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Kuhn and conceptual change: on the analogy between conceptual changes in science and children

Christian Greiffenhagen · Wendy Sherman

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Abstract This article argues that the analogy between conceptual changes in the history of science and conceptual changes in the development of young children is problematic. We show that the notions of 'conceptual change' in Kuhn and Piaget's projects, the two thinkers whose work is most commonly drawn upon to support this analogy, are not compatible in the sense usually claimed. We contend that Kuhn's work pertains not so much to the psychology of individual scientists, but to the way philosophers and historians should describe developments in communities of scientists. Furthermore, we argue that the analogy is based on a misunderstanding of the nature of science and the relation between science and common sense. The distinctiveness of the two notions of conceptual change has implications for science education research, since it raises serious questions about the relevance of Kuhn's remarks for the study of pedagogical issues.

Keywords Conceptual change · Misconceptions · Paradigm · Kuhn · Piaget

1 Introduction

Thomas Kuhn's work in the history and philosophy of science has played a highly influential role in the programme of Conceptual Change in science education (CC)[1]. Adapting Kuhn for their own purposes, proponents of CC have argued that the

[1] For recent overviews of CC see Chi and Roscoe (2002), Duguit and Treagust (2003), and Leach and Scott (2003).

C. Greiffenhagen (✉)
Department of Sociology, University of Manchester, Manchester M13 9PL, UK
e-mail: Christian.Greiffenhagen@manchester.ac.uk

W. Sherman
College of Education, Health and Human Services, Kent State University, 404 White Hall, TLCS Department, Kent, OH 44242, USA
e-mail: wshermal@kent.edu

conceptual changes that young children undergo in school science classrooms, when they are asked to make the move from an intuitive way of understanding natural phenomena to a scientific one, resembles conceptual changes in the history of science.[2] On this reading, historical studies of paradigm changes in science can help us understand the conceptual problems faced by pupils in school science. For example, Hewson (1981, p. 383) argues:

> Learning science is complex – a student has at different times to acquire new information, reorganize existing knowledge and even discard cherished ideas. In addressing this issue, the analogy between individual learning and conceptual change in scientific disciplines has been fruitful in providing aspects of a suitable framework for analysing science learning.

Driver (1983, pp. 9–10) asks:

> [...] children sometimes need to undergo paradigm shifts in their thinking. Max Planck suggested that new theories do not convert people, it is just that old men die. If scientists have this difficulty in reformulating their conceptions of the world, is it a wonder that children sometimes have a struggle to do so?

Vosniadou and Brewer (1987, pp. 54–55) express the analogy in the following way:

> According to Kuhn, the exercise of 'normal science' involves the articulation of an existing paradigm that may result in theory change. Only when these attempts at articulation fail repeatedly does the motivation for a true paradigm shift arise. Paradigm shifts happen in an effort to resolve anomalies that exist in the relation of existing theory to observations (Kuhn [1996], p. 97). The development of knowledge in the child can be seen in similar terms, as a process of enriching and elaborating existing 'theories' that can give rise to theory change, in other words to weak restructurings. Occasionally, when the child is faced with major anomalies that existing conceptual structures cannot account for, a new paradigm is required, giving rise to radical restructurings.

As part of research in science education, CC investigates the ways in which what pupils already know impacts on what they are supposed to learn in school science. One fundamental assumption of CC is the view that pupils' prior knowledge—alternatively called 'naïve knowledge' (Vosniadou 1989), 'naïve beliefs' (McCloskey 1983; Reiner et al. 1988), 'informal science' (Driver et al. 1994), or 'children's science' (Osborne 1980; Gilbert et al. 1982; Kuhn 1989b)—is 'in conflict' or 'in competition' with what they are supposed to learn in school science. The problem for science education then seems to be one of inducing conceptual change in pupils, from naïve/informal to scientific knowledge.[3] Given

[2] The analogy is discussed, for example, in Gruber (1973), Cawthron and Rowell (1978), Hewson (1981), Clement (1982), Siegel (1982), Posner et al. (1982), Driver (1983), Gibson (1985), Carey (1986), Kitchener (1987), Vosniadou and Brewer (1987, 1992), Kitcher (1988), Kuhn (1989), Nersessian (1989), Vosniadou and Ioannidis (1998), Garrison and Bentley (1990), Matthews (2000, 2003).

[3] We acknowledge that the mechanism for conceptual change from informal to scientific ideas about the world is contested (see Duit and Treagust 2003 for a review). Despite these differences, there seems to be widespread agreement that everyday and scientific conceptions of the world are disparate enough for learning in science to require some sort of psychological transition in pupils analogous to Kuhn's 'paradigm shift'.

this picture, the attraction of Kuhn's work for practitioners in CC is immediately apparent: conceptual changes that supposedly occur in school science classrooms are seen as analogous to paradigm changes in the history of science documented by Kuhn (e.g., the change from Ptolemaic to Copernican astronomy). The perhaps most famous formulation of the analogy is by Posner et al. (1982, p. 212):

> Contemporary views in philosophy of science suggest that there are two distinguishable phases of conceptual change in science. Usually scientific work is done against the background of central commitments which organize research. These central commitments define problems, indicate strategies for dealing with them, and specific criteria for what counts as solutions. Thomas Kuhn [1996] calls these central commitments 'paradigms' and paradigm-dominated research 'normal science'. [...] The second phase of conceptual change occurs when these central commitments require modification. Here the scientist is faced with a challenge to his basic assumptions. If inquiry is to proceed, the scientist must acquire new concepts and a new way of seeing the world. Kuhn terms this kind of conceptual change a 'scientific revolution'. [...]
> We believe there are analogous patterns of conceptual change in learning. Sometimes students use existing concepts to deal with new phenomena. This variant of the first phase of conceptual change we call *assimilation*. Often, however, the students' current concepts are inadequate to allow him to grasp some new phenomenon successfully. The student must replace or reorganize his central concepts. This more radical form of conceptual change we call *accommodation*.

In this article we argue that the analogy between conceptual change in science and conceptual change in children is misleading (see also Levine 2000). We contend that proponents of CC misread Kuhn and do so as the result of a mistaken view about the nature of school science and the relationship between science and common sense. We do not disagree with Kuhn's analysis of conceptual change in the history of science, but wonder whether that analysis can be fruitfully applied to what happens in school classrooms.

In our view the analogy suggests that pupils are akin to scientists (engaged in building explanations of the natural world) and that what pupils know, their common-sense knowledge of the natural world, has the same 'purpose' or 'aim' as a scientific theory (since a paradigm change has to occur between *comparable* entities). The analogy is thus based on the assumption that science and common sense are both theoretical schemes and can therefore be, in some way or other, in competition. If that were the case, then it would seem sensible to argue that pupils should adopt scientific instead of ordinary ways of speaking (cf., Maxwell and Feigl 1961). However, we wonder whether science and common sense really are in conflict in the sense intimated by CC researchers. In our view, what pupils learn in school does not stand in conflict with what they already know, but is better characterized as a (selective) specialization of their prior knowledge.[4]

[4] The position that scientific conceptions must replace 'naïve' or everyday conceptions has been critiqued within the CC literature. For example, Driver et al. (1994) argue that scientific and everyday conceptions probably operate in 'parallel' with one another in a student's conceptual ecology. However, this is not what we are saying in suggesting that scientific knowledge is an extension or refinement of everyday knowledge. As we read arguments such as Driver et al.'s, we understand them to be talking about scientific and everyday knowledge as two conflicting-but-equivalent entities, i.e., as having the same purpose, whereas we want to suggest that the two are not sufficiently alike to be in conflict.

In order to demonstrate that CC's picture of conceptual change is 'incommensurable' with Kuhn's picture of conceptual change we give a brief review of Kuhn's main arguments. Since proponents of CC have noted that Kuhn mentions the developmental psychologist Piaget, we then discuss the possible contribution of Piaget's writings to Kuhn's project (since this would seemingly licence the analogy between developments in the psychology of children and developments in the history of science). We will then use this discussion of Kuhn to review the relationship between common sense and school science.

2 The view of Kuhn in science education

Kuhn's writings, especially his *The Structure of Scientific Revolutions* (1996), are widely known in science education. Although Loving and Cobern (2000) found that the majority of citations to Kuhn made in *The Journal of Research in Science Teaching* and *Science Education* between 1985 and 2000 were overwhelmingly 'supportive of Kuhnian positions' (p. 201), more recently, some strongly critical voices have emerged (Matthews 2003; special issue of *Science & Education* 2000). In what follows, we want to argue for an understanding of Kuhn's work that does not fit neatly into either side of the 'for' or 'against' debate currently being played out in science education.

In our view, Kuhn is grappling with the question, 'Do we have a good *picture* of science?'. Kuhn wants to examine standard textbook presentations in which science is portrayed as continuously cumulative and straightforwardly progressive. These textbook presentations, says Kuhn, give a distorted reading of the history of a science, looking at its past achievements only in relation to its present concerns. In contrast, Kuhn recommends a more 'anthropological' perspective, i.e., to approach earlier scientific theories as one would approach an alien culture, where what might seem strange to 'us' might make sense to 'them'.

In *Structure*, Kuhn argues that there is progress in science, in the sense that the mature natural sciences have developed out of a pre-paradigm stage, and into the much more complex and sophisticated condition they are now in. However, he objects to the view that this evolution is progress towards anything. Science evolves '*from* primitive beginnings but *toward* no goal' (p. 172). The reason for saying that a science does not evolve toward a goal is that Kuhn claims that there is no independent standard according to which scientists can judge whether one theory (e.g., Ptolemaic) is better than another (e.g., Copernican). In other words, Kuhn argues that scientific theories cannot be compared wholesale. Of course, one can compare theories with respect to specific questions (e.g., whether they give a more satisfying explanation for a certain anomaly), and this may influence the choice of theory. However, this does not mean that one theory is 'better' in *every* aspect. In our view, Kuhn aims to steer a careful path between both the 'realist' extreme (that there is a reality that can be appealed to for judging which theory is better) and the 'relativist' extreme (that nothing constrains scientific theories).

3 Kuhn's view of conceptual change in science

We want to emphasize three issues in Kuhn's argument that have a bearing on the possibility of applying Kuhn's ideas to what happens in school science. These are:

(1) the target of Kuhn's writings, which is the picture or image of science;
(2) the nature of a paradigm or conceptual scheme, which could be characterized as a shared set of beliefs providing scientists with a programme of work;
(3) the scope or scale of Kuhn's notion of conceptual change, which is change at the level of science (as a social institution) rather than scientists (as individuals).

First, Kuhn is concerned with how outsiders and insiders understand what science 'is', i.e., with the image or picture of science. He starts the introduction to *Structure* with the following observation:

> History, if viewed as a repository for more than anecdote or chronology, could produce a decisive transformation in the image of science by which we are now possessed. That image has previously been drawn, even by scientists themselves, mainly from the study of finished scientific achievements as these are recorded in the classics and, more recently, in the textbooks from which each new scientific generation learns to practice its trade. Inevitably, however, the aim of such books is persuasive and pedagogic; a concept of science drawn from them is no more likely to fit the enterprise that produced them than an image of a national culture drawn from a tourist brochure or a language text. This essay attempts to show that we have been misled by them in fundamental ways. Its aim is a sketch of the quite different concept of science that can emerge from the historical record of the research activity itself. (Kuhn 1996, p. 1)

Kuhn is drawing attention to the fact that most presentations of science look at the past only from the point of view of its contribution to present theories, methods, and problems. Textbooks, for Kuhn, are very successful pedagogic exercises, but they are a poor source for a (historical-philosophical) image of science, since they give a misleading picture of how the enterprise of science works and changes. Kuhn's *Structure* is an attempt to come up with an alternative picture.

In order to provide that alternative, Kuhn adopts an 'anthropological' perspective and treats earlier scientific communities as 'alien' cultures, which have to be understood in their own terms and which may initially seem strange to us:

> A historian reading an out-of-date scientific text characteristically encounters passages that make no sense. That is an experience I have had repeatedly whether my subject was an Aristotle, a Newton, a Volta, a Bohr, or a Planck. (Kuhn 1989a, p. 9)

Kuhn's concept of 'incommensurability' is an attempt to draw attention to the fact that while earlier scientific theories may seem strange to us, this does not mean that they are less 'rational' or 'scientific' than our current theories, and may have made perfect sense to the scientists of the time. In other words, Kuhn objects to the tendency to portray aspects of earlier scientific theories that are in conflict with our current theories as 'mistakes' or 'confusions' without first making sure that, in the context in which those ideas were coined, they would not make very good sense:

> Incommensurability is a notion that for me emerged from attempts to understand apparently nonsensical passages encountered in old scientific texts. Ordinarily they had been taken as evidence of the author's confused or mistaken beliefs. My experiences led me to suggest, instead, that those passages were being misread: the appearance of nonsense could be removed by

> recovering older meanings for some of the terms involved, meanings different from those subsequently current. (Kuhn 1990, p. 4)

Kuhn does not provide textbook introductions to, for example, Ptolemaic astronomy, but accounts of how to *look at* Ptolemaic astronomy. Kuhn thus argues against a ‘Whig history’ of science (cf., Butterfield 1931), which looks at scientific developments in the past solely in terms of the present and treats earlier theories as underdeveloped precursors to current ones. Instead, Kuhn wants to understand earlier scientific theories from the point of view of the scientists of the time. The effect of this anthropological approach is to present an alternative picture of science from that communicated through the textbook tradition.

Second, paradigms for Kuhn constitute the working assumptions of scientists. That is to say, Kuhn characterizes normal science as puzzle solving, with paradigms the source of the puzzles scientists seek to solve. For Kuhn, a paradigm is a programme of work and not just a cognitive ontological worldview. Consequently, conceptual change for Kuhn is not so much a change in ‘looking’ but in ‘doing’ (cf., Sharrock and Read 2002, p. 162). A paradigm gives scientists things to do (puzzles to solve, measurements to make). Although what scientists ‘believe’ may be important, what they actually do as their day-to-day work is even more so. If we may put it this way: Kuhn’s focus is not just what is in scientists’ ‘heads’, but what is in their ‘hands’.

Third, Kuhn’s remarks about ‘normal science’ and ‘scientific revolutions’ in *Structure* are meant to apply at the level of science as a social institution, and not at the level of the psychology of individual scientists:

> Both normal science and revolutions are, however, community-based activities. (Kuhn 1996, p. 179)

> Again, many of my generalizations are about the sociology or social psychology of scientists. (Kuhn 1996, p. 8)

That is to say, a paradigm or conceptual scheme is the property of the scientific group:

> [...] if I am talking at all about intuitions, they are not individual. Rather they are the tested and shared possessions of the members of a successful group, and the novice acquires them through training as a part of his preparation for group-membership. (Kuhn 1996, p. 191)

As Sharrock and Read (2002, p.115) put it:

> The difference between revolutionary and normal science is a difference in the state of science rather than in the inclinations of individuals.

Thus, a scientific revolution for Kuhn occurs when the whole community of scientists changes direction. For example, according to Kuhn (1957), Copernicus was predominantly working within the old Ptolemaic paradigm and only proposed a minor alteration to it. In a certain sense, Copernicus did not himself realize what he was proposing, since it was only his successors that saw the radical implications in his work (a similar thing could be said with respect to Planck who did not see what Einstein later ‘found’ in Planck’s work). As Kuhn (1957, p.183) puts it:

> The work of a single individual may play a preeminent role in such a conceptual revolution, but if it does, it achieves preeminence either because like the *De Revolutionibus*, it initiates revolution by a small innovation which

> presents science with new problems, or because, like Newton's *Principia*, it terminates revolution by integrating concepts derived from many sources. The extent of the innovation that any individual can produce is necessarily limited, for each individual must employ in his research the tools that he acquires from a traditional education, and he cannot in his own lifetime replace them all.

That is to say, although in retrospect (from the perspective of the historian) it may be possible to characterize what Copernicus did as 'revolutionary', this was not how Copernicus himself saw his work. Copernicus only proposed a minor alteration (a moving earth) within the old system (keeping the sun as revolving around the earth). For Copernicus, the proposed modification of the Ptolemaic system constituted no radical change. As Kuhn (1977 [1959], p. 227) puts it:

> Almost none of the research undertaken by even the greatest scientists is designed to be revolutionary, and very little of it has any such effect. On the contrary, normal research, even the best of it, is a highly convergent activity based firmly upon a settled consensus acquired from scientific education and reinforced by subsequent life in the profession. Typically, to be sure, this convergent or consensus-bound research ultimately results in revolution.

Kuhn argues against the picture of the individual 'revolutionary' scientist. Put very succinctly: the Copernican revolution in science was not a scientific revolution 'in' Copernicus. The Copernican revolution described by Kuhn, in contrast, occurred over the course of a century and concerned the status of a whole community of scientists. Kuhn emphasises that although the historian can look at the history of science and identify conceptual changes, these rarely (if ever) occur in individuals:

> How, then, are scientists brought to make this transposition? Part of the answer is that they are very often not. Copernicanism made few converts for almost a century after Copernicus' death. (Kuhn 1996, p. 15)

The conceptual changes that are identified by Kuhn occurred over time and were brought about by individuals who themselves did not change. Scientific revolutions are nobody's intent. Kuhn's conceptual change is thus best understood as *cultural-collective* change at the level of the community of scientists—not as cognitive-psychological change at the level of individual scientists.

Kuhn does talk about individuals, but in the context of undergraduate and graduate education, that is, in the context of the socialization of novices into the scientific community. However, in this situation Kuhn does not talk of 'revolutions', 'conceptual changes', or 'world changes', since the paradigm that students are socialized in is the first paradigm that they encounter. As he puts it in the Postscript to *Structure*:

> A scientific community consists, on this view, of the practitioners of a scientific speciality. To an extent unparalleled in most other fields, they have undergone similar educations and professional initiations; in the process they have absorbed the same technical literature and drawn many of the same lessons from it. (Kuhn 1996, p. 177)

In his characterization of the socialization process, Kuhn does *not* talk of 'conceptual change', but instead outlines how successful textbook presentations of science are for socialising novice scientists. Science education, according to Kuhn, is typically authoritarian and dogmatic (cf. Kuhn 1977 [1959]), a matter of getting novices 'up to speed' in the current ways of the discipline. Kuhn emphasises that this is not necessarily a bad thing, since it is this successful induction into an established tradition that lays the groundwork for the possibility of later revolutions.

In our view, these three issues raise serious questions with respect to the applicability of Kuhn's ideas to what happens in school science.

First, it would seem that the impact of Kuhn should be in school philosophy or history (where pupils are taught how to look at, e.g., science) rather than school science (where pupils are taught results and methods of certain scientific theories). It seems to us that the main task of school science is not to provide philosophical views about science, but to give an introduction to a particular scientific paradigm by familiarising students with basic examples of scientific 'good practice', albeit perhaps on a simplified base using outdated procedures. Classroom exercises are only the beginning of the long educational journey that a student must make if he or she wants to become a professional scientist. In other words, school science resembles the textbook presentations that Kuhn thought were successful as pedagogic devices, but not adequate as philosophical accounts about science.

Second, Kuhn emphasises that scientists are engaged in professional work, and this is certainly not the same kind of work in which school pupils take part. In other words, school children do not form a community that shares similar goals and objectives that they are trying to accomplish through science (this issue will be taken up below).

Third, the changes that Kuhn talks about occur at the level of the scientific community. However, science education wants to induce conceptual change in individual pupils.

In sum, it does not seem that Kuhn's insights could straightforwardly be applied to science education. That said, perhaps his ideas could be merged with another influential thinker, Jean Piaget, who *is* concerned with individual conceptual change. Furthermore, Kuhn himself made occasional references to Piaget. It is thus this possible influence of Piaget on Kuhn that we review next.

4 What did Kuhn learn from Piaget?

Although Piaget's 'genetic epistemology' is as much an engagement with the philosophy and history of science as it is with the psychological development of children, it is certainly the case that Piaget's impact was greatest in psychology and education (cf., Boden 1979, p.11). Here we do not wish to enter into debates about Piaget's own theory of conceptual change. Instead, we only want to discuss the similarities and differences between the projects of Kuhn and Piaget—and the possible impact that Piaget's writings had on Kuhn.

As already pointed out, Kuhn's focus is the history of science, while Piaget is famous for his psychological experiments with children. So it is perhaps surprising that Kuhn writes in the preface to *Structure*:

> A footnote encountered by chance led me to the experiments by which Jean Piaget has illuminated both the various worlds of the growing child and the process of transition from one to the next. (1996, p. viii)

So what was the impact of Piaget on Kuhn? In our view, Kuhn is likely to have been impressed by Piaget's attempt to understand people from an 'alien tribe' from their point of view. In other words, Kuhn may well have admired Piaget's patient exploration of somebody else's perspective. Such an 'anthropological' approach was absent from child psychology before Piaget—and from the history of science before Kuhn. The ingenuity of both Piaget and Kuhn lies in abandoning the *a priori* starting point of looking down at young children or earlier scientists and dismissing their views as 'faulty', 'underdeveloped', or 'primitive', and instead to ask: 'From *their* perspective, does what they do/believe make sense?'. As Boden (1979, p. 30) writes of Piaget:

> For many people, babies are boring. And the more such people are interested in abstract intelligence or scientific knowledge, the more boring babies may appear to be. They cannot do much, it seems, and what they can do apparently bears little relation to the real stuff of human knowledge. Like kittens, they may be amusing; but they have little psychological and less epistemological significance. Part of Piaget's achievement is to have shown how fundamentally mistaken this attitude is.

Just as Piaget took babies and young children seriously, so Kuhn took earlier scientific theories seriously. In that sense, Piaget could be seen as giving Kuhn a vocabulary to describe methodological mistakes in the history and philosophy of science.

There is an interesting parallel with cultural anthropology here. While early anthropologists treated non-Western cultures as 'primitive', proposing that the minds (or even brains) of the people of those societies were in some sense less developed than their Western counterparts, Evans-Pritchard (1937) set out to reject that assumption. For Evans-Pritchard the Azande were not less intelligent than people in the West. Like Piaget and Kuhn, Evans-Pritchard tried to look at the Azande in their own terms. However, Evans-Pritchard was not able to take this step to its conclusion, since for him (Western) science had a special (objective) status. For Evans-Pritchard it was obvious that science produces true and objective knowledge, which then created the following problem: Why do the Azande, who are as intelligent as Westerners, not 'see' that Western science is true while their own beliefs (e.g., concerning witchcraft and oracles) are obviously false?

This question was based in Evans-Pritchard's picture of science. Science, for Evans-Pritchard, was not one practice among many but *the* practice underlying all others. Put differently, for Evans-Pritchard the aim of every practice is to produce knowledge—knowledge of the kind that Western science produces that is. However, as Winch (1964) points out, that is to seriously mischaracterize Azande practices. Consulting an oracle is not akin to making a scientific prediction—neither is rain dancing. So if our goal is to understand what 'they' are doing, then there simply may be no conflict between 'their' and 'our' practices. Looking at religious practices from the perspective of science is a bit like scoring a play of soccer according to the rules of tennis (cf., Wittgenstein 1979).

Evans-Pritchard's 'problem' is a result of his attempt to compare the practices of science and witchcraft. When examining his work, we should ask whether these two are enough alike to be compared, and what standard we could invoke to make the comparison? In effect, Evans-Pritchard stipulates the standard 'truth' (or 'corresponding with reality') as the means for comparing both sets of practices. However, it seems less than adequate to characterize the practice of witchcraft as being concerned with producing truth. As Winch (1964, p. 315) put it:

> Zande notions of witchcraft do not constitute a theoretical system in terms of which Azande try to gain a quasi-scientific understanding of the world. This in its turn suggests that it is the European, obsessed with pressing Zande thought where it would not naturally go – to a contradiction – who is guilty of misunderstanding, not the Zande. The European is in fact committing a category-mistake.

We are trying to emphasize that although we admire Evans-Pritchard's starting point of not treating the Azande as primitive as compared to Western people, we object to his scientism, i.e., the view that all practices are attempts to gain (objective) knowledge of the world. In our view, Evans-Pritchard not only mischaracterizes what the Azande are doing, but also what we are doing, since many of our practices do not aim to produce or propose (scientific) theories of the world: playing games (e.g., football or hopscotch), cooking, reading and writing novels, and making friends are not in the same 'business' as science.

Returning now to our discussion about the impact of Piaget on Kuhn, let us repeat that Kuhn seems to have been inspired by Piaget's attempt to understand children's views of the natural world from their perspective—rather than looking at children from the perspective of adults or science. Furthermore, Kuhn by and large avoided Evans-Pritchard's mistake of mischaracterising other practices as 'versions' of science. This was partly a result of Kuhn's focus, which was explicitly with the history of science itself. It is only very occasionally that Kuhn made remarks about issues outside the history of science (e.g., 1957, end of Chapter 3; 1977 [1964]; 1977 [1971]). It seems to us that it is these occasional remarks that have been (unfortunately) picked up by the proponents of CC.

5 Differences between Kuhn and Piaget

Despite our characterization of Piaget's possible influence on Kuhn, purported similarities in methodological approach do not seem to be the reason that CC links the two. Instead, for many researchers within CC, the link is appropriate because there is a striking similarity between Kuhn's and Piaget's notions of conceptual change. For example:

> In their search for a theoretical framework to conceptualize the learning of science some science educators turned to the philosophy and history of science as a major source of hypotheses concerning how concepts change (see Posner et al. 1982). They drew an analogy between Piaget's concepts of assimilation and accommodation and the concepts of 'normal science' and 'scientific revolution' offered by philosophers' [sic] of science such as Kuhn [1999] to explain theory change in the history of science. They derived from this analogy an instructional theory to promote 'accommodation' in students' learning of science. (Vosniadou and Ioannides 1998, pp. 1213–1214)

This analogy seems to have been licensed by Kuhn himself, who writes:

> Why should a historian of science be invited to address an audience of child psychologists on the development of causal notions in physics? A first answer is

> well known to all who are acquainted with the researches of Jean Piaget. His perceptive investigations of such subjects as the child's conception of space, of time, of motion, or of the world itself have repeatedly disclosed striking parallels to the conceptions held by adult scientists of an earlier age. (Kuhn 1977 [1971], p. 21)

Obviously, we cannot deny that Kuhn himself seems to make an analogy between conceptual changes in children and conceptual changes in science. However, we wish to argue that although Kuhn may have been inspired by this analogy, his subsequent history of science does not hinge on his views about the nature of children's conceptions of the world. Thus we agree with Cawthron and Rowell (1978, p. 46) who argue that 'Kuhn appear[s] to ignore Piaget altogether, at least in [his] main discussion'.

As already pointed out, Kuhn makes only very occasional reference to children's conceptions of space, time, or motion and his philosophical-historical project can be seen as entirely independent from such remarks. Kuhn's aim was to investigate the scientific theories of Aristotle, Copernicus, or Planck—not the views of an African tribe or of young children. In our view, CC is therefore mistaken to assimilate Kuhn with Piaget, since there are fundamental differences between them. We will concentrate on two: first, the nature of their projects, and, secondly, their picture of science.

First, it was certainly Piaget's goal to use a psychological study of the development of children's thought as a substitute or way into an understanding of the genesis of Western science. For example, he writes:

> [...] it may very well be that the psychological laws arrived at by means of our restricted method can be extended into epistemological laws arrived at by the analysis of the history of the sciences: the elimination of realism, of substantialism, of dynamism, the growth of relativism, etc., all these are evolutionary laws which appear to be common both to the development of the child and to that of scientific thought. (Piaget 1930, p. 240)

According to the proponents of CC, Kuhn seems to accept Piaget's parallel. As Matthews (2000, p. 7) writes in his editorial to the special issue on Kuhn:

> Kuhn popularized Piaget's 'cognitive ontogeny recapitulates scientific phylogeny' thesis among historians and philosophers of science, saying: 'Part of what I know about how to ask questions of dead scientists has been learned by examining Piaget's interrogations of living children' (Kuhn 1977 [1971], p. 21). This recapitulation thesis underlies Piaget's Genetic Epistemology programme [...], as Piaget says: 'The fundamental hypothesis of genetic epistemology is that there is a parallelism between the progress made in the local and rational organization of knowledge and the corresponding formative psychological processes' (Piaget 1970, p. 13).

However, as we have pointed out in our exposition of Kuhn, Kuhn makes it very clear that his remarks in *Structure* about conceptual change are made at an abstract level and pertain to the community of scientists (or the institution of science) rather than individual scientists. When Kuhn speaks of 'psychology', he refers to *social* psychology:

> My recourse has been exclusively to social psychology (I prefer 'sociology'), a field quite different from individual psychology reiterated *n* times. Correspondingly, my unit for purposes of explanation is the normal (i.e., non-pathological) scientific group, account being taken of the fact that its members differ but not of what makes any given individual unique. (Kuhn 1970b, p. 240)

That is to say, we think that Kuhn would object to CC's 'psychologising' or 'individualising' of his history of science. In *Structure*, Kuhn is not so much concerned with the psychology of individuals, but with the views and activities of groups. Kuhn is comparing different historical 'cultures' (and thus would seem closer to cultural anthropology than to child development). As Cawthron and Rowell (1978, p. 47) put it:

> While Piaget looks at the cognitive development of the human child and defines various developmental levels or stages, Kuhn looks at the cognitive development of a particular social group or epiorganism, the scientific community, and defines various paradigms or disciplinary matrices as he later calls them.

Kuhn's notion of conceptual change is a picture of changes in the culture of science rather than changes in individual scientists. This is often overlooked in the CC literature. For example, Posner et al. (1982), in the quotation given at the beginning of our paper, portray Kuhn's picture of science in terms of 'the scientist', thereby applying conceptual change to the individual rather than the group. However, it is not so much that 'the scientist must acquire new concepts' (Posner et al. 1982, p. 211), but more that science as a whole that has to do so. It is this mischaracterization of Kuhn that allows the analogy between Kuhn and Piaget, i.e., between 'earlier scientists' and 'young children'. For example, when Posner et al. (1982, p. 224) write:

> If taken seriously by students, anomalies provide the sort of cognitive conflict (like a Kuhnian state of 'crisis') that prepares the student's conceptual ecology for an accommodation.

They overlook that Kuhn's 'crises' are at the level of science (as an institution), while they talk at the level of the pupil (as an individual). A similar conflation is committed by Clement (1982, p. 70), who argues that

> it should be remembered that historically, pre-Newtonian concepts of mechanics had a strong appeal, and scientists were at least as resistant to change as students are.

In these discussions it is important to bear in mind the difference between the incommensurability between different conceptual schemes (e.g., Ptolemaic and Copernican astronomy or Newtonian and Einsteinian mechanics), and the psychological difficulty of changing to a new conceptual scheme.

CC's psychologistic or individualistic reading of Kuhn may have its origin in the Popperian critique of Kuhn (cf., Lakatos and Musgrave 1970). Popper's critique of Kuhn amounts to reading Kuhn's notion of 'normal science' as a prescription for the individual scientist.

> In my view the 'normal' scientist, as Kuhn describes him, is a person one ought to be sorry for. (Popper 1970, p. 52)

However, Kuhn emphasises that he was not talking about normal *scientists*, but about normal *science*:

> When he rejects 'the psychology of knowledge', Sir Karl's explicit concern is only to deny the methodological relevance to the *individual's* source of inspiration or of an individual's sense of certainty. With that much I cannot disagree. It is, however, a long step from the rejection of the psychological idiosyncrasies of an individual to the rejection of the common elements induced by nurture and training in the psychological make-up of the licensed membership of a *scientific group*. (Kuhn 1970a, p. 22; emphasis in original)

In sum, the famous notions of 'normal science', 'paradigm', or 'changes in world view' are used by Kuhn in *Structure* to talk about historical developments in the community of scientists.

Later Kuhn did make occasional reference to changes in individual scientists, but most typically to contrast the perspective of the scientist with that of the historian. For example:

> In recent years I have increasingly recognized that my conception of the process by which scientists move forward has been too closely modelled on my experience with the process by which historians move into the past. For the historian, the period of wrestling with nonsense passages in out-of-date texts is ordinarily marked by episodes in which the sudden recovery of a long-forgotten way to use some still-familiar terms brings new understandings and coherence. In the sciences, similar 'aha-experiences' mark the periods of frustration and puzzlement that ordinary precede fundamental innovation and that often precede the understanding of innovation as well. [...]
> The transfer of terms like 'gestalt switch' from individuals to groups is, however, clearly metaphorical, and in this case the metaphor proves damaging. [...]
> As the conceptual vocabulary of a community changes, its members may undergo gestalt switches, but only some of them do and not all at the same time. Of those who do not, some cease to be members of the community; others acquire the new vocabulary in less dramatic ways. (Kuhn 1989b, pp. 49–50)

Kuhn here draws attention to the difference of perspective of the historian and the scientist. He argues that changes in paradigm occur over time, and not all scientists change at the same time. Note also that Kuhn's remark about changes in individuals apply to changes from one paradigm to another paradigm (e.g., Ptolemaic to Copernican), not to the socialization of novice scientists into their 'first' paradigm (cf., our remarks above).

The second major difference between Kuhn and Piaget concerns their respective picture of science. On closer inspection, Piaget's picture of science seems to be the opposite of that of Kuhn (and this tension is carried into CC). In other words, Piaget subscribes to a picture of science as progress towards objective truth (cf., Rotman 1977, p. 23) that Kuhn explicitly objected to (cf., Kuhn 1996, pp. 170–171). Piaget also has an ahistorical view of science, since current scientific theories can provide a united description of the accumulated theories of science. For example, Piaget's account of mathematics relies heavily on the foundational-philosophical writings of the Bourbaki School, which has a 'Whig history' view of mathematics in that it treats earlier development (e.g., among Greek mathematicians) in terms of current mathematics, in particular set theory (cf., Rotman 1977, pp. 133–134). Thus while Kuhn argued against Whig history, Piaget seems to have been a proponent of it:

> If we step back from Piaget's treatment of Greek mathematics and look again at his general approach to history we can discern an overall pattern. Behind all theories of history like Piaget's which claim to reveal underlying laws there is a large and dubious assumption. This is that the past is interesting only to the extent that it can be seen to contribute to the present; that historical significance is confined to those events which were in some sense successful and led to the future. From this it is a small step to believing that the past exhibits a progressive movement towards the present, and that what did in fact happen had, in principle, to happen. Under the influence of Hegel's idea of dialectical growth and over-rigid nineteenth-century conceptions of scientific law, this view (in Anglo-Saxon terms Whig history) emerges in the conception of history as the study of necessary and inevitable progress. (Rotman 1977, pp. 139–140)

In short: Piaget's view of the history of science is the opposite of that of Kuhn.

This difference is also reflected in Piaget's and Kuhn's notion of 'conceptual schemes'. For Piaget, subsequent conceptual schemes in children form a hierarchy. However, although this may be the case for children, it is certainly not how Kuhn saw the succession of paradigms in the history of science. While for Piaget conceptual schemes are hierarchical, for Kuhn they are more in parallel. For Piaget a subsequent conceptual scheme incorporates the earlier one completely: 'according to Piaget's theory, a higher stage can do everything a lower stage can do, and more' (Siegel 1982, p. 380). In contrast, Kuhn emphasizes that earlier scientific theories can explain certain phenomena that a later one can not. That is to say, conceptual change in science does imply loss in explanatory power (for certain phenomena). For example, Siegel (1982, p. 382) argues that the Daltonian revolution resulted in a new chemistry that lost the ability to account for certain phenomena for which the old chemistry could account. For Piaget, subsequent conceptual stages are superior to previous ones across the board, while for Kuhn they are only superior in certain respects. We thus agree with Siegel's (1982) point about a fundamental difference between Piaget and Kuhn:

> While there are important and profound differences in the explanatory power of Einsteinian and Newtonian physics, those differences are not formal, or logical, in character. [...] The logic of Einstein is in no way superior to the logic of Newton; but the logic of formal operational thought is indeed superior, according to Piaget, to the logic of concrete operational thought. Here then is another case of disanalogy between cognitive development and the development of science. (p. 384)

6 Conceptual change in school science?

We have argued that an analogy between Kuhn's and Piaget's notion of conceptual change seems to be misplaced. However, what about the more general analogy between Kuhn's notion of conceptual change and the kind of changes that may occur in school science (where the latter may be seen as a conceptual change from everyday to scientific conceptual schemes)?

In order to be able to apply Kuhn's notions of 'conceptual change' and 'scientific revolution' to what happens in schools, CC needs to treat what children already know about the natural world as something akin to a scientific theory, since for Kuhn the change is between two *comparable* things. That is to say, in Kuhn's version of paradigm shifts within science, both the new and the old paradigm serve a similar purpose or aim (although the new paradigm may be more successful at certain aspects). Kuhn (1996, p. 92) explicitly makes an analogy between political and scientific revolutions. In both the political and the scientific case, the change is between things that are comparable, i.e., that are the same kind of thing: on the one hand a form of government (e.g., a change from Monarchy to Democracy) or on the other a type of scientific theory (e.g., a change from Newtonian to Einsteinian physics).

However, are pupils' 'naïve beliefs' sufficiently close to a scientific theory? In this section we will argue that they are not. Everyday understandings are not a 'conceptual scheme' in Kuhn's sense, since the latter refers to a systematic theory with interdefined terms (which is what his notion of incommensurability requires). In Kuhn's case, both the old and the new scientific theory serve the same kind of purpose (namely explaining aspects of the natural world in scientific terms) and can therefore be in competition:

> Like the choice between competing political institutions, that between competing paradigms proves to be a choice between incompatible modes of community life. (Kuhn 1996, p. 94)

In our view, common sense and science do not stand in this relation. In our society one can be, e.g., an accountant, a football player, a movie star, or an artist with or without knowledge of the latest scientific developments. People are not faced with a choice between science or common sense as two 'incompatible modes of community life'. It is not as if we meet people on the street and ask them: 'Which one are you—science or common sense?'. Rather, some people choose to pursue a career in science—just as others become football players or accountants.

6.1 How are we to think about our knowledge of the natural world?

Part of the reason why researchers may be compelled to draw a parallel between everyday knowledge and scientific knowledge has to do with a conceptual confusion about the former. In our view, CC has a strange picture of people's common-sense knowledge of the natural world (a picture that to a large extent is carried over from cognitive science). Within CC, children (and adults) are often portrayed as miniature scientists. Such a view has its origin in Piaget (cf., Woodfield 1987) and is frequently adopted by CC researchers, who speak of the 'pupil as scientist' or 'children's science'. According to this picture, children are seen as a disengaged observing 'brain' that is looking at the world, interpreting what it sees, storing the observations as a series of factual statements, and creating causal theories to explain the observed events.

Portraying common sense in the image of science leads to a falsification of children's (and adults') engagement with the world and their knowledge of it. This move has frequently been criticised (e.g., Cook 1969; Coulter 1989; Shanker 2004) and we will restrict our remarks to two mistakes in CC's characterization of common sense; first, a neglect of practices (of what people do), second, an underlying theoreticism (turning common-sense knowledge into versions of scientific theories).

First, proponents of CC are almost exclusively concerned with how children 'think' about the world and completely neglect what children 'do' in the world. For CC, it seems that we first need a theory of the world before we can act in it.

However, this reverses the way in which we develop knowledge of the world: what we know about the world develops as part of what we do (cf., Wittgenstein 1953; Coulter 1989). That is to say, we learn about the world as part of various activities and practices (eating, playing ...) and it is these practices that define what counts, in specific situations, as a mistake or a fault (we will return to this when discussing 'misconceptions' below).

Second, common-sense knowledge could be characterized as 'socially sanctioned-facts-of-life [...] that-any-bona-fide-member-of-the-society-knows' (Garfinkel 1967, p. 76), i.e., as an assortment of socially shared facts and recipes. It is not therefore (as proponents of CC would have it) anything like a systematic causal theory. To give just two examples: 'Unsupported things fall down' might be common-sense knowledge, but that does not mean that we therefore have a (proto-)theory of gravity. 'The sun rises in the east' is part of common-sense knowledge, but that does not imply that we therefore have an astronomical theory of the universe. Adopting a quote by Sharrock and Anderson (1982, p. 111), we might say that CC researchers:

> are the kind of people who would attribute a geocentric theory of the universe to us on the strength of the remark that we intend to get up tomorrow morning to watch the sun rise.

Education research—CC included—frequently stipulates that all our knowledge about the natural world has the same purpose as an explanatory theory. For example:

> 'Commonsense' ways of explaining phenomena, as pictured here, represent knowledge of the world portrayed within everyday culture. (Driver et al. 1994, p. 8)

> Another well-supported finding is that all students, the weak as well as the strong learners, come to their first science classes with surprisingly extensive theories about how the natural world works. They use these 'naïve' theories to explain real world events before they have had any science instruction. (Resnick 1983, p. 477)

> The misconception appears to be grounded in a *systematic intuitive theory* of motion [...]. (McCloskey 1983, p. 123; our emphasis)

Again, the question is not whether pupils have knowledge of the natural world (of course they do), but whether this knowledge has the form of a systematic, causal (proto-scientific) theory. In other words: How much of our 'naïve beliefs' are attempts at explaining, in a systematic and causal manner, what we have observed during the course of our lives? In our view, common-sense knowledge (both of children and adults) does not constitute a systematic theory, but rather a heterogeneous assortment of things (a 'motley' as Wittgenstein might put it). Although these things sometimes work together, very often they do not. In particular, they do not express an underlying overall systematic conception of the world. For example, it is common-sense knowledge that the earth moves around the sun; but it is *also* common-sense knowledge that the sun rises in the East and sets in the West. These two statements do not form a contradiction, as they are not expressions of two competing theories. Similarly, it is common-sense knowledge that humans are mammals and that humans are therefore just another animal; but it is also true that in many contexts we make a distinction between humans and animals (e.g., on signs such as 'No animals allowed'). Again, there is no conflict here, as the second statement does not express an underlying classificatory theory (and we might note how some of common sense is also 'science', e.g., expresses biological classificatory schemes).

In particular, common sense is not a 'disciplinary matrix' in Kuhn's sense. It is neither 'composed of ordered elements of various sorts' (Kuhn 1977 [1974], p. 297)—but a loosely connected heterogonous set of facts; nor is it part of a 'discipline'—what general problem is common sense supposed to 'solve'? This highlights the connection to our first point (the neglect of practices): CC overlooks that theories are tied to practices (and only science systematically tries to explain natural phenomena), but common sense does not constitute a practice, since the purpose of our life could hardly be characterized as 'building explanations of the natural world'. As mentioned in our discussion of Evans-Pritchard above, it is a form of scientism to stipulate that people are always in the business of creating theoretical explanations of their world. We wonder whether CC researchers, by characterising what children say about natural phenomena in the image of science, are not committing a similar category-mistake to Evans-Pritchard.

These points hopefully demonstrate that common-sense knowledge is not anything like a scientific theory—and that therefore Kuhn's picture of conceptual change cannot easily be applied to what happens in school science. Before moving on, let us quickly comment on the frequent claim that children's common-sense knowledge is akin to Aristotle's theory of physics. For example:

> In fact, one might characterize early stages of students' work as the confrontation of an essentially Aristotelian theory of physics, with a Newtonian reality. (di Sessa 1982, p. 41)

> The point is that the real world, that is to say, the practical world of everyday experience, is, to a large extent, an Aristotelian world. (Garrison and Bentley 1990, p. 20)

Even Kuhn, in a side-remark, was tempted to say something very similar:

> Today the view of nature held by most sophisticated adults shows few important parallels to Aristotle's, but the opinions of children, of the members of primitive tribes, and of many non-Western people do parallel his with surprising frequency. (Kuhn 1957, p. 96)

As mentioned in our discussion of Kuhn and Piaget, we agree with Kuhn's history of science, but we are sometimes uncomfortable with side-remarks such as these (which are, however, largely independent from his general position). As should be clear by now, we think that it is misleading to characterize common-sense knowledge as Aristotelian. We understand the temptation of saying this, since it may serve as a reminder that how we experience the material world is very different from the picture suggested by Newtonian physics and therefore 'closer' to Aristotelian physics. For example, heavier objects typically do fall faster than lighter ones. Of course, Newtonian physics does not deny this (in an environment with friction, weight makes a difference), but Newton's claim is about a special case, a vacuum, and it is using this special case as the underlying general principle that is Newton's ingenuity—and that 'removes' Newtonian physics from direct experience.

In our view the analogy is misleading, since it turns common-sense knowledge into a systematic (proto-scientific) explanatory theory, which clearly it is not. For example, it is sometimes claimed that 'children usually think of the earth as a very big, extended, flat physical object' (Vosniadou and Ioannidis 1998, p. 1218)—but even for Aristotle the earth was a sphere (Vosniadou and Brewer 1992, p. 539). Furthermore, the culture that children grow up in (e.g., as portrayed in TV programs) includes things like spaceships or superman flying through the air, which clearly are not part of an Aristotelian universe. Children are saturated in modern culture, but that does not mean that they therefore have a systematic theory of the natural world. Common sense is not a closed system (neither is language).

6.2 The world of science and the everyday world

We have so far argued that common-sense knowledge of the natural world does not constitute anything akin to a systematic, causal, proto-scientific theory. What about the relationship between scientific knowledge and how people think and talk about the natural world in their everyday lives? In this context it is often argued that scientific knowledge and common-sense knowledge stand in competition, i.e., that the former is more 'precise' or 'correct' than the latter, or that scientific language should replace ordinary language.

The first thing we would like to ask is this: What might be 'wrong' with common sense so that it would stand in need of improving or replacing? In our view, to characterize common-sense knowledge as 'faulty' only makes sense if we could identify things that go awry *in the conduct of our everyday lives*, e.g., when playing football, cooking a meal, or talking to a friend on the phone. Errors are tied to what people are trying to accomplish.

Second, we want to note that children's and adults' observations of the natural world (e.g., that things fall down or that it is often difficult to move big objects) are largely independent from developments in science:

> [...] the whole of science is totally irrelevant to most people's day-to-day lives. One can live very well without knowledge of Newtonian mechanics, cell theory and DNA, and other sciences. (Wolpert 1992, p. 16)

We might characterize science and common sense as different 'frames of reference' that can co-exist without being in conflict. This is partly a result of the differences between what are sometimes called 'phenomenal' and 'objective' descriptions (the former would be the aim of phenomenology, the latter of science). For example, for Newtonian physics time is an objective measure, whereas our phenomenal experience of time is such that it sometimes passes slowly and sometimes goes by very quickly. These scientific and phenomenal descriptions of time do not, however, stand in competition, but constitute different 'frames of reference' (and the problem for school science might be to teach how to recognize and coordinate them).

For proponents of CC, however, different descriptions such as these *are* in competition. Even Kuhn sometimes falls into this trap. For example, at the end the end of a chapter in *The Copernican Revolution*, Kuhn speculates about the psychological base of Aristotle's explanation of motion. Kuhn argues that it

> derives from the Aristotelian transmutation of a primitive perception of space. To the members of prehistoric civilizations and primitive tribes, space seems very different from the Newtonian space in which we were all brought up, usually without knowing it. The latter is physically neutral. A body must be

> located *in* space and move *through* space, but the particular part of space and the particular direction of motion exert no influence on the body. Space is an inert substratum for all bodies. Each position and each direction is like every other. In modern terminology, space is homogenous and isotropic; it has no 'top' or 'bottom', 'east' or 'west'.
> The space of the primitive, in contrast, is often more nearly a life space: the space in a room, or in a house, or in a community. It has a 'top' and 'bottom', and 'east' and 'west' (or 'front' and 'back' – in many primitive societies words for direction derive from words for parts of the body and reflect the intrinsic differences of these parts). Each position is a position 'for' some object or 'where' some characteristic activity occurs. Each region and direction of space is characteristically different from every other, and the differences partially determine the behavior of bodies in each region. Usually the primitive's space is the active dynamic space of everyday life; distinct regions have distinct characteristics. (Kuhn 1957, pp. 97–98)

We would argue that Kuhn is here talking outside his area of expertise and exhibits some of the misconceptions of common sense that we have tried to dispel. It is hard to know why he singles out primitives, since we 'sophisticates' operate pretty much the same way. Notice that Kuhn writes of 'Newtonian space in which we were brought up, *usually without knowing it*' (our emphasis). In our view, this demonstrates that Kuhn here conflates phenomenal and scientific descriptions of 'space'. Presumably we could also say that people from primitive societies grew up in Newtonian space (without knowing it)—so there is not necessarily a difference between 'their' and 'our' ordinary concepts of space. The only difference lies in the fact that 'we' have a developed physics, and 'they' have not. A central feature of operating within the system of Newtonian space is that space is the same everywhere (whether or not the people of that part of the world have ever heard of physics) and anytime (even if the people have not yet developed physics, i.e., prior to Newton). Kuhn here conflates a phenomenal description of space (akin to Merleau-Ponty) with a scientific one.

We think that Kuhn's reflections about the historical developments within science could usefully be complemented by the philosophical reflections about the relationship between common sense and science by Alfred Schütz and Gilbert Ryle.

Schütz (1962a, b) wants to emphasize the importance of the natural attitude of everyday life as the paramount reality from which other 'realities' (e.g., that of science, drama, or dreams) derive from. In that sense, everyday life and science are not on the same level, but rather science is a special project (just as theatre or soccer). Consequently, science and common sense are not in competition, and neither are theatre and common sense. For example, we can teach children that people on stage do not 'really' die, or that it is possible for people to fly in a movie, but that children should not try to jump from a roof. Proponents of CC occasionally realize that there is

> the possibility of individuals having plural conceptual schemes, each appropriate to specific social settings. (Scientists, after all, understand perfectly well what is meant when they are told 'Shut the door and keep the cold out' or 'Please feed the plants'). (Driver et al. 1994, p. 7)

However, it seems to us that proponents of CC subsequently place these different 'conceptual schemes' on the same level—rather than treating everyday conceptual schemes as the bedrock, the paramount reality, from which other frames of reference develop.

Ryle (1954a, b), from whom the title for this section is borrowed, also wants us to notice that technical-scientific descriptions are not in competition with ordinary ones. He writes:

> When we are in a certain intellectual mood, we seem to find clashes between the things that scientists tell us about our furniture, clothes and limbs and the things that we tell about them. We are apt to express these felt rivalries by saying that the world whose parts and members are described by scientists is different from the world whose parts and members we describe ourselves, and yet, since there can be only one world, one of these seeming worlds must be a dummy-world. Moreover, [...] it must be the world that we ourselves describe which is the dummy-world. (Ryle 1954a, p. 68)

Ryle uses an analogy with accountancy to exhibit the category mistake inherent in this picture. Ryle says that the descriptions about books in the library produced by accountants and by pupils are not 'in conflict' as they are not interested in the same kind of thing. For the accountant what matters is the price of the book—for the pupil it may be important whether the book is easy or difficult, borrowed or in the library, etc. Similarly, our ordinary ways of talking about tables and chairs are not in conflict with scientific ones (according to the former a table is solid without holes, according to particle physics it is largely composed of holes):

> A bit of the theory of ultimate particles has no place in it for a description or misdescription of chairs and tables, and a description of chairs and tables has no place in it for a description or misdescription of ultimate particles. A statement that is true *or* false of the one is *neither* true *nor* false of the other. It cannot therefore be a rival of the other. (Ryle 1954a, p. 79)

6.3 Misconceptions?

Given that there is no general conflict between common-sense and scientific knowledge, we think it is problematic to speak of 'misconceptions' (or of 'naive knowledge') without specifying the context or standard according to which children's knowledge is labelled as 'misconceived' or 'naive'. Speaking of 'misconceptions' per se, i.e., without specifying a standard according to which there is a fault or error, suggests that there is only one correct conception—the one of science. This is wrong on two grounds:

First, if proponents of CC took Kuhn seriously, they would have to realize that there is no such thing as 'the' scientific explanation of a phenomenon—but different, incommensurable, explanations that are employed according to the prevalent scientific culture or the problem under consideration. For example, Kuhn (1957, p. 38) emphasizes that the Ptolemaic two-sphere universe is still used by many people today, e.g., in navigation and surveying. In other words, Kuhn argues that it is not possible to speak about 'truth' or 'error' per se, stressing that these have always to be considered within a system. Nor, according to Kuhn, are earlier scientific theories (e.g., the two-sphere model) wrong per se, since they may even better at calculating certain phenomena than subsequent theories. Rather, earlier scientific theories will fail to explain some phenomena that later theories do cover, and it will be those failures which, in turn, give rise to eventual scientific revolution. As a consequence, there can be no conflict between common sense and science—since there is no such thing as 'the' scientific explanation of a phenomenon.

Second, as we have argued in the previous section, science and common sense are not in competition. Proponents of CC argue that there is a conflict between what children already know and what they are supposed to learn in school science. However, what children already know has developed as part of their out-of-school lives. Thus to label this knowledge as simply 'faulty', 'naïve', or 'erroneous' is to imply that CC researchers have identified mistakes or errors of judgement *within children's everyday lives.* A boy throwing a stone in the air and expecting it to fly into the sky is clearly making an error of judgement (which he will realize as soon as the stone falls on his head). However, CC researchers never identify errors or misconceptions of this kind. They do not look at what pupils do outside school, i.e., identify misconceptions as part of the various activities that children engage in. Instead, CC researchers only look at 'misconceptions' within the context of school science (or experimental situations). The 'misconceptions' identified by CC are *only misconceptions within school science*.

The irony of the programme of Conceptual Change in science education is that it argues against a 'blank slate' conception of children's prior knowledge, i.e., aims to take into account children's knowledge of the world. However, CC subsequently only investigates children's knowledge in the context of school science rather than in those occasions in which the knowledge was acquired. This restriction of context and applicability sneaks in (usually in brackets) when CC researchers talk about 'misconceptions', e.g.:

> Naïve knowledge [...] is often incorrect (*when compared to formal knowledge*) (Chi and Roscoe 2002, p. 3; our emphasis)

> The paradox of science education is that its goal is to impart new schemata to replace the student's extent ideas, *which differ from the scientific theories being taught*. (Carey 1986, p. 1123; our emphasis)

> These theories are often labelled 'misconceptions' [...], *because they do not agree with the current scientific view*. (Hewson and Hewson 1984, p. 4; our emphasis)

All these quotations exhibit that what is 'wrong' with children's knowledge is only so from the point of view of the scientific theory that children should learn in school science. In other words: these pupils simply have not yet mastered the specific kind of science in question. CC overlooks how words and concepts are used as part of pupils' everyday life and the practices that they engage in. However, it is these practices that set the standards of adequacy needed to label them as 'misconceptions' (or not).

In CC research, pupils may seem to be using their everyday knowledge to explain the world because the researcher's purpose is to uncover 'children's explanations of the world'. So for example, a question like 'Can you see in a completely dark room?' asked of a student, is used by a researcher to make a claim about a pupil's 'private theory' about how the world works. But of course, from the pupil's perspective, this question can be interpreted (perfectly reasonably) to call for a report of personal experience (Macbeth 2000). Thus, researchers are led to see 'gaps' and 'misconceptions' where none in fact exist.

7 Conclusion: what happens in school science?

In this paper we have argued that CC is mistaken to make what pupils learn in school science analogous to Kuhn's conceptual changes in the history of science. The main reason for the inappropriateness of this analogy lies in the simple fact that school pupils are not scientists. Kuhn's conceptual schemes are tied to what scientists do (e.g., performing new experiments and calculations). However, school pupils are not engaged in work in that sense, since they do not produce new explanations of the natural world. Pupils are in school to learn what others have discovered. They might learn something new for themselves ('personal discovery'), but what they learn has been known for a long time (i.e., is not a 'scientific discovery'). That schools pupils are not engaged in 'real' scientific work is recognized both by teachers and pupils:

> [...] while pupils may well acquire a taste of what it is to be a professional scientist, there is, in these data at least, no pretence that they are 'real scientists' engaged in real discovery. Indeed the distinction between themselves and 'real scientists' is quite explicitly stated by the teacher. (French 1989, pp. 34–35)

In school science pupils witness and perform demonstrations 'in' or 'about' science, but they don't 'do' science (cf., Macbeth 2000, p. 237).

We want to point out that we are making a logical argument here, not a moral one. Understandably, educators exhibit a strong preference for pupils to be engaged in 'meaningful' activities, and by extension, 'meaningful' learning. To these educators, our comments above sound as if we are 'against' progressive school curricula, or if we are against pedagogies meant to 'empower' pupils. Rather, what we are saying is that accepting the thesis that human social life is contextually bound—as Kuhn argued for—requires one to recognize that pupils and scientists are just not doing the same thing. No curriculum or pedagogy can change this (logical) observation.

Conceptual change for Kuhn occurs from a growing sense within the scientific community that the existing tools are not adequate for dealing with new questions and problems:

> One aspect of the parallelism must already be apparent. Political revolutions are inaugurated by a growing sense, often restricted to a segment of the political community, that existing institutions *have ceased adequately to meet the problems* posed by an environment that they have in part created. (Kuhn 1996, p. 92; our emphasis)

However, school science does not address problems of this kind in the lives of pupils outside of, and prior to, school. What pupils learn in school science does not address 'inadequacies' in common sense. Although what pupils learn in school may change how they look at the world and may enable them to do new things, it does seem misleading to say that school education solves systemic 'problems' that pupils had prior to going to school. That is to say, there are no 'anomalies' for pupils (as suggested by Posner et al. 1982, p. 223) that science education will 'resolve'. Pupils do not participate in school science lessons with the anticipation that what they will learn might help them to solve problems that they have been struggling with for some time. Their life outside school is 'in order' before and after instruction in school science—although they may as a result be able to do new things. School science does not have new answers to old problems, but poses new questions (just as school grammar does not teach you how to speak, but gives you a new perspective on language). Science is very much an addition to, an expansion of, and in important respects a transfiguration of, everyday understandings, but not an extensive refutation of them. Furthermore, current common-sense knowledge has already an admixture of science 'in it'. That is to say, our common-sense knowledge is different from common-sense knowledge one hundred or two thousand years ago.

So how would we characterize what happens in school science classrooms? In our view what children are supposed to learn in school science is not a replacement, but rather a specialization of what they already know. School science could be said to 'upgrade' common-sense descriptions and explanations (Lynch and Macbeth 1998, pp. 284–286) rather than to 'replace' them. Thus, rather than adopting Kuhn's (1957, pp.43–44) way of speaking of the 're-education of common sense' we would speak of the 'continuing education of common sense' in schools. School science starts with pupils' experience of the world as a 'way in' to school science, i.e., as a way of seeing the new in terms that pupils can recognize. Although at the end of a course in school science, some pupils make be able to talk 'scientifically' and use scientific concepts correctly, this is not how they *start*. This allows us to look at concepts in a new way:

> Concepts lose their familiar 'special' estate, a substantially Platonic estate of types and formal relations. Their first work is found in ordinary, rather than disciplinary worlds. (Macbeth 2005, p. 19)

School science creates a new, specialized 'frame of reference' (that does not stand in conflict with the old one, but constitutes a new one). For example, 'what goes up must come down' holds in most situations. What pupils learn in school science is that in the very special situation of a very high acceleration (as in the case of rockets) it may not hold. Similarly, it is true that our bodies do not have holes (except when we have been shot). When pupils learn about atomic theory, they learn a different (but not contradictory) way of looking at bodies, which now consist mainly of empty space, i.e., are full of holes. School science thus gives certain expressions (e.g., 'hole' or 'earth') a new sense, which allows that new questions can be asked and new statements can be made (using the old concepts). CC, exhibiting a form of scientism, thinks that school science has to replace one frame of reference with another. Instead, in school science pupils learn additional frames of reference and learn how to coordinate between them. As a consequence, understanding the relation between how we ordinarily speak of things and how science speaks of, and conceives of, things is one of the fundamental challenges of science teaching.

In our view, CC mystifies what goes on in school science classrooms to the extent that it treats what pupils know as a set of proto-scientific, ontological beliefs. What pupils have to learn in school science is difficult and it is worthwhile finding ways of helping pupils with acquiring this new and difficult material. However, this is a pedagogical, not an ontological, problem.

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