CHAPTER 16

A History of Conceptual Change Research

Threads and Fault Lines

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Characterizing Conceptual Change

Within the learning sciences, conceptual change is probably best defined by its relevance to instruction. In the broad educational experience, some topics seem systematically to be extremely difficult for students. Learning and teaching in these areas are problematic and present persistent failures of conventional methods of instruction. Many areas in the sciences, from elementary school through university level, have this characteristic, including, in physics, concepts of matter and density, Newtonian mechanics, electricity, and relativity; in biology, evolution and genetics.[1] To learn such topics, students must go through a conceptual change. Conceptual change contrasts with less problematic learning such as skill acquisition and acquisition of facts, where difficulty may be evident, but for more apparent reasons such as sheer mass of learning, or the necessity of practice to produce quick, error free, highly refined performance.

The name "conceptual change" embodies a first approximation of what constitutes the primary difficulty: students must build new ideas in the context of old ones; hence, the emphasis on "change" rather than on simple acquisition. Strong evidence exists that prior ideas constrain learning in many areas. The "conceptual" part of the conceptual change label must be treated less literally. Various theories locate the difficulty in such entities as "beliefs," "theories," or "ontologies," in addition to "concepts."

Conceptual change is among the most central areas in the learning sciences for several reasons. First, many of the most important ideas in science seem to be affected by the challenges of problematic learning. Conceptual change also engages some of the deepest, most persistent theoretical issues concerning learning. What is knowledge in its various forms? When and why is it difficult to acquire? What is deep understanding; how can it be fostered? Conceptual change is important not only to education but also to developmental psychology, epistemology, and the history and philosophy of science. In the history of science, consider: What accounts for the challenges posed by scientific revolutions, such as those engendered by Newton, Copernicus, and Darwin?

Conceptual change research is difficult to review. Problems (what changes; why is change difficult; how does it happen?) have led only slowly to solutions, and solutions have been tentative and partial. In addition, the involvement of multiple disciplines has produced a plethora of orientations and theories. There are, in fact, no widely accepted, well-articulated, and tested theories of conceptual change. Instead, the field consists of multiple perspectives that combine many commonsense and theoretical ideas in kaleidoscopic fashion. My review aims to highlight critical *threads* and *fault lines* – the former through a historical orientation, and the latter by noting a few important changes in perspective and differences of opinion. The review ends with a set of recommendations for future work.

Preview

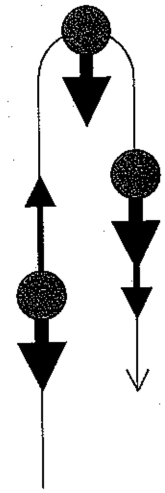
This section uses one example to demonstrate several of the most important issues in conceptual change research. These issues can serve as landmarks with respect to which other important, but more subtle issues, can be located.

The concept of force in physics provides an excellent example of conceptual change. Figure 16.1 shows the correct scientific account of the simple event of tossing a ball into the air. Physicists would say that there is only one force, gravity, on the ball after it has left the hand. Gravity acts on the speed of the ball, diminishing it until the object reaches zero speed at the peak of the toss. Then, gravity continues acting, "pulling" the speed of the ball downward, and so the ball accelerates downward until it is caught.

Before conceptual change research began, instructors who noticed student difficulties with problems like the toss might have attributed their difficulties to the abstractness of physics, or to its complexity. Instructional interventions might include simplifying exposition or repeating basic instruction. These reactions to student difficulties assume a simple acquisition model of learning. In contrast, listening closely to student explanations yielded a stunning discovery. Students do not exhibit lack of descriptive or explanatory capability, but they have radically different things to say than experts. Figure 16.2 illustrates a typical novice explanation: Your hand imparts a force that drives the ball upward against gravity. The upward force gradually dies away until it balances gravity at the peak. Then, gravity takes over and pulls the ball downward. Students seem to have a prior concept of force, but it is different from experts'. Instruction must deal with these ideas and change them: enter the era of conceptual change.

How should one deal with students' "misconceptions"? Early in conceptual change research, most people assumed that student ideas were coherent and integrated. Under such an assumption, one has little choice but to argue students out of their prior ideas, and convince them to accept the ideas of physicists. But a very different view has gradually grown in influence. Rather than a coherent whole, students' ideas may consist of many quasi-independent elements. Instead of rejecting student conceptions, one can pick and choose the most productive student ideas, and refine them to create normative concepts. For example, students see balancing at the peak of the toss. And, balancing is a rough version of an incredibly important principle in physics, conservation (e.g., of energy or momentum). Similarly, students see speed as proportional to net force. A subtle change can turn this into correct physics. Force does not act *directly* to change position; instead, it acts directly on an intermediary, velocity, and velocity does the work of changing position.[2] Finally, the upward "force" in the incorrect explanation is not absent, but it is what physicists call momentum.

The opposing views of students' naïve ideas as either (1) coherent and strongly integrated, or (2) fragmented so as to allow disassembling, refining, and reassembling into correct physics, constitute a watershed



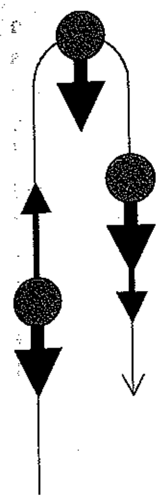


Figure 16.1. Expert explanation of a toss includes only one force (a) Gravity (thick arrow) drives the ball's velocity (thin arrow) downward, (b) bringing it to zero at the peak, (c) then gravity extends velocity downward in the fall.

fault line whose history and current status will occupy a prominent place in this chapter.

I now turn to a chronology of conceptual change research.

Premonitions in the Work of Piaget

Jean Piaget and his colleagues contributed an immense body of work on children's developing understanding (see Gruber & Voneche, 1977, for a compendium), which had a strong influence on conceptual change studies. The philosophical study of knowledge – epistemology – had traditionally concentrated almost exclusively on issues of certainty: "Knowledge is justified, true, belief." Piaget introduced the idea of *genetic epistemology*, that ideas and thinking grow gradually, and that the *evolution* of knowledge and understanding is a more productive study than certainty and timelessness. His empirical work produced an incredibly rich corpus on how children's ideas develop in many domains. His group studied biology (e.g., the notion of being alive), physics (e.g., the concept of force), conceptions of space and time (e.g., perspective and simultaneity), representation (drawing), categorization, logic, and other topics. His work in biology has been particularly influential on conceptual change studies.

Some of Piaget's theoretical apparatus penetrated into conceptual change research. For example, Piaget viewed *equilibration* as a key mechanism: new experiences disequilibrated prior knowledge, and re-equilibration drives learners toward better, more advanced thinking. For the most part, however, Piaget's belief in equilibration is a fault line separating his ideas from modern conceptual change work. It remained too vague to provide satisfying explanations.

A more definitive fault line separating Piaget from most conceptual change work is that Piaget tried to develop an encompassing, domain independent theory of intelligence, where changes in conceptualization in multiple domains all reflected common, core differences in thinking. Conceptual change approaches to learning are domain specific, although the mainstream view is that the mechanisms are similar across domains. Researchers now generally regard Piaget's stage theory of intelligence

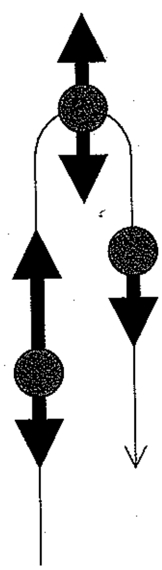


Figure 16.2. Novice explanation of a toss includes two forces (a) An upward force overcomes gravity, (b) but the force gradually dies away and comes into balance with gravity at the peak, (c) then it gives way to gravity on the fall.

as perhaps his most famous, but least interesting, contribution.

Piaget established the continuing, central thread of *constructivism* – the notion that new ideas and ways of thinking emerge from old ones. This foundational dynamic helped seed conceptual change work proper. Piaget also undermined older one-sided views, including both the *empiricist* view that knowledge originates either in purely empirical observations (as claimed by British philosophers such as David Hume), as well as the *rationalist* view that knowledge is inherently the product of rigorous thought, independent of experience (as epitomized by Descartes). Constructivism and the astounding revelation that children systematically think in very different ways than adults constitute the most important threads from Piagetian studies into conceptual change.

The Influence of the Philosophy and History of Science

Akin to Piaget, prior study in the history and philosophy of science exerted great influence, starting from the earliest theorizers of conceptual change in the learning sciences. I review here the ideas of Thomas Kuhn. To many, Kuhn defines the enduring relevance of the history of science to studies of conceptual change broadly. By contrast, Kuhn had strong opposition within the history of science, and there was probably never a time when his work represented the consensus view in that field. To represent Kuhn's opponents, I discuss Stephen Toulmin, who anticipated important threads in current opposition to Kuhn's ideas. The enduring fault line between coherence and fragmentation, which I introduced in the Preview section, can be traced back to Kuhn (coherence) versus Toulmin (fragmentation).

Kuhn's Scientific Revolutions

In his landmark work, *The Structure of Scientific Revolutions* (1970), Kuhn laid out a different view of progress in science, compared to most of his predecessors. Kuhn rejected the idea that science progresses incrementally. Instead, he claimed that ordinary "puzzle-solving" periods of science, called "Normal Science," are punctuated by revolutions, periods of radical change that work completely differently than Normal Science. In particular, the entire *disciplinary matrix* (referred to ambiguously but famously as a "paradigm" in the earliest edition) gets shifted in a revolution. What counts as a sensible problem, how proposed solutions are judged, what methods are reliable, which symbolic generalizations apply, and so on, all change at once in a revolution. Kuhn famously compared scientific revolutions to gestalt switches, where practitioners of the "old paradigm" and those of the "new paradigm" simply do not see the same things in the world. Gestalt switches happen when the coherence of ideas forces many things to change at once.

Kuhn articulated his belief in discontinuity in terms of *incommensurability*. Concepts simply come to refer to different things. Incommensurability means that claims of the new theory cannot be stated in the old terms, and vice versa. Incommensurability constitutes both a definitional property of conceptual revolutions and also a cause for their problematic nature. As such, Kuhn's incommensurability established an enduring thread in conceptual change work. In contrast, the more sociological aspects of Kuhn's views, such as the importance of the disciplinary matrix, were not imported into conceptual change work. This is ironic since, to Kuhn, sociology is absolutely central. Kuhn's own defense and elaboration of *Scientific Revolutions* ended:

> *Scientific knowledge, like language, is intrinsically the common property of a group, or else nothing at all. To understand it we shall need to know the special characteristics of the groups that create and use it.* (p. 208)

Few conceptual change researchers comment on this core precept when adapting Kuhn to individual learning. Those who do (e.g., Karmiloff-Smith, 1988) treat it as something one can take or leave.[3]

Toulmin's Rejection of Strong Coherence

In addition to selectively incorporating Kuhn's ideas, conceptual change research ignored competing perspectives in the history of science. Stephen Toulmin's *Human Understanding* (1972) provides a good example, particularly since it appeared only a few years after *Scientific Revolutions* and since this work perspicuously introduces the other side of the coherence versus fragmentation fault line.

Human Understanding begins with an extensive review and rejection of assumptions about the level and kind of systematicity (a synonym Toulmin used for "coherence") that philosophers assumed in scientific thought. Toulmin traced the "cult of systematicity" back to the model of logicomathematical coherence abstracted from certain mathematical forms, such as Euclidean geometry viewed as an axiomatic system.[4]

Presumptions of systematicity were, for Toulmin, pernicious. Not only is there no global fixed framework for all science (as, for example, the philosopher Kant claimed), but the assumption that local frameworks (particular theories) are strongly systematic fails as well. Toulmin singled out Kuhn for criticism. Presumptions of incommensurability are an artifact of mistaken assumptions. Assuming strong systematicity, in addition to being factually inaccurate, guarantees the appearance of incommensurability and also guarantees that change will always appear mysterious (Kuhn's "gestalt switch"). There can be no account of how a complex, rigid framework "jumps" to a new one.

> *Rather than treating the content of a natural science as a tight and coherent logical system, we shall therefore have to consider it as a conceptual aggregate, or "population", with which there are – at most – localized pockets of logical systematicity. (p. 128)*

In the context of his attack on strong systematicity, Toulmin made two important methodological observations. First, he maintained that the dominant "before and after" view of conceptual change had to be abandoned.

> *This change of approach [away from strong systematicity] obliges us to abandon all those static, "snapshot" analyses.... Instead, we must give a more historical, "moving picture" account... (p. 85)*

Toulmin also complained about the adequacy of treatment of central ideas in conceptual change.

> *The term concept is one that everybody uses and nobody explains – still less defines. (p. 8)*

Misconceptions

> *Even the brightest students in the class [have] false ideas based on enduring misconceptions that traditional instructional methods cannot overcome. (Promo materials for* A Private Universe: Harvard-Smithsonian Center for Astrophysics *(1987), http://www.learner.org/onesheet/series28.html)*

Students Have False, Persistent Beliefs

Starting in the mid to late 1970s, a huge social movement, which we dub "misconceptions," began modern conceptual change studies in educational research and in neighboring disciplines, including experimental psychology and developmental psychology. The movement exploded to prominence in the early 1980s, spawned a huge literature, and tailed off somewhat in the early 1990s, although its presence and influence is still strong. The power of the movement can be gauged by the fact that an early bibliography collected literally hundreds of studies (Pfundt & Duit, 1988). Confrey (1990) provided an excellent review of misconceptions in physics, biology, and mathematics.

Physics was the locus of much early work. Three European scholars were important contributors. R. Driver, L. Viennot, and A. Tiberghien did foundational studies, often involving reformulated instructional interventions, of such topics as elementary school students' conceptions of matter, high school students' conceptions of force and motion, and middle school students' conceptions of heat and temperature (Driver, 1989; Tiberghien, 1980; Viennot, 1979). These and other researchers discovered, documented, and theoretically considered "false beliefs" such as "small bits of matter – say, a speck of dust – don't weigh anything," "forces cause movement at a speed proportional to their strength" (as opposed to the Newtonian: *acceleration* is proportional to force), and "heat and cold are different things."

In the United States, some important early misconceptions researchers were D. Hawkins (1978), J. Clement (1982), J. Minstrell (1982), and M. McCloskey (1983a & b). David Hawkins formulated a succinct, early view of the presence and influence of false ideas that systematically block science learning; he called them "critical barriers." Terminology for student ideas has been diverse, including alternative conceptions, alternative frameworks, intuitive or naïve theories, and naïve beliefs. However, the term "misconceptions" caught the broadest public.

One of the great positive influences of misconceptions studies was bringing the importance of educational research into practical instructional circles. Educators saw vivid examples of students responding to apparently simple, core conceptual questions in nonnormative ways. Poor performance in response to such basic questions, often years into the instructional process, could not be dismissed. One did not need refined theories to understand the apparent cause: entrenched, "deeply held," but false prior ideas. The obvious solution was very often phrased, as in the quotation heading this section, in terms of "overcoming," or in terms of convincing students to abandon prior conceptions.

This simple story – entrenched but false prior beliefs interfere with learning and need to be overcome – drove much of the misconceptions movement. Many of the leading researchers developed more refined views, some elements of which we review in the next section. However, public impact and the broadest swath of research remained largely at the primitive level of documenting misconceptions: "entrenched false beliefs that need to be overcome."

Three Early Threads

THE ANALOGY WITH THE HISTORY OF SCIENCE

Three important and related threads in conceptual change research came to prominence nearly simultaneously early in the misconceptions movement. The richest and arguably the most generally influential was the analogy of the development of students' ideas with the history of science. Susan Carey (1991, 1999) was one of the earliest and most consistent in citing Kuhn's ideas in the context of children's conceptual change. She has systematically used the idea of incommensurability between conceptual systems as a primary index of conceptual change ("deep restructuring"). Incommensurability distinguishes conceptual change from "enrichment" (adding new ideas or beliefs) or even mere change of beliefs. Carey's main work was in biology, where she argued that children undergo a spontaneous and important conceptual change (Carey, 1985). The concepts "living," "real," "intentional" (as in having wishes and desires), and "animate," for example, are merged and confused before children sort out a true biology, where "alive" participates cleanly in a theory of "the body as a machine that sustains life," distinct from psychology and other domains. Carey began with observations by Piaget but argued that domain-independent theories of intelligence cannot explain changes in childhood biology. The extensive empirical and theoretical argumentation in Carey's work constituted an influential landmark, especially among developmental psychologists.

Carey also worked with Marianne Wiser in the domain of heat and temperature, where a prominent subthread of the analogy with the history of science appeared. Not only are the structures, critical attributes, and processes (concepts, theories, incommensurabilities, radical restructuring) similar or the same in children compared to the history of science, but content shows remarkable commonalities as well. Wiser and Carey (1983) built the case that naïve conceptions of heat and temperature parallel the ideas of an early group of scientists.

The analogy with history of science has been used in multiple ways. Karmiloff-Smith (1988) denied or downplayed content parallelism between child development and the history of science, but she highlighted process-of-change parallelisms. Similarly, Nersessian (1992) advocated the use of "cognitive-historical analysis" to determine empirically the processes involved in scientists' change of theories. Those processes include bootstrapping using analogical and imagistic models, thought experiments, and extreme cases. Nersessian projected that the same processes could be used to help students' conceptual change in school (p. 40).

THE THEORY THEORY

The second thread that emerged in early misconceptions research is closely related to the analogy with the history of science. *The theory theory* is the claim that children or beginning students have theories in very much the same sense that scientists have them. While this may have been inspired by the broader analogy with the history of science, it has often been invoked independent of content or process similarity. Carey consistently advocated a version of the theory theory. With respect to another domain, theories of mind, Allison Gopnik (Gopnik & Wellman, 1994) strongly advocated the theory theory. Gopnik was fairly extreme in the parallelism she claimed (while still admitting some differences between scientists and children, such as metacognitive awareness); others are more conservative in allowing such differences as limits in systematicity and breadth of application (Vosniadou, 2002). By and large, theory theorists align themselves strongly with the coherence side of the coherence/fragmentation fault line.

Michael McCloskey (1983a, 1983b) did a series of studies that became perhaps the most famous of all misconceptions studies. He claimed that students entered physics with a remarkably coherent and articulate theory that competed directly with Newtonian physics in instruction. The naïve theory, in fact, was very nearly the novice explanation of the toss (Figure 16.2). Within McCloskey's theory theory, he also proposed a strong content connection to medieval scientists' ideas, such as those of John Buridan and early claims of Galileo. In contrast to others, however, he made little of process-of-change similarities and, for example, did not refer in any depth to Kuhn or the philosophy of science. His attitude seemed to be that the content connection to the history of science was empirically evident rather than theoretically motivated.

McCloskey's work was incredibly influential. Despite counter arguments and empirical claims by others, McCloskey has often been cited authoritatively as showing that naïve ideas in physics are strongly coherent, and, indeed, theoretical (e.g., Wellman & Gelman, 1992, p. 347).

A RATIONAL VIEW OF CONCEPTUAL CHANGE

Another early landmark was the introduction of rational models of conceptual change. Rational models hold that students, like scientists, maintain current ideas unless there are good (rational) reasons to abandon them. Rationality is a highly contested idea, often fraught with cultural overtones of the superiority of Western scientific thinking as opposed to "primitive" irrational or mystical thinking (Goody, 1977). However, the rational view has proved persistent.

Posner, Strike, Hewson, and Gertzog (1982) established the first and possibly most important standard in rational models. They argued that students and scientists change their conceptual systems only when several conditions are met: (1) they became *dissatisfied* with their prior conceptions (experiencing a "sea of anomalies," in Kuhn's terms); (2) the new conception is *intelligible*; (3) the new conception should be more than intelligible, it should be *plausible*; (4) the new conception should appear *fruitful* for future pursuits (in Lakatos's [1970] terms: should contribute to a *progressing paradigm*).

The relationship of Posner et al.'s framework to studies in the history of science is complex. The framework drew from both Kuhn and Lakatos, even while the latter strongly criticized the former for abandoning rationality for "mob rule," and despite the implausibility that Kuhn's inherently sociological framework might transfer easily to individual students. Posner et al. drew equally from Kuhn's opponent, Toulmin, appropriating the idea of "conceptual ecology" (as opposed to "logical system").

A later version of this rational model of conceptual change (Strike & Posner, 1990) stepped back from a purely rational framework, admitting the importance of motivation and other such factors, while also admitting less articulate and less propositional forms of encoding (e.g., imagistic or enactive forms).

Despite continuing protests by Posner et al. that their framework was epistemological and did not reflect direct psychological reality – and, further, that it was far from a scheme for instruction (e.g., establishing these conditions as prerequisites to conceptual change) – many science educators were inspired to organize instruction around the framework (e.g., Smith et al., 1997). Some educators went so far as introduce students explicitly to Posner et al.'s framework (Hewson & Hennessey, 1992).

Assessing the Misconceptions Movement

POSITIVE CONTRIBUTIONS:

1. Misconceptions highlighted the importance of qualitative understanding and explanation against a historical background of emphasis purely on ability to solve quantitative problems.
2. Misconceptions established a broad prominence for contructivist thinking in instruction in contrast to prior, "blank slate" acquisition models of learning.
3. Misconceptions provided a focus for instructional problems, and new measures of attainment. It diminished attention to domain-general student difficulties (e.g., Piagetian stages), and emphasized the domain specific.

NEGATIVE CONTRIBUTIONS:

1. Recognizing important exceptions, most misconceptions studies were relatively devoid of theory development or testing. The "depth" of misconceptions was often uncalibrated, and an essential question was unasked: whether a wrong answer given by a significant number of students could always, on the face of it, count as a "conception."
2. Misconceptions work strongly emphasized negative contributions of prior knowledge. There were exceedingly few claims that prior concepts provided productive resources.
3. Following item 2, how learning is actually possible was minimally discussed.
4. Misconceptions studies led to a preemptive dominance for theory theory points of view and "conflict" models of instruction (e.g., item 1 of Posner et al.'s framework; also see McCloskey, 1983a, or Hewson & Hewson, 1983).

See Smith, diSessa, and Roschelle (1993) for an extended analysis of the misconceptions movement.

Beyond Misconceptions

I review work that transcended the misconceptions perspective in three categories: elaborated content analysis of conceptual change in particular domains; "knowledge in pieces"; and theory development.

Conceptual Change in Particular Domains

What, in detail, do students or children know about any particular domain at any particular age? Some lines of research that began in the heyday of misconceptions continued to ask and answer this question, with much greater precision and empirical support than previously.

Two domains stand out in developmental studies: naive psychology and naive biology.[5] Especially naïve biology has advanced in part in virtue of a substantial interacting community of researchers (Carey, Keil, Hatano, Inagaki, Atran, and others) who shared innovative methods, and critiqued and built on each other's ideas.

What, then, do children know about biology? Is learning the accretion of ideas, or enrichment, or, in contrast, does it involve "radical restructuring," which is true conceptual change? Carey (1985, 1986) argued that a conceptual change quite specific to biology occurs in childhood. Carey maintained that the concept of "animal" was embedded in a childhood theory of psychology; for example, that animals are distinct from inanimate objects in their perception- and goal-directed activities. She argued that, because of a near total ignorance of bodily mechanisms, a childhood biology is established only by about age ten, and it represents a true conceptual change from prior, psychological, ideas.

The burgeoning field of developmental studies of biological knowledge revised and refined Carey's ideas, and it provides at present a wonderfully enriched view of the emergence of biological knowledge. Two prime innovators were Frank Keil and Giyoo Hatano (with his collaborator, Kayoko Inagaki). These researchers pushed the emergence of distinctive biological thinking back at least six years. Keil developed an extensive program that isolated children's sensitivity to biological phenomena in multiple strands, including the biological essence of animals, inheritance, biological contagion, and the distinctive properties of the "insides" of living things. In the latter category, Keil (1994) showed that children age four or even earlier systematically expect the insides of rocks to be random while plants' and animals' insides are more organized. In one of his studies, Keil showed that very young children believe it is easy to paint or in other ways physically change a skunk into a raccoon, whereas older children, still before the age of six, feel such operations cannot essentially change the creature. He also showed that parents and offspring become much more important in establishing the essence of animals, compared to visible form or behavior.

Inagaki and Hatano (2002) brought a new degree of order to early biology by describing a core theory, which they called a *vitalist* biology. In this theory, the ingestion and use of some vague vital force accounts for animals' (and, later, plants') activity, health, and growth. Their experiments established a vitalist biology by age six, if not earlier. Hatano, Inagaki, and Keil argued and produced data to dispute Carey's earlier claim that biology emerged from psychology, transforming previously psychological concepts "animal" and "alive." Inagaki and Hatano also claimed that a subsequent shift – from a vitalist biology to a mechanistic one – constituted a true conceptual change during childhood.

Knowledge in Pieces

The theory theory thread dominates studies of naïve biology (and naïve psychology). It represents the "coherence" side of the coherence/fragmentation fault line. This section introduces a historical line, which I call "knowledge in pieces," that takes the "fragmented" side of the issue. Both Minstrell and I were early advocates of knowledge in pieces. Thus, we advanced Toulmin's critique of strong coherence.

From an instructional perspective, Minstrell (1982, 1989) viewed intuitive ideas as resources much more than blocks to conceptual change in physics, in contrast with the predominant misconceptions point of view. He described intuitive ideas as threads that, rather than rejecting, need reweaving into a different, stronger, and more normative conceptual fabric. Recent work has charted hundreds of "facets" – which are elemental and instructionally relevant ideas students have upon entering instruction – in many topics in physics instruction (Hunt & Minstrell, 1994). Coherent naïve theories are nowhere to be seen in this view.

In the same book in which McCloskey provided perhaps his definitive statement of "naïve theories," I (1983) introduced the idea that intuitive physics consisted largely of hundreds or thousands of elements, called p-prims, at roughly the size-scale of Minstrell's facets. P-prims are explanatorily primitive, provide people with their sense of which events are natural, which are surprising, and why. P-prims are many, loosely organized and sometimes highly contextual, so that the word "theory" is highly inappropriate. P-prims are hypothesized to play many productive roles in learning physics. For example, "Ohm's p-prim" prescribes that more effort begets more result, and a greater resistance to that effort begets less result. Ohm's p-prim applies to a very broad set of circumstances, including moving everyday objects (where size might be a "resistance" variable) and personal psychology (the reason for "greater effort" in the face of failure), and it accounts for the relative learnability of Ohm's law in electrical circuit theory. Several of the productive elements of novice students' toss explanation (Figure 16.2) – such as balance are p-prims.

The idea of knowledge in pieces has been all but ignored in developmental psychology. Even in early educational studies, syntheses of conceptual change research emphasized theory theory perspectives, and reported the ideas of researchers supporting fragmentation as minority opinions (see, for example, Driver, 1989, and Smith et al., 1997). A more extensive review of the history of theory theory versus knowledge in pieces appears in (diSessa, Gillespie, & Esterly 2004).

However, the knowledge in pieces perspective has picked up steam, at least in educational circles. Marcia Linn, for example, has elaborated "scaffolded knowledge integration" as an instructional framework (see Linn, this volume). In this view, the multiplicity of intuitive ideas is explicitly recognized, and integration (increasing systematicity) is virtually the definition of conceptual advancement.

Fragments of Theory

I referred earlier to conceptual change research as kaleidoscopic in the sense that many threads are combined in diverse ways into many different theoretical perspectives. This section follows that observation by sketching the theoretical landscape of conceptual change in terms of glosses on a number of theoretical threads. The alternative would be to select a few leading theorists and to present their perspectives in more detail. But this would produce a less broad and less illuminating view of the theoretical landscape of conceptual change.

I discuss two groups of theoretical issues. First, what are the mental entities involved in conceptual change, and how are they organized? Second, why is conceptual change difficult, and how does it happen when it does?

ENTITIES AND SYSTEMS IN CONCEPTUAL CHANGE

The obvious answer to the question of what changes in conceptual change is "concepts." However, this scarcely does justice to the diverse assumptions pursued by the field.

A perspective advocated early on by Carey makes an excellent entry point. Carey (1986) distinguished beliefs from concepts. Beliefs are relational entities. For example, "people are animals" relates two important concepts, people and animals; Newton's laws (summarized by $F = ma$) relate the concepts force, mass, and acceleration. Carey believed that belief change is relatively easy, and that the difficulty is change in the very concepts in which beliefs are expressed. When children eventually come to believe that people are animals, a very different concept of animal is implicated, compared to their earlier beliefs.

Carey is, as mentioned, an adherent of the theory theory perspective. Concepts and beliefs are constituents of larger-scaled systems, intuitive theories, which strongly constrain concepts and, hence, beliefs. Generalizing, most theories of conceptual change are *nested* in the sense that at least two levels (entities and systems) are implicated, and the relational constraints involved in the higher level are critical; those relations constrain individual concepts and beliefs so that incremental change is difficult or impossible. Of course, this brings us directly to the core of the coherence vs. fragmentation fault line. If relational constraints are too tight, then the difficulty of change is insuperable. So, a great deal rests precisely on the relations at the system level, which is a theory in Carey's case.

I mention three other nested theories. Vosniadou has proposed two versions. In her work concerning children's models of the earth's shape, she implicated vague but strongly persistent *framework theories*. When children are asked questions about the shape of the earth, their framework theories constrain a generation of specific models to a few possibilities. So, in this case, *models* are nested in, and constrained by, (framework) theories. Models can change relatively easily, but the framework theories take a long time. In more recent work, Vosniadou extended her theory to deal with force and motion. In Ioannides and Vosniadou (2002), framework theories constrain meanings (not models) such as "force," and the higher, relational level (theories) is still the real locus of difficulty in change.[6]

Keil (1994) introduced the idea of *modes of construal* as a weaker version of theories at the system level. Modes of construal weaken the presumed relational structure and simultaneously appear to address the issue that some ways of thinking cross core domain boundaries. For example, the natural home of teleological (purpose-oriented) thinking is in psychology. Yet, thinking in terms of purposes is important in naïve biology: "Fingers are for picking things up; hearts are for pumping blood."

A third nested view of conceptual change involves concepts as entities, but the higher level is not theories, but ontologies. Micheline Chi (1992) posited that concepts are strongly constrained by their presumed ontological nature. In her early work on this view, intuitive physics was claimed to be largely bound up in the ontology of matter, but Newtonian physics lies predominantly in an ontology very different from matter, "constraint-based processes." Shifting ontologies, like shifting theories, is very difficult. Often, the new ontology has to be developed first, and then new concepts or radically revised concepts can grow more naturally within the new ontology.

Contrasting with theory-theory perspectives, adherents to knowledge in pieces tend to rely less on terms provided by the history and philosophy of science (concepts, theories, ontologies). In my own work, I have used a series of constructs, each with its own definition and model: following a rough spectrum from low to higher level, "p-prims," "nominal facts," "narratives," "mental models," and "coordination classes" (diSessa, 1996; diSessa & Wagner, 2005). A coordination class is an explicit model of a certain kind of concept. Coordination classes are complex systems that include many coordinated parts, including p-prims. A distinctive characteristic of this view of conceptual change is multilevel nesting of multiple cognitive types: p-prims are nested in coordination classes (concepts), and coordination classes – along with mental models and other entities – constitute the "conceptual ecology" of students.[7] Constraints arising from nesting exist in this view also, but they are diverse (according to particular entities and levels in the system) and weak (compared to "logically consistent"). See diSessa and Wagner (2005) for a review of studies that use the coordination class model.

MODELS OF CONSTRAINT AND CHANGE

Following Kuhn, incommensurability has been a proposed locus of difficulty in change. In view of incommensurability, how is conceptual change at all possible? Inspired by the history of science, various researchers have proposed such mechanisms as analogy and use of imagistic models. A common assumption is that differentiation of diffuse initial concepts (heat and temperature become distinct), and coalescence of old categories (plants and animals become joined in the category of living things) are generic processes that must take place in overcoming incommensurability (Carey, 1986 and 1999; Smith et al., 1997).

Knowledge in pieces leads to a broader set of hypotheses about difficulties of change and feasible pathways. Any particular version of a nested model provides specific kinds of difficulties and specific pathways. However, a more generic type of difficulty is the mere collection and coordination of a large set of elements into a newly organized system. For example, the coordination class model proposes that the use of one concept might involve distinct knowledge in different circumstances. Not only is it necessary to collect a large amount of situation-specific knowledge for use in different situations, but ensuring that the concept works in functionally the same way in different contexts is problematic and takes time. A distinctive characteristic of the knowledge in pieces perspective is that the reasons for difficulty of change may be the same in cases where a conceptual structure evolves from scratch, compared to cases where one conceptual system emerges from a different one (theory change).[8] Collecting and coordinating elements is difficult whether or not a prior competitor system exists. Indeed, one of the key difficulties might be creating an entirely new level of breadth and coherence, compared to naïve ideas.

Rational models continue to be surprisingly well regarded, despite sparse evidence for their adequacy in dealing with conceptual change. Gopnik and Wellman (1994) mentioned most of the same elements as Posner et al., including the accumulation of counter evidence, and a version of the requirement that an intelligible and plausible replacement exists. In terms of blockage or resistance, they mentioned quasi-rational processes resonant with professional science, such as denial of the need for change, and the formation of auxiliary hypotheses to fend off deep change.

Although, as I mentioned, Piaget's equilibration has diminished in popularity, some new versions have appeared. Inagaki and Hatano (2002, pp. 173–175), for example, provided two models of conceptual change where new ideas disturb the coherence of prior ideas, and reestablishing coherence drives conceptual change. See also Ioannides and Vosniadou (2002, p. 58).

Instruction and Intuitive Epistemology

This section mentions issues that cannot be treated in detail here. Of principal importance for conceptual change research is improving instruction. Theory theory views and knowledge in pieces prescribe some strong differences in strategy and process (e.g., rational decision making vs. a long period of multicontext accumulation and coordination). However, evaluating interventions has been indecisive for the following sorts of reasons.

1. Instruction is a complex mixture of design and theory, and good intuitive design can override the power of theory to prescribe or explain successful methods. Almost all reported innovative interventions work; almost none of them lead to improvements that distinguish them categorically from other good instruction.
2. The very general constructivist heuristic of paying attention to naïve ideas seems powerful, independent of the details of conceptual change theory. Interventions that merely teach teachers about naïve ideas have been surprisingly successful.
3. Researchers of different theoretical persuasions often advocate similar instructional strategies, if for different reasons. Both adherents of knowledge in pieces and of theory theories advocate student discussion, whether to draw out and reweave elements of naïve knowledge, or to make students aware of their prior theories in preparation for judgment in comparison to instructed ideas. The use of instructional analogies, metaphors, and visual models is widespread and not theory-distinctive.
4. Many or most interventions rely primarily on pre/post evaluations, which do little to evaluate specific processes of conceptual change.

"Intuitive epistemologies" concern how children and students view the nature of knowledge. Posner et al. mentioned student epistemological attitudes, particularly concerning rationality. Since then, a substantial literature has proposed different versions of "student ideas about knowledge," and it seems clear that these exert powerful influence (consult, for example, Sinatra & Pintrich, 2002, and Hofer & Pintrich, 2002).

Finally, conceptual change has its own distinctive set of sociocultural issues, such as the contribution of culture to intuitive theories. I note here simply that sociocultural views of conceptual development must at least be compatible with the cognitive/individual ones described here, and vice versa.

Mapping the Frontier

I close with suggestions about what seem to be the best near-future pursuits in studying conceptual change. I focus on the persistent fault line of coherence versus fragmentation, and on a set of more general recommendations concerning near-future pursuits.

Coherence: A Central Fault line

Historically, I introduced the issue of coherence vs. fragmentation in terms of the debate between Kuhn, who has been immensely influential in conceptual change research, and Toulmin, who has been less influential. Toulmin emphasized problems of overestimating systematicity and proposed that we need to rethink it. Kuhn motivated the idea of theory change in studies of conceptual change, in which the coherence of different theories provides for an essential incommensurability across them, and hence the necessity of holistic change. Coherence is a byword of theory theorists; it is recognized as the defining attribute that distinguishes naïve theories from other forms of knowledge (e.g., Wellman & Gelman, 1992); and it is among the most common adjectives describing naïve conceptions in the broadest range of conceptual change research. Yet few explicit models of coherence exist,[9] and still less empirical work has been done to test them.

This fault line has not been well articulated in the history of conceptual change research. Theory theorists with a strong commitment to coherence have held sway, particularly in developmental psychology. Many influential researchers completely ignore the issue, while others briefly mention fragmentation views as "minority opinions." Others, such as Chi (1992, p. 161), dismiss the issue as unimportant.

However, the tide seems to be turning, both in terms of facing the issue and in terms of the center of gravity in the debate. Recent studies have explicitly provided empirical or theoretical argument and counter-argument concerning coherence in naïve ideas (Ioannides & Vosniadou, 2002; diSessa et al., 2004). In educational studies the ascendancy of views favoring fragmentation seems vivid; both Linn's instructional framework, and Minstrell's facet analysis of learning physics, have been extensively developed and brought to widespread instructional practice, whereas no views based on the idea of intuitive theories have passed beyond research prototypes. No "naïve theories" have become influential, for example, in extensive, empirically based curriculum development in physics (e.g., McDermott et al., 2002). Finally, the trend in intuitive epistemologies seems distinctly toward knowledge in pieces views, and away from "intuitive theories of knowledge" (Hammer & Elby, 2002).

The modern view of the content of intuitive biology, despite the commitment to the idea of naïve theories by many researchers, has trends that undermine strong systematicity. In particular, the multiple lines in biological knowledge developed by Keil and others beg the question of how much they cohere with each other. Vitalism, as described by Inagaki and Hatano, is only a part of naïve biology, not a full, pervasive theory. And the "theory" that defines the mechanistic phase of intuitive biology is not succinctly characterized, nor is its coherence empirically measured. Inagaki and Hatano show that more primitive ways of reasoning about biology than naïve theories (based, for example, on similarity rather than biological categories) persist into adulthood. Adults also use vitalism long past the supposed transition to mechanistic biology. How is this consistent with strongly coherent theories and gestalt switch transitions?

Settling the coherence/fragmentation dispute requires theory development. We need better cognitive models and more precise coordination between models and evidence. The metaphor of theory drawn from the history of science ambiguously covers both strongly and weakly systematic knowledge systems, as exemplified by Kuhn and Toulmin's debate (even if theory theorists emphasize coherence). Specifications such as "theories have domains, are abstract, define distinctive explanatory ontologies, and are coherent" are immensely ambiguous as cognitive models. The field must do better.

The debate over coherence versus fragmentation is subtle. No one thinks children are completely unsystematic in their thinking about domains such as physics or biology. Furthermore, all existing views of scientific competence, when actually achieved by students, entail substantial systematicity (see diSessa, 1993, concerning the emergence of systematicity from the knowledge in pieces point of view). The central issue, rather, is *specification of the nature and extent of systematicity*. A proper resolution involves more precise models and better empirical determination of their parameters.[10]

Foci for Near-Future Work

The purpose of this review has been to prepare readers (a) to understand the history and state of conceptual change, and (b) to help orient researchers to productive avenues of future pursuit. Here is a list of suggestions.

1. *Pursue detailed specification of the content development of conceptual domains.* For example, the rich empirical work in naïve biology provides a protean resource for developing and testing theories of conceptual change. In addition, educational application will likely depend as much on content details as on general theoretical schemes.
2. *Assume domain variation, and empirically validate commonalities (or intractable differences).* Almost all conceptual change research homogenizes domains theoretically. But, consider the nature of conceptual competence in three regimes.
   a. Babies may be surprised by certain events (the core methodology of studies of infant conceptualization), but the extent of instrumental use of the schemas involved is highly uncertain. Data show a long, gradual emergence of, for example, adult common sense about gravity, and it cannot be taken for granted that the nature of "concept" involved has much in common from babies to adult common sense.
   b. Pre- and early-elementary school students integrate a large number of observations and ideas into their naïve biology, almost certainly under far less innate guidance than baby causality. Are the principles of conceptual growth and even the meaning of "concept" at all comparable at these two levels of development?
   c. Learning the concept of force in physics almost certainly builds on one of the richest and most directly useful (in everyday actions) naïve domains. Can such a difference in context *not* be consequential in development, compared to other domains? The context of use of the instructed concept of force is "high stress," where an extended series of problems require precise, reductive, and quantitative analysis. Could that fact be irrelevant to the nature of conceptual development, compared to the low-stress use of naïve biology to make rough and ready sense of the biological world?
3. *Develop explicit models of constructs like "concept" and "theory," and test them*

against data; models need to highlight relational structure (coherence). Researchers need to commit themselves to particular constructs, with specified meanings. What is an "entrenched belief"? We have not progressed enough since Toulmin complained that no one ever says what a concept is. I also highlighted the critical importance of understanding the nature and level of coherence in naïve and instructed competences.

4. *Accept the challenge of process validation of models of entities and change.* Again, Toulmin presciently argued for abandoning both snapshot models of conceptual change and snapshot validation of theories of change. Everyone agrees change is slow; but few models exist to track the slow progress.
5. *Make contextuality a central concern.* The body of research on intuitive physics often reveals sensitive dependence on problem context, on framing of questions, on modality (viewing, drawing, enacting), and so on (diSessa et al., 2004). In naïve biology, subjects reveal early vitalist sensitivities only when explicitly prompted. Can this be inconsequential to the nature of the child's "theory"? To offer a comparable example from physics, articulate, "high-stress" use of the schooled concept of force, where a claim such as "students think of using $F = ma$ only when prompted" would be unacceptable. More generally, developmental studies consistently report intrusion of one way of thinking into others (e.g., psychology intrudes on biology; or weight intrudes on density). When? Why? What are the general implications?

Footnotes

1. The conceptual change paradigm is systematically less often applied in mathematics.
2. That force acts directly on velocity is the essence of the expert explanation in Figure 16.1.
3. Kuhn carefully guarded the coverage of his theory. Fields that (1) do not have the Normal Science phases of puzzle solving, that (2) have multiple, persistent schools of thought, or in which (3) rights of judgment are not strictly confined to specialists, simply are not candidates for his model. It seems unlikely that Kuhn would have sanctioned use of his ideas for conceptual development outside of professional research communities of a very restrictive sort.
4. Toulmin did not restrict his critique to strictly logical forms of sytematicity. For example, he also rejected Collingwood's model of "systematicity in hierarchical presumptions."
5. Naïve physics is a third important developmental domain. However, coverage was spotty compared to psychology and biology. Studies of baby cognition of physics revealed stunning early competence and development (Spelke, Philips, & Woodword, 1995; Baillargeon, 1986). But middle years received less attention, and late development was mainly left to misconceptions researchers.
6. Vosniadou often emphasized the nature of framework theories as "background assumptions," similar to Collingwood's view, which Toulmin criticized for lack of rendering such assumptions explicit and testable.
7. In contrast, Carey has concepts embedded in theories, but concepts are not described as decomposable objects.
8. Incommensurability does not make obvious sense when no prior competitive system exists.
9. Thagard (2000) is a notable exception.
10. Few if any researchers believe that intuitive ideas are deductively coherent. Yet explicit alternative forms of coherence are rarely presented and also generally vague. Wellman and Gelman (1992) mention two meanings for coherence: lack of contradiction (which, as they point out, could apply to a set of beliefs that have nothing to do with one another), and the idea that concepts refer to each other (where "reference" is undefined).

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