but whether the speculation is correct, or justified, is something for the disciplines of philosophy, history, sociology, and perhaps social psychology to ascertain. Some did so and supported Kuhn; others did the same and rejected the Kuhnian picture. Thus the 'Science Wars' were constituted.[48] It is a peculiar situation that so many professionals in these disciplines, including education, so lightly took Kuhn's word for the book's many philosophical, historical, and sociological claims. They adopted and promulgated the Kuhnian picture even when their interpretations were being rejected by Kuhn himself.

John Ziman (1925–2005), a physicist and educator, was sympathetic to Kuhn's cross-disciplinary excursions. He saw this as a strength of Kuhn:

> The deep message of THE STRUCTURE OF SCIENTIFIC REVOLUTIONS was that these jurisdictional disputes were futile. A scientific theory can only be grasped metascientifically as an entity with intertwined philosophical, historical, and sociological characteristics. … That is why we are all Kuhnians nowadays. (Ziman, 1983, p.24)

Doing research across the disciplinary board is as commendable as it is rare, but it does not mean that standards need not be reached; it just means that more of them need to be reached.

[48] See at least Brown (2001) and contributions to McMullin (1988), Brown (1984), and Gross et al. (1996).

Wes Sharrock and Rupert Read, in their careful and sympathetic study of Kuhn, are decisive: 'To say it again, Kuhn is a philosopher above all' (Sharrock & Read, 2002, p.110). And it is as such that he needs ultimately to be understood and appraised. For them:

> Kuhn, we have argued, neither provides a general and true theory of science, nor a set of normative prescriptions for how to pursue science correctly. (Sharrock & Read, 2002, p.210)

These two caveats take a large 'bite' out of Kuhnianism as a guiding educational, or any other, light. And should cause pause when reading David Treagust's claim about *Structure* that opened this essay: 'This small book really changed the way we look at the enterprise that is science' (Treagust, 2022, p.16). What are the consequences of something so described by Sharrock and Read having such impact? What does it say about the science education community and its influence on public understanding of science?

Steven Weinberg (1933–2021), a physicist, historian, and Nobel laureate, observed that:

> But even when we put aside the excesses of Kuhn's admirers, the radical part of Kuhn's theory of scientific revolutions is radical enough. And I think it is quite wrong. (Weinberg, 1998)

Elaborating, he writes:

> It is important to keep straight what does and what does not change in scientific revolutions, a distinction that is not made in *Structure*.There is a "hard" part of modern physical theories … that usually consists of the equations themselves, together with some understandings about what the symbols mean operationally and about the sorts of phenomena to which they apply. Then there is a "soft" part; it is the vision of reality that we use to explain to ourselves why the equations work. The soft part does change; we no longer believe in Maxwell's ether, and we know that there is more to nature than Newton's particles and forces. (Weinberg, 1998)

There are important lessons to be learned about the academy, and culture more generally, from how Kuhn, untrained in any meta-scientific discipline, could have such an international impact and influence. Outside of science, this phenomenon is, depressingly, common. It is the mainstay of political and religious movements and associated rallies.[49] Building HPS into teacher training programmes could mitigate it.

11 Kuhn's Philosophy I: Undergraduate Encounter with Kant

As a Harvard undergraduate, Kuhn completed an elective 'History of Philosophy' course. He admits that not much of the course made an impression, but 'Kant was a revelation' (Baltas et al., 1997/2000, p.264).[50]

[49] Recent US, Brazilian, British, and Russian history well testifies to the reality and power of 'mob psychology'. For a 'long view' of the phenomena, see Kurt Andersen's *Fantasyland.* He devotes two of 450+pages to Thomas Kuhn, describing *Structure* as 'one of the most influential books of the age' (Andersen 2017, pp.191). And adds: 'it fed the new skepticism about science and scientists and, by extension, about rationality as propounded by elites, the mainstream, the Establishment' (ibid).

[50] The central text is Kant's 1786 *Metaphysical Foundations of Natural Science*. For its preface and commentary, see Matthews (2022a, chap.VIII).

I gave a presentation on Kant and the notion of preconditions for knowledge. Things that had to be the case because you wouldn't be able to know things otherwise.

Fifty years later, reflecting on that undergraduate episode and summarising his philosophy, he said:

Oh, it's an important story because I go round explaining my position saying I am a Kantian with moveable categories. (Baltas et al., 1997/2000, p.264)

In his 1993 'Afterwords' reflections on his career, Kuhn writes:

Though it is a more articulated source of constitutive categories, my structured lexicon [Kuhn's new term for paradigm] resembles Kant's a priori when the latter is taken in its second, relativized sense. Both are constitutive of *possible experience* of the world, but neither dictates what that experience must be. (Kuhn, 1993, p.331)

Due to the war, his Harvard programme was cut by a year, and so also cut was the possibility of him doing more philosophy courses. More is the pity as his Kantianism might have been refined or abandoned.[51]

11.1 Observation and Perception

Three years after graduating from Harvard with a physics PhD, Kuhn, in 1951, confidently strode into the philosophers' domain when he wrote:

For scientific observation is always a process of abstraction. One abstracts the length, the color, the texture from a natural object which always provides an infinity of alternate abstractions. Some choice is demanded, and the choice must ultimately rest upon personal prejudice. (Kuhn, 1951/2021, p.17)

Understanding the epistemic function of observation and perception has been central to philosophy since at least Plato who affirmed that 'we see through the eyeball, not with the eyeball'. Bacon and the British empiricists cemented the centrality of perception for science; Kant built a whole system by underwriting, and fleshing out, the observational foundations of Newtonian theory. The crucial role of observation for science was embraced by Mach and then by logical empiricism. On perception, Kuhn states the truism:

What a man sees depends both upon what he looks at and also upon what his previous visual-conceptual experience has taught him to see. (Kuhn, 1970a, p.113)

And he refers to research on the psychology of perception. Philosophers have also, since Plato, been writing on sensation, perception, and observation, but he largely ignored this tradition.[52]

[51] There are many sophisticated accounts of Kant's contribution to understanding of science. See at least Friedman (1992) and contributions to Watkins (2001).

[52] For philosophical analysis of perception and observation, see at least Mandelbaum (1964), Shapere (1982), Dretske (1969), and Kosso (1992, chap.6). Of particular relevance is the philosopher-physicist Abner Shimony (1977).

11.2 Ontological Idealism and Epistemological Relativism

To the end, Kuhn held on to three foundational claims of Kantianism:

(i) Reality as it is (the noumena) is beyond our knowledge.
(ii) The known world, the world of which we can have knowledge (the phenomena), is constructed by us, and depends upon our cognitive machinery.

For Kant, the mind's cognitive machinery—forms of sensibility and a priori categories, including, space, time, and causality—that it imposes on the noumena, is universal; they are part of having a human mind. Crucially, social constructivists, and cultural theorists in education, remove universality from the second bedrock and substitute cultural plurality. So, they post:

(iii) The concepts used to construct the known world vary among linguist, scientific and social groups. Consequently, they live in different worlds.

This third claim is not the banal truth that different people have different *experiences* in the same situation. This is so banal as to hardly warrant being called a claim. Consider, for example, a Chinese and an Australian standing in front of a sign:

The Chinese person has propositional perception STOP and behaves accordingly; the Australian simply has, at best, propositional perception SIGN and does not know what to do. The third claim above is the ontological claim that the real world, not the experiential worlds are different for different groups; they live in different real, not experiential, worlds. This is relativistic metaphysical constructivism, the philosophical position that became a best seller in the academy, and beyond.

A popular, and extended, statement of this third, socialised-ontology, position was Peter Berger and Thomas Luckmann's *The Social Construction of Reality*. There they are adamant that 'reality' and 'knowledge' must be in scare quotes because these supposed realities always 'pertain to specific social contexts' (Berger & Luckmann, 1966, p.15). Their failure to distinguish reference from description, and their ontological conjuring, is endemic. At science education conferences, presenters use fingers to indicate that their use of 'knowledge', 'truth', 'reality', 'proven', and the like is not literal; finger movement shows that the presenter is not simple-minded and is not among the philosophically unwashed.

Observation, more correctly propositional perception, is theory dependent, but it is also nature dependent. Making basic observations has a strong survival value for animals. Science requires, in the way that non-sentient beings do not, that such observations be articulated as propositions—'I see that *p*' where *p* is some propositional statement.[53] For example, 'I see that there is a magnetic field', 'I see that President Biden is on TV', and 'I see that the proportion of tall plants is 1:3'. The practice of science requires the communication, and evaluation, of such observations. Propositional perception requires language, hence some level of theory, but there also needs be some state of affairs to be described. Not all communication need be propositional—gestures, pointing, expressions, body language, a whistle, or music can suffice in many circumstances—but not for scientific communication.

12 Kuhn's Philosophy II: Separation of Truth from Science

In the 1970 Postscript, Kuhn famously, and for some 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, 1970a, p.171)

Separating truth from science was a major break from orthodox understanding. Kuhn repeats his pre-*Structure* conviction:

> We can have no recourse to notions like the 'truth' or 'validity' of paradigms in our attempt to understand the special efficacy of the research which their reception permits. (Kuhn, 1963, p.358)

This is subsequently elaborated as:

> There is, I think, no theory-independent way to reconstruct phrases like 'really there'; the notion of a match between the ontology of a theory and its 'real' counterpart in nature now seems to me illusive in principle. (Kuhn, 1970a, p.206)

The separation is maintained twenty years later, in his 'Afterwords' contribution to a 1990 MIT conference dedicated to appraising the gamut of his philosophical and historical claims. There he makes a much-referenced claim with ontological and epistemological dimensions:

> On the one hand, I aim to justify claims that science is cognitive, that its product is knowledge of nature, and that the criteria I use in evaluating beliefs are in that sense epistemic. But on the other, I aim to deny all meaning to claims that successive scientific beliefs become more and more probable or better and better approximations to the truth and simultaneously to suggest that the subject of truth claims cannot be a relation between beliefs and a putatively mind-independent or 'external' world. (Kuhn, 1993, p.330)

[53] For issues and literature concerning object and propositional perception, see Matthews (2015, pp.250–253).

Concerning ontology, his use of scare quotes around 'external', and reference to 'putative' reality, suggests, if not downright means, that, for Kuhn, there is no such thing as an external, observer-independent world. Otherwise, why use such quotes?

He elaborates puzzle solving, not truth-finding, as the goal of science. Twenty years after publication, and despite all the criticism, he writes that his claim in *Structure* was the 'right one' (Kuhn, 1993, p.338). But Kuhn says his reader has to:

> … set aside the notion of a fully external world toward which science moves closer and closer, a world independent, that is, of the practices of the scientific specialties that explore it. (Kuhn, 1993, p.338)

Again, what function and purpose does 'fully' play in this claim? Might an external world be partly dependent upon the observer? Seemingly, for Kuhn, this is so. This, at best semi-idealism, was clearly stated in the first edition, and it was not retracted in the second, nor thereafter:

> I have so far argued only that paradigms are constitutive of science. Now I wish to display a sense in which they are constitutive of nature as well. (Kuhn, 1970a, p.110)

This is the beginning of an ontological idealist slope that ends, as John Passmore observes, in 'the French intellectual's dream … of a world that exists only in so far as it enters into a book' (Passmore, 1985, p.32). There are many social constructivists, in and out of education, lining the slope, and cheering all who slide down it.

Concerning epistemology, if truth is taken off the science table, then what is progress toward? Economic betterment? Political power? Research funds? All Kuhn offered was better 'puzzle solving'. In the Postscript, he writes:

> Taken as a group or in groups, practitioners of the developed sciences are, I have argued, fundamentally puzzle-solvers. (Kuhn, 1970a, p.205)

And once they are initiated into their puzzle-solving craft:

> Whether or not individual practitioners are aware of it, they are trained to and rewarded for solving intricate puzzles be they instrumental, theoretical, logical, or mathematical at the interface between their phenomenal world and their community's beliefs about it. (Kuhn, 1993, p.338)

As with so much of Kuhn, readers need to pass quickly over clauses such as 'the interface between their phenomenal world and their community's beliefs about it'. Staying too long and asking: 'What does this mean?' makes for very slow reading without guarantee of any resolution.

12.1 Shape of the Earth

Demonstrably, competing theories can be appraised with respect to how adequately or approximately they depict the world. Consider the 2000 + years investigation of the Earth's shape. Pythagoras' 500BC claim that the Earth is spherical is closer to the shape of the Earth, and was progressively seen to be so, than competing claims that the Earth was flat. Until the fifteenth century, official Chinese astronomers (and there were no astronomers apart from official ones) were 'flat earthers'. This is seen in the important early fourteenth-century astronomy book, *Ge xiang xin shu*, written between 1324 and 1335 by the Daoist priest and astronomer Zhao Youqin. In his version of neo-Confucian cosmology, the earth is likened to a flat board floating on water, with China in its centre, and surrounded by the heavens; the cosmos had a globular or egg structure.

In 1583, Matteo Ricci the Jesuit priest, astronomer, and natural philosopher began his mission to China (Brockey, 2008). He advanced the Copernican system, though it was banned in Rome, against official 'flat earthers'. His accurate predictions of the 1601 solar eclipse in comparison to the failure of the court astronomers made such an impact on the Ming Emperor Wanli that the Copernican spherical earth theory became official policy.[54] Scientific understanding could be, and was, shaped by how the world was.

In the eighteenth century, the spherical theory was adjusted. The realisation that the seconds pendulum was slowing at the equator prompted refined views about the Earth's supposed spherical shape. Some, the 'squeezers', constricted the equator; others, the 'flatteners', expanded it. The latter, who included Newton, eventually won a debate that folded metaphysics, theology, mathematics, national interest, and technology into science.[55] Thereafter, belief in the spheroidal, oblate Earth became the cultural and scientific norm. Different of Darwin's theories of evolution might not fully 'capture' the natural realities of evolution, but they do better than Special Creation and so on across the landscape of science. Why agree with Kuhn's denial of any meaning to 'claims that successive scientific beliefs become more and more probable or better and better approximations to the truth'? Progressively, the true shape of the Earth was ascertained. Contra Kuhn, John Worrall sensibly writes:

> It seems difficult to deny, I suggest, that the development of science has been, at least to a very good approximation, cumulative at the observational or experimental level. (Worrall, 2002, p.32)

12.2 Pseudoscience

It is of significant philosophical and social importance to distinguish science from pseudoscience. With difficulty, this can be done.[56] But if science in its totality is just puzzle solving, as Kuhn would have it, without a voice from the world, then the separation of scientific puzzle solving from pseudo puzzle solving becomes impossible. Any book of pseudoscience is full of solved puzzles; most pseudoscience websites will, for a fee, solve your puzzle. Most pseudosciences have a large or small dash of technology which might well solve puzzles. But technology is not science. In addition, there are personal and social consequences of not distinguishing science from pseudoscience. Why set up 'Truth and Reconciliation' commissions if there is no truth to be found?[57]

[54] This episode is discussed and documented in Matthews (2019a, chap.6).

[55] The shape of the earth debate, and its dependence on accurate timekeeping, is canvassed in Matthews (2000c, chap.6).

[56] For literature, and arguments on this distinction, see Curd et al. (2013, section I) and Matthews (2019a, chap.13).

[57] On Kuhn's problems occasioned by eliminating truth from science, see Devlin (2015).

13 Kuhn's Philosophy III: Anti-Realism

The realism/anti-realism divide is perhaps the longest-running debate in philosophical reflection on science beginning at least when Aristotle asserted his realism against forms of Platonic idealism. The modern form of the debate was initiated when the Protestant scholar Andreas Osiander inserted an unsigned Preface into Copernicus's 1543 *On the Revolution of the Heavenly Spheres*. The instrumentalist preface asserted that Copernicus's Earth was not really revolving, and it was just said to be so in order to simplify astronomical calculations. Galileo famously upheld the realist reading of Copernicus. Only under inquisitorial pressure did he formally adopt an anti-realist, instrumentalist position.[58] Publication of *Structure* brought this philosophical debate to quarters hitherto unaware of it.

Ernan McMullin (1924–2011) correctly recognised that 'The radical challenge of *Structure* is directed not at rationality but at realism' and went on to observe that 'Kuhn's influence on the burgeoning anti-realism of the last two decades can scarcely be overestimated' (McMullin, 1993, p.71). Kuhn concurred with McMullin's, and earlier critics', charges of anti-realism:

> 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, which I expect to provide in another place. (Kuhn, 1990, p.317)

The threats do require more discussion than Kuhn gave them. Some have endeavoured to elaborate, and make consistent, Kuhnian anti-realism. One such interpreter, Michela Massimi, does add the obvious proviso that 'Kuhn's view is either relativist or realist; it cannot be both at the same time' (Massimi, 2015, p.143).

There are a variety of realisms.[59] One book, titled *Varieties of Realism*, has 22 contributions (Agazzi, 2017). In recent decades, three dominant variants have emerged: *Structural Realism* advanced by John Worrall (Worrall, 1989), *Ontic Structural Realism* advanced by both Steven French (French, 2017) and James Ladyman (1998), and *Selective Realism* advanced by Alberto Cordero (Cordero, 2017).

Realism maintains that there exists an observer-independent world, that scientific theories make claims about both observational (compass needle movements) and non-observational (magnetic fields) things in that world, that those claims are approximately true, and that scientific progress occurs and is marked by widening and deepening the pool of such true and approximately true claims. Realist positions share the following commitments:

- An *ontological* commitment to the reality and independence of the world: external things and events, including unobservable and inferred entities, exist independently of cognising subjects.
- A *semantic* commitment to the linkage of scientific claims to external things and events: science makes claims about the world.
- An *epistemological* commitment: namely, that science has made some truthful, or approximately truthful, claims about entities and processes in both the observed and unobserved world. The observed or experienced world is the everyday world revealed

[58] The Galilean episode is discussed, with texts, in Matthews (1989, chap.II).

[59] For elaboration of realism, see Bunge (1993), Devitt (1991), Kukla (1998), Harré (1986), Sankey (2009), and Psillos (1999, 2000) and contributions to Leplin (1984), Agazzi (2017), and Lyons and Vickers (2021).

by ordinary vision (billiard balls, fish, clouds, etc.); the unobserved world is that indicated by instruments and about which inference are made (molecules, atoms, magnetic fields, proteins, gravity waves, etc.).

- An *axiological* commitment that the aim and purpose of science is to produce statements and theories about the world that are true; other purposes, such as utility, economic gain, professional advancement, or national pride, are secondary, or merely by-products of truthfulness.

Counter-wise, there is a family of anti-realist positions that are united by their rejection, sometimes for different reasons, of one or more of the above claims. The anti-realist family include positivism, empiricism, instrumentalism, constructivism, constructive empiricism, idealism, and of course the whole gamut of postmodernisms.

Some scholars reject the dichotomous division of realism/anti-realism. In the Introduction to a recent collection *Contemporary Scientific Realism*, the editors write:

> The debate has come a long way since the 1970s and the solidification of its framework in the ’80s. The noted dichotomy of “realism”/“antirealism” is no longer a given, and increasingly “middle ground” positions have been explored. (Lyons & Vickers, 2021, p.1)

The ‘middle ground’ that they identify are those ‘partial’ or ‘select’ realisms for whom putative existence is only claimed for the components of a successful theory that ‘do the heavy lifting’, the hypotheses within the theory that generate testable and confirmed predictions. Realism does not need to assert that all postulates of a successful theory are real just the effective parts need to be real. And epistemically, claims about these parts need to be true or approximately true. So, when experiment confirmed Fresnel’s theory of light, initially its associated ether, along with its transversal microscopic particle oscillations, was thought to be real. This was wholesale realism. When, eventually, the ether was discarded, selective realists could stand-by the components and argue that experiment confirmed their existence and efficacy. Select realism is still realism; it is no concession to idealism; the effective parts need to be real. And epistemically, claims about these parts need to be true or approximately true.

Kuhn’s anti-realism recurs in his historical studies. He provided a detailed study of the overthrow of Stahl’s and Priestley’s phlogiston account of combustion by Lavoisier’s oxygen account, oft named ‘The Chemical Revolution’. The episode had been the second of eight case studies in Conant’s celebrated *Harvard Case Histories in Experimental Science* (Conant, 1948, vol.1, pp.67–115). Kuhn taught the case. He correctly recognised that what Lavoisier discovered in 1777 was not ‘so much oxygen as the oxygen theory of combustion’ (Kuhn, 1970a, p.56). For Kuhn, ‘discovery means seeing both *that* something is, and *what* it is’. Priestley regarded the new oxygen as ‘dephlogistated air’. Irrespective of the translatability of ‘dephlogistated air’ into ‘oxygen’, the two theories could be, and were, compared by natural philosophers on their scientific merit. Lavoisier’s account triumphed, though it came into the world with its own anomalies.[60]

In a much-cited passage, Kuhn goes on to assert: ‘after discovering oxygen Lavoisier worked in a different world’ (Kuhn, 1970a, p.118). It reads nicely, but it cannot be literal. They corresponded with each other; the furniture of their houses and laboratories remained

[60] For accounts of the episode see Conant (1948, chap.2), Jackson (2005), and Musgrave (1976).

the same; and they breathed the same oxygen. And, more to the point, their rival theories and conceptual schemes could be and were compared. Whatever the standing of incommensurability, it does not mean incomparability.

Kuhn says, in discussing the acceptance by chemists of Dalton's new atomic theory and law of constant proportions, that subsequently: 'even the percentage composition of well-known compounds was different' (Kuhn, 1970a, p.135). Taken literally, this is gold medal idealism. For realists, and most thoughtful people, the actual percentage composition of chemicals did not change, and the percentage remained constant. Chemical bonds are not made, and unmade, atomic constituents do not come, and then go, as beliefs about a compound change among those researching it, or at the same time while competing researchers look at it. What changes is the understanding of chemists. Another case of Kuhn's lazy writing that fuels idealism and relativism.

14 Kuhn's Philosophy IV: Incommensurability

Kuhn consciously tried to make up lost philosophical ground. He wrote different pieces responding to his philosophical critics (Conant & Haugeland, 2000). In his 1991 Rothschild lecture, and in his 1993 'Afterwords' (Kuhn, 1993), he held out the promise of finally, after working for many years, making a substantial philosophical statement. This would be a book that would elaborate and defend his thesis of incommensurability. It was the subject of a 1989 NSF grant for a book provisionally titled: *The Plurality of Worlds: An Evolutionary Theory of Scientific Development*. A version of the book finally appeared in 2022 (Mladenovic, 2022).[61]

Writing such a book was always going to be a hard task for Kuhn. It was with good reason that Nicholas Maxwell, an English philosopher of science, succinctly stated:

> Incommensurability was Kuhn's worst mistake. If it is to be found anywhere in science, it would be in physics. But revolutions in theoretical physics all embody theoretical *unification*. … It always astonished me that anyone took incommensurability seriously for a moment, especially as Michael Faraday solved the problem around 1834, long before Kuhn and Feyerabend invented it. … It is at once clear that the picture of science Kuhn gives us in *Structure* is very seriously inadequate. (Maxwell, 2014, pp.133, 140)

Paul Feyerabend gave prominence to incommensurability in his landmark contra-logical empiricist, 1962 paper 'Explanation, Reduction and Empiricism' (Feyerabend, 1962). Kuhn made comparable use of the concept in his 1962 *Structure*. He stressed that different paradigms had different vocabularies for seemingly the same things, they had different puzzles to solve, what were puzzles in one paradigm did not arise in others, methodological and testing techniques and standards differed between paradigms, adherents saw different things when looking at the same object, and so on (Kuhn, 1970a, pp.148–150).

For Kuhn, incommensurability had semantic, perceptual, methodological, and ontological dimensions. Ontological, because different realities came into being with different

[61] Details of the history of the book are in Marcum (2015, pp.125–126) and Hoyningen-Huene (2015, pp.190–191).

paradigms. With a new paradigm, ‘scientists lived in different worlds’, they ‘talked through’ each other, and they had ‘incompatible modes of community life’. He maintained that, for different puzzle-solving traditions, there was ‘no one world within which to work on them’ (Kuhn, 1970a, p.147). For many, after Kuhn, incommensurability functioned as a form of diplomatic immunity: ‘You cannot criticise, evaluate or even question me, as I am in a different paradigm, I live in a different world’.[62]

Kuhn made several efforts to amplify, modify, walk-back his original ‘radical’ claims about incommensurability across paradigms and its consequent epistemologically relativist and ontologically idealist implications.[63] When he abandons paradigms in favour of ‘lexical structures’, he writes:

> Applied to a pair of theories in the same historical line, the term [‘incommensurability’] meant that there was no common language into which both could be fully translated. Some statements constitutive of the older theory could not be stated in any language adequate to express its successor and vice versa. (Kuhn, 1990, p.299)

Consequently, ‘Incommensurability thus equals untranslatability’ (ibid).[64]

Untranslatability might be a genuine linguistic problem, one language might lack the required word or words to express something that another language does, but this is unrelated to epistemological incommensurability. Theories are rivals because they are about the same thing. And they can be compared even if not completely translatable. Incommensurability does not mean unable to be compared. Alternative views, theories, and systems can be compared with respect to intelligibility, consistency, coherence, testability, evidence, originality, heuristic power, and agreement with established science.[65] Ideally, after some debate and with some difficulty, sub-scores can be allocated, a total derived, and a best-contender chosen. Kuhn and others stress that this is ideal; in reality, the declaration of a short-term winner often depends on many external inputs (money, culture, religion, careers, political pressure, etc.), but in the long term, these factors are minimised. The well-known histories of Catholic anti-Copernican astronomy, Nazi science, Stalinist science, Maoist science, and tobacco science all fit this pattern. Very powerful forces maintained errant theories but could not keep doing so; better, more truthful ones prevailed. External authority can only carry science so far and for so long.

15 Kuhn’s Influence on Education I: Conceptual Change Research

Notwithstanding all of the foregoing caveats, qualifications and trenchant criticisms, Kuhnian philosophy,[66] more particularly Kuhn’s purported epistemological relativism and his ontological idealism, had enormous impact on educational theory, research, curriculum,

62 This ontological idealism has serious consequences when gender equality, human rights, or political systems are being appraised. So, ‘Such and such group might have human rights in your reality, but not in our reality’, and so on.

63 See Hoyningen-Huene (1993, pp.206–222), Marcum (2015), and Sankey (1993). There is an abundance of technical literature on incommensurability: see contributions to Hoyningen-Huene and Sankey (2001) and Sankey (1994). On Kuhn’s own account, and critique of it, see Sankey (1993) and Mizrahi (2015).

64 Gerald Doppelt described Kuhn’s final position as ‘moderate relativism’ and defended it (Doppelt 2001).

65 On such an approach to incommensurability, see Bunge (1999, pp.253–254).

66 The term ‘Kuhnian’ rather than ‘Kuhn’s’ is deliberate as many positions were advanced in his name that he did not accept.

and classroom teaching. Four fields where the impact was most felt will be examined here: Conceptual Change Research, Constructivism, Science-Technology-Society education, and Cultural Studies in Education.

Early in Kuhn's research career, and certainly by 1966, he sought to collaborate with Jean Piaget, the internationally renowned developmental psychologist and author of *Genetic Epistemology* (Piaget, 1970). Kuhn relates how accidental and serendipitous was his discovery of Piaget:

> 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, 1970a, p.vi)

This was an encounter with consequences. By Kuhn's admission:

> 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, 1977a, p.21)

Kuhn provided an entrée to HPS for educators. In the 1970s and 1980s, pupil's conceptual change in science learning was a prime focus of educational research. The field was energised and vindicated by Kuhn's so public making of parallels between the growth of science and children's conceptual change.

In the 1970s and early 1980s, there was much research published on children's and adults' scientific misconceptions.[67] Well-attended research conferences on 'Misconceptions and Education' were staged (Novak. 1987). R.L. Doran's, 1972 article 'Misconceptions of selected science conceptions held by elementary school students' (Doran, 1972) and James Wandersee's, 1985 article 'Can the History of Science Help Science Educators Anticipate Students' Misconceptions?' (Wandersee, 1985) are representative of the genre.

The noun 'misconception' was the then norm. But soon enough, the research field caught up with Kuhnian 'new philosophy'. John Gilbert and Michael Watts documented how supposed advanced philosophy of science of the time—citing Kuhn, Feyerabend, and Lakatos—had moved 'from realism to relativism', and catching up with this move, educators no longer researched misconceptions, but rather researched alternative conceptions (Gilbert & Watts, 1983). This was not merely a change of vocabulary; it was a serious epistemological reorientation.

This move, ultimately, had great consequence in arguments over appropriate multicultural and indigenous education. For these researchers, some groups and cultures do not have misconceptions about COVID infection, the healing power of crystals, the use of divining rods, or the efficacy of prayer in treatment of malaria: they just have alternative conceptions. If it is a misconception, then there is some inherent educational ground for correcting it, difficult though it might be. If it is an alternative conception, then there is no particular educational ground for correction. An alternative is not wrong; it is just an alternative, much like a music preference.

As an anthropological and cultural fact, there are millions of alternative conceptions. Some held by whole societies, and others held just by small, down to family-sized groups. The crucial philosophical and educational question is when, and where, to label some as misconceptions and endeavour to rectify them. Not all alternative conceptions need be corrected. They can be left as misconceptions if they do no personal or cultural harm, but not

[67] See Doran (1972), Driver and Easley (1978), Novak (1987), and the 2,000 + entries in Reinders Duit's 2009 bibliography (Duit 2009).

so left in a science class when they are at odds with enabling children to understand how the world has been shown to work. If pupils are to understand the aetiology and transmission of COVID, the teachers cannot be relaxed and indifferent to their misconceptions.

A very influential article in the programme was George Posner and colleagues' 'Accommodation of a Scientific Conception: Toward a Theory of Conceptual Change' (Posner et al., 1982).[68] 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 of scientific change 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 programme.

Hewson, in his elaboration, says:

> Provided that due consideration is given to discussion of the metaphysical and epistemological ideals underlying modern science, the teaching of the current conceptions need not be viewed as indoctrination, because the basis for a rational justification is available. (Hewson, 1981, p.395)

Clearly, discussion of the 'metaphysical and epistemological ideals underlying modern science' requires the rudiments of history and philosophy of science. But, how can such ideals be discussed without knowledge of HPS? The shame is that, internationally, science teacher education, and research, carries on with little, if any, input from HPS.[69]

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). They focus on learning where there is *good* reason for coming to hold the belief. This is an echo of Piaget's distinction between *epistemic* and *psychological* subjects.

Despite their explicit entreaty, much conceptual change research, or research on the learning of science, which followed Posner and Strike's paper was poorly, or not at all, informed by philosophy.[70] Where epistemology is mentioned, it is commonly taken to be 'personal epistemology', that is, investigations of what learners *thought* knowledge was. So, one much-cited book was titled *Personal Epistemology: The Psychology of Beliefs About Knowledge and Knowing* (Hofer & Pintrich, 2002). This detour from philosophy into psychology is hardly surprising as philosophy is not part of science teacher education and is rarely part of graduate programmes in education.[71] Educational research did not, and

68 Having 9000+citations (April 2022).

69 This claim is elaborated in Matthews (2020)

70 Exceptions can be found in the 1991 thematic issue of *Journal of Research in Science Teaching* devoted to the subject (vol.28 no.9). See especially Duschl and Gitomer (1991).

71 Proponents of 'naturalistic epistemology' are more comfortable with accepting such work as epistemology, but that is an in-house philosophical debate.

more seriously could not, engage with what might constitute *rational* conceptual change, or believing with *good* as distinct from *bad* reason: philosophical competence, or at least interest, was needed to identify such change.

15.1 Cleavage Between Epistemology and Psychology

With the exception of the Piagetian tradition,[72] and some cognitive psychologists,[73] there was a deep cleavage between epistemology and psychology in educational research. This is evidenced in the title of an article: 'Psychology and Epistemology: Match or Mismatch When Applied to Science Education?' (Duschl et al., 1992). For example, the study by educators of knowledge acquisition has largely been the study of changing beliefs: Supposedly, if beliefs develop, then knowledge grows. Psychologists and educational researchers are indifferent to the distinction between knowledge and belief: whether beliefs are true or false, whether they are held for good or bad reasons, whether any change of belief is rational or irrational, and so on down a standard philosophers' list of epistemological queries. The better learning by heart of the Bible, the Koran, mathematical tables, or a science textbook constitutes, for psychologists, knowledge growth. This was manifested during the regime of behaviourist psychology, and the neutrality principle has not been discarded in the successor regime of cognitive psychology. This epistemically indifference route in psychology is the same as constructivists advocate for sociology and anthropology. For Berger and Luckmann:

> … the sociology of knowledge must concern itself with whatever passes for 'knowledge' in a society, regardless of the ultimate validity or invalidity (by whatever criteria) of such 'knowledge'. … we contend that *the sociology of knowledge is concerned with the analysis of the social construction of reality*. (Berger & Luckmann, 1966, p.15)

The cleavage of epistemology from psychology was front-of-stage for the long-entrenched behaviourist tradition in educational psychology. Ernest Hilgard and Gordon Bower, in their much-reprinted textbook, write:

> Learning is the process by which an activity originates or is changed through reacting to an encountered situation, provided that the characteristics of the change in activity cannot be explained on the basis of native response tendencies, maturation, or temporary states of the organism (e.g. fatigue, drugs, etc.). (Hilgard & Bower, 1966, p.2)

The study of learning makes no contact with reason, justification, evidence, rationality, or any other such epistemological consideration. Such an account of learning flowed easily into accounts of teaching which was also seen as a 'philosophy-free zone'. So, as described in a major 1973 NSSE Yearbook:

> Teacher education programs may be conceptualized as behavior modification systems designed to modify complex behavioral repertoires which are adaptable to a variety of learning problems. (McDonald, 1973, p.41)

[72] See Piaget (1970, 1972) and Kitchener (1986, 1993).
[73] See Nersessian (1989, 2003).

It was not psychologists, but philosophers, who called out the inadequacy, and inaccuracy, of such truncated and miserable understanding of teaching and learning. Israel Scheffler, Kuhn's Harvard colleague and critic, wrote:

> Teaching ….is clearly not, as the behaviourists would have it, a matter of the teacher's shaping the student's behaviour or of controlling his mind. It is a matter of passing on those traditions of principled thought and action which define the rational life for teacher as well as pupil. (Scheffler, 1967, p.133)

And Richard Peters, a psychologist turned philosopher and co-founder with Israel Scheffler of analytic philosophy of education,[74] wrote:

> Teaching is a complex activity which unites together processes, such as instructing and training by the overall intention of getting pupils not only to acquire knowledge, skills and modes of conduct, but to acquire them in a manner which involves understanding and evaluation of the rationale underlying them. (Peters, 1966, p.261)

These more sophisticated and, frankly, more intelligent accounts of teaching and learning arise from philosophy of education. This once important and valued component of teacher education has, unfortunately, now all but disappeared throughout the world.[75] Sadly, too many teachers, curriculum writers, and administrators just stumble around without philosophical direction. And in the worst case, they take direction from the last political, ideological, religious, or commercial foot that trod upon, or funded, them. Being 'employment ready' is now, mantra like, taken as the educational aim for numerous school and university systems.

16 Kuhn's Influence on Education II: Constructivism

Kuhn is front and centre of constructivism which for nearly 40 years has dominated educational research and theorising. Kenneth Tobin says that, in literature published in high-impact journals from mid-1970s to 2005, more than 3000 used 'constructivism' in their title or in their associated key words (Tobin, 2007, p.291).[76] The reach of constructivism was well captured in the sub-headings of a single article: 'A constructivist view of learning', 'A constructivist view of teaching', 'A view of science', 'Aims of science education', 'A constructivist view of curriculum', and 'A constructivist view of curriculum development' (Bell, 1991). Multiple thousands of constructivist-inspired research articles, and hundreds of books, were published.[77] Leading constructivists acknowledge Kuhn as the fount of their relativist and idealist view of science.

[74] On this tradition, see contributions to the three volume Dearden et al. (1972).

[75] See Hirst (2008) and contributions to Colgan and Maxwell (2020).

[76] For an account of the influence of constructivism in science education, see Matthews (2000, 2015 chap.8, 2021b). For wider views of the matter, see contributions to Matthews (1998a) and Phillips (2000).

[77] Pfundt and Duit (1994) contain a cumulative index of the 2000+constructivist articles published to the early 1990s.

16.1 Constructivist Philosophy

The metaphysical idealism of *Structure*, when adopted by educators, had enormous consequences for the goals and justification of science teaching. Kuhnian constructivism informed a great deal of educational policy, pedagogy, curriculum writing, and decision-making. Egon Guba and Yvonna Lincoln, authors of a multi-edition, major education-methodology handbook (Guber & Lincoln, 1989), having 23,000 + citations, embrace Kuhn's 'new philosophy' and say of their handbook that:

> Chapter 3 develops the paradigm in detail, contrasts its basic belief system with that of positivism, and cites arguments and evidence supporting the rejection of positivism and the acceptance of constructivism in its place. (Routledge website)

With their constructivism comes multi-ontologies and subjectivist and relativist epistemology.

Richard Duschl and Richard Grandy, in the Introduction to an important anthology *Teaching Scientific Inquiry*, correctly note:

> What views teachers hold about constructivism will affect not only the content of what they teach and the methods they use, but also what they take as the goals of science teaching. (Duschl & Grandy, 2008, p.23)

David Hawkins (1913–2002), a physicist and educator, spoke for many, when in an article on the history of constructivism, wrote that Kuhn's *Structure* 'provided '"constructivist" justification' for 'philosophies of relativism and subjectivism' (Hawkins, 1994, p.10). Joseph Novak, a leading science education researcher, 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).[78] He wrote:

> In philosophy, a consensus emerges that positivism is neither a valid nor a productive view of epistemology … What is emerging is a *constructivist* view of epistemology, building on ideas of Kuhn (1962), Toulmin (1972) and others. (Novak, 1977, pp.5-6)

Ernst von Glasersfeld (1917–2010) in the opening sentences of a much-cited paper—'Cognition, Construction of Knowledge and Teaching'[79]—points out 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)

For many science educators, von Glasersfeld was their conduit to Kuhn. His 'radical constructivism' blossomed across the science and mathematics education landscapes.[80] Andreas Quale's book is typical of the genre: *Radical Constructivism: A Relativist Epistemic Approach to Science Education* (Quale, 2008). For Glasersfeld, 'Truth in constructivism, as I keep repeating, is replaced by viability' (Glasersfeld, 1993, p.25).[81] Little, if

[78] Joseph Novak guided much work at Cornell University where in the decade after 1977, over 100 graduate students were enrolled (Novak 1977, p.7), and over his whole career he supervised, or contributed to, theses of 300 + graduate students (Novak 2018, p.138).

[79] As of September 2022, the paper has 2880 citations.

[80] See Glasersfeld (1984, 1993, 1995) and Steffe and Thompson (2000).

[81] For critical appraisal of von Glasersfeld's positions, see Matthews (1994, pp.148–158) and contributions to Matthews (1998b).

any, attention was paid to philosophical criticisms of the position. The criticisms mirrored the criticisms of Kuhn's *Structure* made by Shapere, Masterman, Devitt, Shimony, and so many others. The Sydney philosopher, Wallis Suchting, wrote a long, 22-page, careful analysis of von Glasersfeld's, 1989 paper. He employed the philosopher's 'lingering line-by-line method' of analysis, concluding:

> First, much of the doctrine known as 'constructivism' ... is simply unintelligible. Second, to the extent that it is intelligible ... it is simply confused. Third, there is a complete absence of any argument for whatever positions can be made out. ... In general, far from being what it is claimed to be, namely, the New Age in philosophy of science, an even slightly perceptive ear can detect the familiar voice of a really quite primitive, traditional subjectivistic empiricism with some overtones of diverse provenance like Piaget and Kuhn. (Suchting, 1992, p.247)

Von Glasersfeld did reply (Glasersfeld, 1992), but Suchting's paper was ignored, while von Glasersfeld was given 'Plenary Speaker' status at education conferences and repeatedly cited in research literature, having 20,000 + Google Scholar citations in October 2022.

Duschl and Grandy, in their *Scientific Inquiry* publication, adopt the familiar distinction between *cognitive* constructivism and *metaphysical* constructivism, recognising that 'both … trace back to the work of Kuhn' (Duschl & Grandy, 2008, p.24). The former, in brief, is a *psychological* theory about learning, concept acquisition, and change of belief. The latter, also in brief, is a *philosophical* theory (ontological idealism) about the furniture of the world; namely, things are created by either individuals or groups. Duschl and Grandy repeat the comfortable, non-disruptive view that debate about metaphysical constructivism is:

> irrelevant for science education once we understand fully the implications of cognitive constructivism. (Duschl & Grandy, 2008, p.25)

The comfort is deceptive. Many influential educators reject realism and affirm idealism. When this is spelt out, their position is not so comforting. John Staver (1944–2022), a prominent science educator, affirmed:

> …For constructivists, observations, objects, events, data, laws, and theory do not exist independently of observers. The lawful and certain nature of natural phenomena are properties of us, those who describe, not of nature, that is described. (Staver, 1998, p.503)

That observations and theory do not exist apart from agents is a truism, but the ontological status of events and objects is a different matter. This should be obvious. To run them together requires additional argument.

Rosalind Driver (1941–1997), an influential science educator and constructivist, claimed:

> science as public knowledge is not so much a "discovery" as a carefully checked "construction" … and that scientists construct theoretical entities (magnetic fields, genes, electron orbitals …) which in turn take on a "reality" (Driver, 1988, p.137).

For her, and all the other metaphysical constructivists, the Earth does not have a structure until geophysicists impose it; there is no natural selection in the animal world until Darwinian biologists impose it; atoms have no structure until physicists propose one; DNA was not a double helix until it was found to be such; Neptune did not exist, and thus could not have influenced the motion of Uranus, until John Galle, guided by the calculations of

Le Verrier, observed it; and so on. One might ask: If gravity waves are our own creation, why spend so much time and money looking for them?

Driver's basic argument form is common:

Premiss	Concept X is a human construction.
Conclusion	Therefore, the referent of X does not exist.

One only has to state this argument to see that it is an invalid inference, and its validity depends upon making explicit a suppressed premiss:

Suppressed premiss	All concepts that are human constructions can have no existential reference.

But this suppressed premiss is simply philosophical dogma for which no evidence is provided. Not only are 'electron orbitals' and 'magnetic fields' human constructions, but so also are 'my house', 'that mountain', 'this table', and all the other observational terms we use. If the foregoing widespread constructivist argument were valid, then not only would electron orbitals not independently exist, neither would our house, the tables in it, nor mountains that we might live near. Indeed, given that the personal pronoun 'I' is a human construction, individual cognising subjects might not exist.

These considerations are not 'philosophical quibbles' that can be put aside while the business of teaching and learning science proceeds. They are matters with which students should, in ways appropriate to their ages, engage. This will contribute to social well-being. A great deal hinged on whether the SARS-CoV-2 virus does actually exist, independently of anyone recognising it, so also its mutant Alpha, Beta, Gamma, Delta, and Omicron variants. The conjunction of ontological idealism and epistemological relativism provides succor to denialisms of all kinds and props up the minority non-scientific responses to the COVID pandemic (McIntyre, 2019). When a student, or citizen, asks: 'Does the virus exist, and should money be spent looking for it?' realists can give a straight answer: 'we think it exists and money should be spent looking'. Idealists must give a more complicated answer, one that can only diminish trust in science.

The fact that theories and their posits are humanly constructed (this is, after all, tautological), and that natural objects are considered in science only in their theoretical dress—apples as point masses in physics, as exchange values in economics, as calories in biochemistry, as rewards in psychology, or metaphors in art history—does not imply that there are no apples, that real objects are human creations, or that the real objects have no part in the appraisal of the scientific worth of the conceptual structures brought to bear upon them. Realism is consistent with there being no privileged description; the adequacy of the propositional description is bound to the adequacy of the theory providing it. Different disciplines have different languages, lexical structures, and vocabularies, for describing the same thing. Realism recognises a difference between real objects and theorised objects.

16.2 Constructivist Pedagogy

As well as educational theorising, Kuhnianism informed pedagogical practice. Derek Hodson, a prominent New Zealand, then Canadian, science educator made a direct link between Kuhn and constructivist pedagogy:

> 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 connection to curriculum is not as direct as Hodson writes; indeed the link is orthogonal. But the link to pedagogy was easy because much of the teacher-training establishment adopted the principle that 'science should be taught as it is practised'. This was an old principle that had its origins in John Dewey's distinction between science as subject matter and as method, and his conviction that the latter needs be taught by being practised (Dewey, 1910/1995). And taught not just for the sake of science, but for the health of culture and society. Beginning in the 1980s, advocates of this tradition spoke of 'the pupil as scientist'.[82] The Deweyan conviction underwrote internationally ubiquitous 'Science as Inquiry' programmes.[83]

It was a misfortune that advocates looked at science as it was practised, not at the training and formation that was required for scientists to engage in scientific practice. To be a musician, or a good player of sport, requires the repetitive practise of a great amount of tedious, low-level skills. The learner of any intellectual or practical craft needs to see exemplary practice, be shown the best, hopefully be motivated by it, but also understand what effort goes into achieving the performance. Conant's Harvard case studies performed precisely this role.[84] Kuhn, as will be indicated, fully appreciated this. Many of his education readers appreciated it less fully.

Nancy Davis and colleagues, and indeed many others, make a direct, but mistaken, link between Kuhn's theory of science and pedagogy. They write that they used 'Thomas Kuhn's (1970a)work as a basis to support change in guiding epistemological paradigms', whereby they endorse constructivism and reject objectivism (Davis et al., 1993, p.627). They then move easily from constructivist epistemology to constructivist pedagogy:

> From a constructivist perspective, the individual learner has a primary role in determining what will be learned. Emphasis is placed on providing students with opportunities to develop skills and knowledge which they can connect with prior knowledge and future utility. … The learner decides with others what learning is important to him or her and means of learning are explored. While working with others, the student solves problems and examines solutions. This view of curriculum is closer to the actual work of scientists. (Davis et al., 1993, p. 629)

No practising scientist, and certainly not Kuhn, could identify any of this in their own formation as scientists. Notwithstanding, constructivism was adopted as the official education theory in numerous provinces, states, and countries. In the USA:

> … the current teaching standards in the USA call for teachers to embrace a social constructivist view of learning and teaching in which science is described as a way of knowing about natural phenomena and science teaching as facilitation of student

[82] The canonical statement was Rosalind Driver's book *The Pupil as Scientist* (Driver 1983). See also Kitcher (1988).

[83] See Bybee (2000), DeBoer (2004), Duschl and Grandy (2008), and National Research Council (2000).

[84] Conant established the Natural Science 4 course, titled 'Research Patterns in Physical Science'. For some years, Kuhn taught this course (as did Gerald Holton). On the General Education programme, see Fuller (2000a), Rudolph (2002, pp.48–51). On the utilization of the case studies in schools, see Klopfer (1964).

> learning through science inquiry … In particular, the reform emphasizes teacher education by promoting social constructivist teaching approaches … These sophisticated epistemological perspectives are promoted in the US science education reform documents as both learning goals and teaching approaches. (Kang, 2008, pp. 478, 480).

Jerome Bruner (1915–2016), who was a Harvard colleague of Kuhn (professor of psychology 1945–1972), when speaking of his famous *Process of Education* (Bruner, 1960) that presented a constructivist alternative to didactic, transmissionist, behaviourist-informed 'banking' pedagogy, wrote:

> Its ideas sprang from epistemology and the sciences of knowing … all of us were, I think, responding to the same "epistemic" malaise, the doubts about the nature of knowing that had come first out of the revolution in physics and then been formalized and amplified by philosophy. (Bruner, 1983, p.186)

For Bruner, it might have been his colleague, Kuhn, who amplified the purported 'epistemic malaise' of modern times.

The early twentieth-century revolution in physics understandably fuelled a range of philosophical responses. Some are productive, others are unproductive and misguided. Not all philosophers were moved by the revolution to 'formalized doubts about the nature of knowing'. Among the best philosophers, the revolution prompted epistemological refinement, not scepticism. This was documented in the 1930s by Susan Stebbing (Stebbing, 1937/1958) and subsequently by philosopher-physicists such as Abner Shimony (Shimony, 1963/1993), Mario Bunge (Bunge, 1967b, 1979/2001), and many others. Mario Bunge, in discussing Einstein's two relativity theories, observed:

> In sum, the two relativities, just like classical mechanics, electrodynamics, and thermodynamics before them, and the quantum theory shortly thereafter, have given rise to a number of ontological and epistemological problems. Whereas some of these are legitimate and still worth discussing, others are pseudo problems generated by the attempt to cast science into an inadequate philosophical mould. Moral: Any mismatch between science and philosophy is bound to harm both. (Bunge, 1979/2001, p.243)

16.3 Pedagogical Tension

There was a tension, seldom addressed, between the liberal, humanistic science education of Conant and Kuhn's General Education case studies programme, and the dogmatic, unquestioning science education that Kuhn identifies as required for the practice of normal science. Indeed, for Kuhn, such 'closed minded' education was a sign that proto-science or pre-science was finally progressing along the normal science trajectory and so preparing the scientific community to identify an anomaly and deal with a crisis. It is resolution of the latter that precipitates scientific revolutions. If there is a multitude of diverse scientific beliefs, then there is no paradigm, and there cannot be a crisis. Anomalies can only be recognised against a detailed, established, 'taken for granted' background. Something forged in science classrooms.

Kuhn, notoriously in many quarters, described normal scientists as conservative, uncreative, closed-minded, and dogmatic. Their education made them this way. And it had to so make them. Many have said that Kuhn is describing not the cut and thrust of normal science but sub-normal science. His 1959 address 'The Essential Tension' (Kuhn, 1959a) was his first substantial discussion of the characteristics of effective pedagogy and the cognitive mechanisms involved in learning scientific concepts. His linkage of normal science to distinctive, authoritative science pedagogy is further laid out in an essay, 'The Function of Dogma in Scientific Research' (Kuhn, 1963) that was presented at a 1961 conference organised by A.C. Crombie. Kuhn noted:

> Yet even a cursory inspection of scientific pedagogy suggests that it is far more likely to induce professional rigidity than education in other fields, excepting, perhaps, systematic theology. (Kuhn, 1963, p.350)

In summary:

> … scientific education remains a relatively dogmatic initiation into a pre-established problem-solving tradition that the student is neither invited nor equipped to evaluate. (Kuhn, 1963, p.351)

Educators, and others, saw Kuhn's eye-catching scientific revolutions, paradigm changes, incommensurability, rejection of truth, and other such putative features of science. Passed over was his account of grinding normal science and the tunnel-visioned pedagogical requirements for its practice. For many, he has been identified with progressive, student-centred, inquiry-based teaching (Matthews, 2021b). This is an association from which he would run.

Kuhn's little-known 1990 essay 'On Learning Physics' (Kuhn, 1990) is a careful and valuable contribution to the science of learning. [85] 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)

For Kuhn, if a society wants, or needs, normal science to be conducted, then it requires a rigid science education. Other systems, worldviews, and understandings can be taught, but not as science and not in the science programme. The conceptual apparatus of science needs to be internalised. Much of this needs to be done 'on faith' as school laboratories and equipment are never ideal, and they fall well short of clinical standards. Friction, impurities, and everything else will always be present. Balls do not fall as they should, current does not flow in the quantity it should, pendulums do not swing as they should, and tall plants do not occur in Mendelian ratios. Hanne Andersen noted:

> 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)

This is at stark odds with the pedagogical constructivism so often associated with Kuhn's name. The abject failure of unguided inquiry, and discovery teaching and learning,

[85] 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.

is especially well documented in mathematics, but also in science programmes. This pedagogy is oft-referred to as 'inquiry', 'discovery', 'problem-based', 'student-centred', 'open classroom', 'open-ended', 'real-world', and other such synonyms.[86]

16.4 A Case Study

Ken Tobin is a noteworthy example of Kuhnian impact on education. He is one of the most influential, highly awarded, and most-cited researchers in science education.[87] Appraising the impact of Kuhn on so substantial a researcher provides an informative case study from which to draw some wider conclusions about Kuhn's impact on science education. As for so many others, Ernst von Glasersfeld was Tobin's self-acknowledged conduit to Kuhn (Tobin, 2007, p.292). Tobin acknowledged, and then dismissed, Suchting's critique of von Glasersfeld writing:

> Too often the critiques [by philosophers] were based on analyses of the use of single words and sentences from one text … word-by-word and line-by-line analyses were not convincing when the [constructivist] authors regarded the meaning as constituted in entire texts or collections of texts. (Tobin, 2000, p.242)

The comment is noteworthy because it so clearly displays a disciplinary difference between philosophy and science education: philosophers were schooled to be attentive to, not impatient with, clarity and consistency. In 1991, after his embrace of, or in Kuhn's term 'conversion' to, constructivism Tobin cautioned:

> To become a constructivist is to use constructivism as a referent for thoughts and actions. That is to say when thinking or acting, beliefs associated with constructivism assume a higher value than other beliefs. For a variety of reasons, the process is not easy. (Tobin, 1991, p.1)

This is a big claim calling out for elaboration: What parcel of beliefs, policies or practices can be trumped by constructivist commitment? On the face of it, everything. A year later, Tobin, with a co-author, wrote:

> … 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)

This is, of course, pure empiricism; it is almost a caricature of individualist, subjectivist, looking-out-at-the-world-and-acquiring-knowledge empiricism. But because HPS was not part of education programmes, this similarity between constructivism and its self-proclaimed foe was little noticed.[88] To their credit, social constructivists, who took their

[86] The debate is laid out in contributions to Tobias and Duffy (2009). On the pedagogical failure of constructivist methods, see Kirschner, Sweller and Clark (2006), and Sweller (2009). The literature and arguments are traversed in Matthews (2021a, pp.169–173).

[87] He has authored or edited over 400 books, anthologies, chapters, and papers in science education. He has supervised scores of PhD theses. Google Scholar, as of September 2022, records 25,000 + citations of his work.

[88] On the empiricist foundations of constructivism, see Matthews (1992, 1993, 1994 chap.7).

lead from Lev Vygotsky, did urge the inclusion of society, culture, language, dialogue, and interactions with others, to be added to the sources of an individual's knowledge. For social constructivists, knowledge did not result merely from individuals walking around, looking at the world and having sense impressions.[89]

In the Introduction to his AAAS-sponsored and published anthology, *The Practice of Constructivism in Science Education*, Tobin utilised, as did millions of others, Kuhnian imagery to announce:

> there is a paradigm war waging in education. Evidence of conflict is seen in nearly every facet of educational practice. … [but] there is evidence of widespread acceptance of alternatives to objectivism, one of which is constructivism. (Tobin, 1993, p. ix)

An itemised deficiency of objectivism is that: 'it seeks to identify causal relationships among salient variables' (ibid). For many, of course, it is indeed the aim of education research.

Tobin did, early on, recognise one problem with embracing 'Kuhnian' philosophy, namely, its soft-focused, undisciplined, ambiguous formulation. These ills were not just Kuhnian, but they were, to a degree, there in Kuhn's own writing. In Tobin and Deborah Tippins' words:

> As we have thought about constructivism, we have come to realize that it is not a unitary construct. Every day we learn something new about constructivism. Like the bird in flight, it has an elusive elegance that remains just beyond our grasp. (Tobin & Tippins, 1993, p. 20)

The imagery is poetic, but how anything that remains permanently 'beyond our grasp' can be a useful learning, teaching, or curricular theory in education, or give philosophical guidance in epistemology, ontology, or methodology, is not explained. But, like the bird in flight to nowhere in particular, Tobin pushed on with his advocacy and 22 years later announced his refined position:

> In contrast to the mainstream of research in science education, I advocate a multilogical methodology that embraces incommensurability, polysemia, subjectivity, and polyphonia as a means of preserving the integrity and potential of knowledge systems to generate and maintain disparate perspectives, outcomes, and implications for practice. In such a multilogical model, power discourses such as Western medicine carry no greater weight than complementary knowledge systems that may have been marginalized in a social world in which monosemia is dominant. (Tobin, 2015a, p.1)

He explains polysemia as: 'many meaning systems, hence truths'. And elsewhere:

> Polysemia is a powerful construct that acknowledges people's social positioning in the world as primary frames for what is regarded as socially true. Having accepted a polysemic stance it behooves us not to judge from outside a framework, but to endeavour to step inside to understand what is happening … that is, to adopt an emic perspective. (Tobin, 2015b, p.4)

[89] The educational wing of social constructivism took inspiration from the 1920s writings of the Soviet social psychologist Lev Vygotsky that had been translated into English in the 1960s and 1970s (Vygotsky 1962, 1978).

This is the most common interpretation of Kuhn's philosophy. Tobin not only adopts polysemia and polyphonia, but also polyontology:

> However, since we accept poly-ontology (i.e., the co-occurrence of multiple realities/many ways to answer the question: What happened?), we acknowledge that the frameworks we use, with and without awareness, illuminate and obscure what we experience as researchers. That is, our research frameworks are subjectivities that orientate and inform, but are not unique and should not be privileged to justify inequalities and social harms. (Tobin, 2015a, p.3)

Although it sounds pollyanna-ish, it is a faithful Kuhnian echo. Polyontology prepares the ground for a philosophically seamless segue into Tobin's, website-announced, current occupations:

> Tobin began formal studies of Jin Shin Jyutsu in 2014 and continues to learn through practice of the art of JSJ, undertaking research on use of JSJ and disease such as diabetes 2, and participation in classes offered by Jin Shin Jyutsu (Scottsdale, AZ). As part of his focus on different complementary modalities, Tobin also has studied Integrated Iridology with Toni Miller. Presently he uses both JSJ and iridology in ongoing studies of wellness, mindfulness, and more broadly, contemplative inquiry.[90]

Many Kuhn-influenced science educators had taken the same path. Wolff-Michael Roth, a major figure having 47,000 + Google Scholar citations, and a Tobin collaborator, wrote in a co-authored piece:

> What remains here is the question how to de-privilege science in education and to free our children from the "regime of truth" that prevents them from learning to apply the current cornucopia of simultaneous but different forms of human knowledge with the aim to solve the problems they encounter today and tomorrow. (Van Eijck & Roth, 2007, p.944)

This 'research programme' transition can be tied to Kuhn who, in his 'Afterwords' contribution, wrote:

> The fact that experience within another form of life another time, place, or culture might have constituted knowledge differently is irrelevant to its status as knowledge. (Kuhn, 1993, p.332)

So, there can be alternative, stand-alone knowledge systems such as JSJ, Iridology, Q-Anon, Feng Shui, and whatever else might appear on the scene. The epistemic status of one system has no bearing on the others; they are not competitive; they merely co-exist, and, for some, cannot even be compared. And this is just what Tobin is committed to in with his 'multilogical' methodology whose very purpose is to maintain disparate perspectives, not to compare or adjudicate between them. This is fine, provided nothing serious is at stake. But when there is national research money to allocate, science curricula to write, hospitals and doctors to fund, COVID viruses to find, then comparative judgement is required, and the separation of genuine from bogus science is required.

[90] Wikipedia entry: https://en.wikipedia.org/wiki/Kenneth_Tobin

17 Kuhn's Influence on Education III: Science-Technology-Society (STS) Studies

Kuhn had an impact on Science and Technology Studies (STS) and, as a flow-on, on the new field of Cultural Studies of Science. Since at least Robert Merton and John Bernal in the 1930s (Bernal, 1939; Merton, 1938/1973), there have been comprehensive social studies of science. Since the 1960s, STS has been institutionalised.[91]

Naomi Oreskes,[92] an historian and philosopher, 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)

She is correct. *Structure* was the basic text in the Edinburgh course titled 'A Philosophical Approach to Science'. It speaks volumes that the text of a self-confessed 'amateur' in philosophy, who had never learnt the subject, was chosen in Edinburgh as the way to give students a philosophical understanding of science. Barry Barnes, a co-founder of the Strong Programme, wrote in 1982:

> Kuhn has made one of the few fundamental contributions to the sociology of knowledge. It fell to him to provide, at the time it was most needed in the 1960s, a clear indication of how our own forms of natural knowledge could be understood sociologically. This encouraged the empirical studies of scientific culture. (Barnes, 1982, p.x)

Not long after Kuhn's death in 1996, Clifford Geertz, the preeminent US social anthropologist, stated that Kuhn's *Structure* 'opened the door to the eruption of the sociology of knowledge' into the study of science (Weinberg, 1998). This is not disputed.

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 et al., 1993, p.ix)

17.1 Recurrent Idealism

Kuhn never renounced his anti-realism, he held on to youthful Kantian notions of investigators creating the world, and scientists who worked in different paradigms (later those utilising different lexical structures) lived in different worlds. This ontological idealism flowed through to STS studies. This is on plain view in the classic *Laboratory Life* (Latour

[91] David Edge, a leading STS researcher, provides an informative history (Edge 1994). The full gamut of STS studies is laid out in contributions to Jasanoff et al. (1995).

[92] 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.*

& Woolgar, 1986).[93] Latour had earlier claimed that nothing extraordinary, indeed nothing 'scientific', was happening in research laboratories (Latour, 1983, p.141). In *Laboratory Life*, the authors conclude of science, after Latour's two years of field work in Roger Guillemin's laboratory in the Salk Institute, that:

> Each text, laboratory, author and discipline strives to establish a world in which its own interpretation is made more likely by virtue of the increasing number of people from whom it extracts compliance. (Latour & Woolgar, 1986, p.285)

And further:

> The "out-there-ness" [of the external world] is the consequence of scientific work rather than its 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)

As with so many Kuhn-influenced scholars, they were seriously idealist and avowedly anti-realist. It was human discourse, language, which brought the 'thing' into existence:

> the thing and the statement correspond for the simple reason that they come from the same source. Their separation is the *final stage in the process of their construction*. (Latour & Woolgar, 1986, p.183)

In case the idealism was not fully grasped, Woolgar subsequently wrote:

> There is no sense in which we can claim that the phenomenon … has an existence independent of its means of expression … There is no object beyond discourse … the organisation of discourse is the object. Facts and objects in the world are inescapably textual constructions. (Woolgar, 1988, p.73)

David Bloor had the good sense to describe Latour's procedure as 'obscurantism raised to the level of a general methodological principle' (Bloor, 1999, p.97). Unfortunately, science educators did not share Bloor's opinion. Peter Fensham, in his landmark study of the formation of leading science education researchers, reports: 'One book stood out as an influence about the culture of science and that was Latour and Woolgar's *Laboratory Life: The construction of scientific facts*' (Fensham, 2004, p.58).

STS programmes were widely promoted as a way of making science teaching contextual, meaningful, and avoiding the dogmatism, abstractness, and personal irrelevance of orthodox, disciplinary science courses. Surveying UK education in the time since Snow's 'Two Cultures', David Edge observed:

> The pedagogical context created by such moves gave considerable scope for the incorporation of STS scholarship (especially the humanistic insights of SSK and its derivatives) into the syllabuses of budding scientists and technologists. (Edge, 1994, p.9)

Harry Collins, a leader of the sociology of science programme that claimed Kuhn as its founder, wrote a paper on 'Stages in the Empirical Programme of Relativism' in which he made the head-turning claim that: ' … the natural world has a small or non-existent role in the construction of scientific knowledge' (Collins, 1981, p.3). And a few years later:

[93] The book is widely, and approvingly, cited in science education having 21,500+citations to October 2022. Peter Slezak provided a withering critique (Slezak 1994b).

> It is not the regularity of the world that imposes itself on our senses, but the regularity of our institutions and beliefs that imposes itself on the world. (Collins, 1985, p.148)

So, it is not just nature that goes missing in the creation of science but, seemingly, the individual researcher who functions merely as a conduit for different social regularities that impose themselves on our knowledge of the world. Or in Marxist historiography, as a conduit for class divisions.[94] If taken literally and seriously, Collins' position re-directs the search for, and understanding of, laws of nature and causal relations: We do not look outside at the world; we look inside at our belief systems or around us at our social structures. Of course, this has to be done, as Bacon long ago advised when detailing his 'Idols of the Mind',[95] but at some point, the world has to be looked at, measured, experimented upon, taken into account.[96]

The same influences were at play in the USA, Canada, Europe, Australia, and New Zealand.[97] The NSTA yearbooks of 1984 (Bybee et al., 1984) and 1985 (Bybee, 1985) deal with the rationale and content of such STS school programmes. The NSTA publication *The Science, Technology, Society Movement* (Yager, 1993) reviewed their implementation. The US National Science Teachers Association (NSTA) endorsed the STS orientation to science in its 1971 statement *School Science Education for the 1970s* (NSTA, 1971). This view was repeated in its 1984 and 1985 yearbooks. All of this sounds good and progressive. But, of course, a great deal of the good, if not the entire deal, hinges on what is taught as the 'humanistic insights of SSK'. That is, how accurate is the sociology of scientific knowledge (SSK) account of the nature, methods, and status of scientific knowledge?

Glen Aikenhead, a leading Canadian science educator, summed up STS scholarship when he informed readers that contemporary social studies of science reveal science as:

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

As with nearly all prominent science educators, Aikenhead acknowledges Kuhn's formative influence on him:

> Thomas Kuhn's *Structure of Scientific Revolutions* is one of the few books I've reread several times … It was extremely helpful in my thinking with all sorts of implications for teacher education and everything I did. (In Fensham, 2004, p.56)

Considering the supposed findings of STS, science should not be taught in schools. But before banishing science, the first task is to unpack and then appraise Aikenhead's claims, and the comparable claims of so many other SSK proponents. But this requires that science teachers, administrators, and curriculum writers have some grasp of HPS. What science is being described? Where? When? At different times and places, all of these ills have been manifested in science as practised: Maoist science, Nazi science, Soviet science, Islamic science, military-industrial-complex science, Christian science, Feng Shui science, Vedic

94 The classic statement was Boris Hessen's 1931 'The Social and Economic Roots of Newton's *Principia*' (Hessen 1931/2009). It was oft enough said that Darwinian 'Survival of the Fittest' was just Victorian England transferred to the animal world.

95 *New Organon*, 1620, Aphorisms 31–46.

96 One critical account of STS studies is Maxwell (2015).

97 See at least Bybee (1977) and Solomon and Aikenhead (1994).

science, and creation science. The list can go on. Whether indigenous science should be added to the list is a much debated and very consequential question.[98] There has been consistent effort to show that these are inadequate and failed, if not corrupt, sciences. Aikenhead's proffered summary of HPS scholarship if off the mark.

HPS-informed teachers, administrators, and curriculum writers can strive for putting the best of science, and its history, into school programmes while also point to corruptions, detours, and pseudosciences. Examples can be given and fleshed out. The importance of such HPS competence is manifested in an Editorial published in a 2022 issue of *The Journal of Science Teacher Education*. The editorial was titled: 'Challenging the Hegemony of Western Scientism in Science Teacher Education' (Melville et al., 2022). The editorial was to be: 'both a *mea culpa* and a call to recognize and challenge the hegemony of Western scientism in the science education literature'. Readers are warned: 'the power of scientism is often just below the surface, ready to reassert itself in our lives' (ibid., p.706). A prerequisite for any intelligent engagement in this question is some knowledge of HPS, minimally the ability to separate scientism from pseudoscientism.

Scientism began with Newton who proposed extending the methods of natural philosophy to the subject matter of the moral sciences. This was the core programme of the Enlightenment. Contrary to the impression given in the editorial, many philosophers have extended and defended that core programme.[99] They are not mentioned in the editorial, rather readers are advised: 'we can all renegotiate our identities in relation to scientism' (Melville et al., 2022, p.707). This is another missed opportunity for promotion of HPS in science education. Without solid HPS training, teachers are just moved by faddism, in this case about what scientism is and is not.

Peter Fensham documented how the bulk of science educators have zero formal training in philosophy, psychology, sociology, or history—the education foundations subjects—yet have to teach courses related to those fields.[100] His conclusion is sobering:

> science educators borrow psychological theories of learning … for example Bruner, Gagne and Piaget …The influence of these borrowings is better described as the lifting of slogan-like ideas. (Fensham, 2004, p.105)

The sorry picture applies even more to philosophical theories. There, educators are even more susceptible to faddish views and the lifting of slogans.

18 Kuhn's Influence on Education IV: Cultural Studies of Science

Just as Kuhn-influenced STS studies found a home in education, so too did Kuhn-influenced cultural studies of science find a home.[101] In the early 1990s, the journal *Science as Culture* was published in order to provide 'research space' for anthropological studies of

[98] For how the issue has played out in New Zealand, see Corballis et al. (2019).

[99] See Ross et al. (2007) and Ladyman (2018) and contributions to Boudry and Pigliucci (2017).

[100] Fensham says that when he was appointed to Monash University in 1967, as Australia's first professor of science education, a master's degree in one of the foundations of education—history, philosophy, psychology, or sociology—was a prerequisite for doctoral studies. But he removed the requirement (Fensham 2004, p.23). Monash's foundation-free pattern is now the norm for science education doctoral programmes in Australia, and internationally.

[101] An authoritative insider's account of how this happened is given in Tobin (2015b). See also contributions to Milne et al. (2015).

science of the kind promoted by the Edinburgh 'Strong Programme' and exemplified in Bruno Latour and Steve Woolgar's above-mentioned *Laboratory Life* (Latour & Woolgar, 1979). Just as the anthropology of religion is not concerned with the truth or falsity of religious claims, or even the strength or weakness of specific religious arguments, so too this approach to studying science was consciously 'truth neutral'; it was to be 'naturalistic', meaning that truth, better evidence, or good warrant could not be appealed to as the explanation of scientific consensus, much less scientific progress. Claims were not believed because they were true, or positions adopted because there were epistemically good reasons for so doing. It was external pressures or, at best, personal interests that shaped, powered, or caused belief and change of belief.

Pierre Bourdieu (1930–2002), a French philosopher and sociologist of education, utilised this naturalistic methodology in appraising the philosophical, historical, and sociological arguments occasioned by *Structure*:

> The reactions provoked by Thomas Kuhn's book, *The Structure of Scientific Revolutions,* show this very clearly, and would provide high-quality experimental material for an empirical analysis of the ideologies of science and their relationship with their authors' positions in the scientific field. (Bourdieu, 1999, p.44)

There are not 'better' arguments, just more compelling personal and social pressures.

Many adopted this position. David Bloor titled one article: 'The Sociology of Reasons: Or Why "Epistemic Factors" are really "Social Factors"' (Bloor, 1984, pp.295–324). Steven Shapin proposed a thorough-going sociological account of the history of science (Shapin, 1982). The research avenues and questions for the new field were sign-posted in the 15-chapter landmark anthology *Science as Practice and Culture* edited by Andrew Pickering, a founder of the discipline (Pickering, 1992).

The Pickering anthology appeared the same year that the journal *Science & Education: Contributions from History and Philosophy of Science* was launched. The first editorial of this journal stressed the need for clear communication and the avoidance of jargon in scholarly writing. As if to show the relevance of the editorial, three years later, Pickering published a book, *The Mangle of Practice: Time, Agency and Science* (Pickering, 1995). This contained many challenging sentences, including the following:

> The dance of agency, seen asymmetrically from the human end, thus takes the form of a *dialectic of resistance and accommodation*, where resistance denotes the failure to achieve an intended capture of agency in practice, and accommodation an active human strategy of response to resistance, which can include revisions to goals and intentions as well as to the material form of the machine in question and to the human frame of gestures and social relations that surround it. (Pickering, 1995, p.22)

These 80 words constitute more than a purple passage, it is an incomprehensible sentence. The sentence suggests that a better title for the book would have been *The Mangle of Language.* However, it was a trailblazer, and it set the tone for writing in the new field. If this sentence gets through review, copyediting, and into print, what can be kept out?

In 2006, Kenneth Tobin and Wolff-Michael Roth co-founded the Springer journal *Cultural Studies of Science Education.* The journal's first editorial announced:

> The journal encourages empirical and non-empirical research that explores science and science education as forms of culture … It was anticipated that the forms of dissemination will make visible the non-linearity of doing research and the recursive nature of delineating problems. (Tobin & Roth, 2006, p.1)

From the foundational 2006 editorial, the journal has been home to, one might charitably say, less than clear writing.[102]

The journal's founding editors, in the Introduction to an anthology of 'Sociocultural and Cultural-Historical [Research] Perspectives', write:

> If, on the other hand, we begin with the ontological assumption of difference that exists in and for itself, that is, with the recognition that A↓A (e.g., because different ink drops attached to different paper particles at a different moment in time), then all sameness and identity is the result of work that not only sets two things, concepts, or processes equal but also deletes the inherent and unavoidable differences that do in fact exist. This assumption is an insidious part of the phallogocentric epistemology undergirding science as the method of decomposing unitary systems into sets of variables, which never can be more than external, one-sided expressions of a superordinate unit. (Roth & Tobin, 2007, pp.99-100)

Comment is not needed, and could not be made anyway. Except to note that the passage was composed by the two most-cited, and probably most-awarded, researchers in the discipline of science education.[103]

Earlier Roth had laid out clearly his multi-world, idealist ontology:

> according to radical constructivism, we live forever in our own, self-constructed worlds; the world cannot ever be described apart from our frames of experience. This understanding is consistent with the view that there are as many worlds as there are knowers. (Roth, 1995, p.13).

And then proceeds:

> Radical constructivism forces us to abandon the traditional distinction between knowledge and beliefs. This distinction only makes sense within an objective-realist view of the world (Roth, 1995, p.14).

And concludes, for those who could not draw the lesson:

> Through this research [sociology of science], we have come to realize that scientific rationality and special problem-solving skills are parts of a myth. (Roth, 1995, p.31).

The effect of Kuhn's idealism is apparent. It is of some note that the claims were published in a book part-titled: *Authentic School Science*. Is that really the understanding of science that any decent society wants its citizens to have? It is unfortunate that promotion of these claims does local harm in a graduate classroom. But when countries are trying to deal with pandemics, climate change, environmental degradation, and other pressing real issues, promotion of such views does national harm; they consistently erode trust in science and open doors to countless alternatives.

In 2011, two researchers asserted, in a *Cultural Studies* article, the now disciplinary commonplace:

> Recent scholarship in science studies [STS] has opened the way for more thoughtful science education discourses that consider critical, historical, political, and

[102] For a philosophical review of the cultural studies research genre, with an abundance of illustrative quotations, see McCarthy (2014).

[103] Wolff-Michael Roth 47,004 citations (September 2022), Ken Tobin 25,173 citations (September 2022).

> sociocultural views of scientific knowledge and practice …. 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)

Each sentence is either false or lacks supporting argument. The first completely ignores the 150+years of HPS scholarship that has richly documented the 'historical, political and sociocultural views of science'. And this scholarship has, for the same length of time, been utilised in European, UK, and US science education (Matthews, 1994, chaps.4, 5). Consider, for instance, the rich understanding of science displayed in the writings of even the much-maligned archetypical positivists Ernst Mach, Philipp Frank, and Herbert Feigl.[104]

The authors of a 2020 CSSE paper maintain:

> we highlight the post-critical curricular perspectives to problematize discursive demands and articulations as part of processes of struggle for the fixation of particular meanings in the field of science education. With emphasis on discourse theory and categories such as discourse, articulation, hegemony and antagonism, we sought to identify hegemonic and counter-hegemonic discourses defended in the struggle for curricular proposals of science education and teacher training in Brazilian scholarship and some international examples. (Rezende & Ostermann, 2020, p.1047)

In case the project was not clear, they followed with a clarifying elaboration:

> Traditional curricular and teacher-training projects were seen as products of discursive articulations in defence of the universalism of Western Modern Science (*knowledge itself*) and of the technical rationality. (ibid)

Although the enormously influential, social-constructivist Edinburgh programme was, beginning 30 years ago, convincingly criticised,[105] confidence about universal science has been widely diminished. Kuhnianism has been a significant factor in the rise of relativism (all views are equally good) and scepticism (we cannot know anything) concerning knowledge of the natural, social, cultural, and moral worlds (Barnes & Bloor, 1982; McIntyre, 2018, 2019). Such relativism and scepticism are depressingly common in science education.[106] To be a sensible fallibilist (there is no perfect, cannot-be-improved knowledge) is not to be a relativist. A point oft not appreciated.

As with many writers, 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 senior Australian science educator, 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)

[104] For texts and elaboration on Mach, see Matthews (1990, 2019b); for Frank and Feigl, see Matthews (2004b).

[105] See minimally Bunge (1991, 1992) and Slezak (1994a, b).

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

No evidence is adduced for these sweeping claims; no argument is provided for why 'finding a truth for reality is highly questionable'. The one piece of supposed evidence is an unpaginated reference to Kuhn.

The practice of having an unpaginated Kuhn reference substitute for evidence, or argument, is widespread in science education. It is almost the disciplinary norm. Merely putting the name 'Kuhn' in brackets after some claim is widely regarded as sufficient warrant for making the claim, no matter how outrageous, self-contradictory, or ill-supported it might be. Cathleen Loving and William Cobern, in a study of science education citations of *Structure*, found, staggeringly and to the disgrace of the discipline, that only 1.5% (144 out of 9715) provided a page reference. The rest were 'generic' references, and the Kuhn's name was good enough (Loving & Cobern, 2000, p.194). Needless to say, those citing Kuhn paid no attention to the published arguments that challenge or refute the claim made on his behalf, even when the claims were refuted by Kuhn himself!

Sal Restivo, a sociologist and former president of the Social Studies of Science Society (4S), identified this, to put it overly kindly, academic malaise:

> By the early1980s 'T.S. Kuhn' had become a cultural resource more or less detached from T.S. Kuhn, his writings, and the social contexts of his arguments. 'Kuhn' has served the interests of left, right, and center across the entire spectrum of intellectual discourse. (Restivo, 1994, p.99)

19 Conclusion

Kuhn deserves credit for taking the history and philosophy of science into the education, academic, and cultural domains. Unfortunately, what went into these public domains was, at best, Kuhn I or 'radical' Kuhn, not Kuhn II or 'mature' Kuhn. The vehicle for Kuhn I was his monumental 1962/1970 *Structure*. This had a, so to speak, four-cylinder engine: physics, philosophy of science, history of science, and sociology of science. But, as has been shown, the latter three of these cylinders progressively gave up as Kuhn drove the vehicle down the research road. Many key elements of radical philosophy were, under the weight of criticism, gradually abandoned. But he held on to enough—relativism and idealism—to cause trouble. His revolutionary apparatus for history of science turned out not be used even by himself in his major historical study on the early twentieth-century quantum revolution. He conducted no research in sociology of science to support his claims about decisive processes within the scientific community.

Philosophers cannot be entirely responsible for the writings and arguments of their followers. But, given the deleterious impact of Kuhn-inspired philosophy in educational constructivism, in STS studies, in Cultural Studies of Science, in fuelling post-modernism in education, and in misdirecting debates about inclusion of indigenous knowledge in science programmes, he should have been more considered and careful in his writing. Cultivating a more orthodox philosophical 'cast of mind' would have done no harm, and would have done considerable good, both inside and outside the academy. Kuhn had ample opportunity at Harvard and beyond to acquire a good philosophical education, but he declined. And he half-thought he would be better off without it, avowedly saying that getting a philosophical education would have been at the cost of writing *Structure*.

But if Kuhn is to be blamed for writing purple passages and so carelessly affirming relativist and idealist positions, then surely the science education community (and others) needs to be blamed for their uncritical and ill-informed reading of his work. With enthusiasm, they embraced Kuhn I and ignored his own re-making of himself into Kuhn II. And they ignored pretty much all critics of Kuhn. The community *en masse* mistakenly thought Kuhnian pedagogy was constructivist, child-centred, inquiry-powered pedagogy. Kuhn was as clear as can be that the pedagogical preparation for normal science was the antithesis of this. These failings of the science education community were disciplinary rather than an individualist; individuals are not to be blamed. The failings will not be rectified until philosophy and philosophical thinking returns, in some degree, to undergraduate and graduate programmes in education.[107]

The following are some steps that could be taken:

1. Inclusion of some education-related HPS course in the preparation of science teachers is a necessity. Ideally, it means the creation of specific courses that pick up tangible theoretical, curricular, and pedagogical topics in science teaching that teachers can identify and recognise as genuine classroom and curricular problems; then, elaborate how HPS considerations can contribute to the better understanding and resolution of the issue. There is a massive 'engagement' difference between sending trainee teachers off to a philosophy department to do a HPS unit and providing an education orientated HPS course for them (Matthews 2014). One-day, multi-presentation, in-service workshops for teachers conducted by scientists, historians, philosophers, and sociologists are one small way this can be done.[108]
2. Encourage science teachers doing higher degrees in education to undertake one or more formal courses in HPS. Even dissuade them from a higher degree in education in favour of an undergraduate degree in HPS; after that, do a PhD in Education. This is good for their personal growth, and it is ultimately beneficial to whatever education research programme with which they might engage.
3. Ensure that PhD committees in science education have foundations faculty on them. The participation of a psychology, philosophy, history, or sociology researcher on thesis committees, along with a science professor, would contribute to raising candidate and supervisor awareness of past, and current, issues and literature in the relevant disciplines. It is noteworthy that in all the contemporary literature on activist and social justice science education, there is very little attention to how easy indoctrination could be, and has been, substituted for education.

   Notoriously, promoting a 'mature' or 'informed' NOS position is simply taken to be promoting the NOS position of the instructor or examiner (Matthews, 1998b). Indoctrination replaces education. Without some grounding in philosophy of education, it is difficult to even recognise the distinction, let alone weigh up appropriate responses in programmes where it is practiced.[109]
4. Try as much as possible to ease publication pressure so that new faculty can effectively pursue their own reading and scholarship. Far better for newly appointed science educators to spend a semester attending a philosophy, psychology, linguistics, or history

[107] On philosophy in teacher education programmes, see contributions to Colgan and Maxwell (2020) and Matthews (2015 chap.12, 2020). On ways to incorporate HPS into classrooms, see substantial studies in McComas (2020).

[108] For programmes of three such annual in-service workshops, and teacher evaluations, see Matthews (2021a, pp. 264–268).

[109] See Hannson (2018) and discussion in Matthews (2021a, pp. 277–279).

course, and reading substantial books, than conducting yet another study of misconceptions or the impact of talking on classroom learning. Better that a few things be done well than a hundred things be done poorly.

5. Work towards a system of joint appointments between education and foundation disciplines. Encouragingly, this happens to a small extent between education schools and science departments; if other faculty could be cross appointed to philosophy or HPS or psychology, this would assuredly lift the quality of scholarship and research in education and broaden scholarship and social utility in the foundation field.[110]

Declarations

Conflict of Interest The author declares no conflict of interest.

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[110] Israel Scheffler (Harvard) and Denis Phillips (Stanford) had appointments in both education and philosophy with manifest benefit to their institutions and graduate programmes. Harvey Siegel, for instance, was an education student whose thesis was supervised by Scheffler. And there are other such exemplars.

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