Chapter 17
Where Are We? Syntheses and Synergies in Science Education Research and Practice

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17.1 Introduction

I begin with some personal history. I graduated from my undergraduate institution with a B.A. in physics, and the full intention of obtaining a Ph.D. in that same discipline. However, after about 2 years of graduate school I determined that I was in the wrong place; it turned out that I greatly enjoyed *learning* physics, but that I was less interested in spending many years of my life working in one tiny corner of the field. It was thus that I found my way to educational research.

It was an exciting time both for me and for educational research in general. The cognitive revolution (Gardner, 1987) was gaining traction, and computers seemed to have the potential to be transformational tools for education. In my new graduate program in educational research I was beginning to read material that was quite different than what I had read as a physics student. There was Piaget, the basics of cognitive science and artificial intelligence, and the new applications of this work to the study of learning.

But I was struggling to make it all fit together. For example, I did not fully understand what Piaget meant by "logical structures" or "operations" (e.g., Inhelder & Piaget, 1958). Nor did I understand how these ideas were similar or different than, for example, what the most current cognitive scientists were writing. I expected learning theory to be like physics – maybe not exactly, but at least close. But I gradually came to understand that this expectation was not one that would be fulfilled. There weren't principles or even definitions that researchers agreed upon. Researchers used terms such as "concept," "metacognition," and "strategy," that I thought were intended to be technical terms, and hence well-defined. But even

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O. Levrini et al. (eds.), *Engaging with Contemporary Challenges through Science Education Research*, Contributions from Science Education Research 9,
https://doi.org/10.1007/978-3-030-74490-8_17

within individual journal articles, these terms were neither explicitly nor consistently defined. This was hard news for the junior physicist in me 30 years ago.

The even worse news is that the situation has not substantially improved. Thirty years ago, it seemed possible that the cognitive revolution would provide a basis for a real, shared science of science learning. But that has not happened. We have learned—we have made some progress. But there is certainly not a shared theory and well-defined terminology. The purpose of this paper is to do some work in that direction, or at least to name some of the bigger problems.

The talk on which this paper is based was itself based on work done for a book, *Converging perspectives on conceptual change*, which was guided and assembled by Olivia Levrini and Tamer Amin (Amin & Levrini, 2018; Sherin, 2018). Tamer Amin has himself been an author of some of the best attempts to synthesize the range of existing work on conceptual change in science, and his work has shaped the view presented here (Amin, 2009; Amin et al., 2014).

For the earlier book, I was asked to put together a section on the nature of concepts and conceptual change. Multiple authors contributed, and my job was to synthesize those contributions. However, unlike the earlier book, here I focus on an individual, cognitive level of analysis, since I understand that to be my charge.

17.2 What Is "Conceptual Change"?

Conceptual change has no precise and shared definition. It can be taken to apply to any learning events in which there are changing "concepts." When used in that way, conceptual change is essentially a synonym for learning. But, more frequently, we use the phrase *conceptual change* to apply to a subset of learning—learning that is dramatic in scope or, in some manner, difficult to attain. So, for example, learning to understand Darwin's theory of evolution might well require conceptual change; learning that mammals give birth to live young probably does not.

In the field of science education, we often mean something still more specific when we speak of conceptual change. The idea that we begin with is that students have knowledge of the natural world that they gain prior to formal instruction, both from direct interaction with the world, as well as communication with other humans. The story of conceptual change is often taken to be the story of how this pre-existing knowledge changes; it is the process through which a learner makes difficult or extensive change to existing knowledge about the workings of the world.

Understood in this more narrow sense, research on conceptual change in science is concerned with three questions:

1. What is the nature of students' pre-instruction knowledge of the natural world? (i.e., what is the "stuff" that is changing?)
2. How does this knowledge change as learners develop and scientific expertise is acquired? (i.e., What are the processes of change?)
3. What is *difficult* about this change?

What I'll attempt to show in the rest of the paper is that there is no consensus about the answers to these questions. This is particularly striking given how long we have been seeking answers. In fact, I first encountered them in a graduate course on conceptual change, taught by Andrea diSessa and Ann Brown, sometime around 1990.

The remainder of this paper will have three parts. In the first part I'm going to give something like an illustrated tour of different ways the three questions have been answered. Then, in the second part I'm going to talk about impediments to progress, that is, why we have not achieved consensus on answers to the questions. Then, in the last section I will briefly discuss how we might make further progress. I feel I must warn the reader before beginning. Identifying the problems is a relatively easy endeavor. But offering solutions remains difficult, and my suggestions, at best, offer only small steps forward.

17.3 An Illustrated Tour

17.3.1 Conceptual Change in Physics

Our tour begins in physics. The domain of physics has been one of the main foci of research in conceptual change. This is partly historical accident, but also partly because of features of the domain. We have rich everyday experiences of the physical world and learning formal physics does seem to require a dramatically new way of understanding the world. One of my favorite examples to illustrate this point is that, from the point of view of Newtonian physics, an automobile moves forward because the road pushes it forward. Even diehard physicists must admit that there is something intuitively bizarre about this notion.

Much of the early research on physics learning concerned how students understand the motion of projectiles. I'm going to illustrate some issues with a bit of data. It's drawn from project that that I was part of with Andrea diSessa and David Hammer, while I was graduate student, and it has become classic data—it has been discussed in many prior journal articles and chapters (e.g., diSessa & Sherin, 1998). The episode begins with the interviewer (diSessa) asking the university student, who he calls "J", to talk about the forces acting on a ball that is tossed straight up and falls back down. J immediately answers as follows:

> Not including your hand, like if you just let it go up and come down, the only force on that is gravity. And so it starts off with the most speed when it leaves your hand, and the higher it goes, it slows down to the point where it stops. And then comes back down. And so, but the whole time, the only force on that is the force of gravity, except the force of your hand when you catch it.

If you know your physics, then you will have noticed that J's answer here seems unproblematic. Most notably, she says that the only force acting on the ball is the force of gravity. But the interview immediately took a dramatic turn. The interviewer asked J to focus on what happens at the peak of the toss, and J gave a somewhat meandering response:

> …when you throw it, you're giving it a force upward, but the force can only last so long against air and against gravity ... But so you give this initial force, and it's going up just fine, slower and slower because gravity is pulling on it and pulling on it. And it gets to the point to the top, and then it's not getting any more energy to go up. You're not giving any more forces, so the only force it has on it is gravity and it comes right back down. ...

I want to first draw your attention to the last part of J's statements here. The key point is that, in her initial explanation, J said there was only one force acting on the ball, the force of gravity. But, by the end of the second passage, she is saying that there are *two* forces: gravity and a force that is imparted by the hand.

This latter passage provides a nice entry into one way in which conceptual change has been talked about in physics. The idea, according to some researchers, is students such as J can be said to have a "naïve theory" of physics; in particular, J's answer might be seen as consistent with what has been called the "impetus theory" (e.g., McCloskey, 1984). Like any good theory, the impetus theory is seen to have its own principles:

- Motion requires a causal explanation.
- Forces can be stored up in objects. These stored forces are the "impetus."
- The motion of an object depends, in some manner, on a combination of the internal and external forces.
- Sometimes the stored force just dies away on its own. Sometimes an external force can cause the stored force to die away.

Hopefully, you can see how J's response might be seen to align with these principles. She says that, when a ball is tossed upward, the ball is given an upward force, which is then diminished by gravity.

This way of understanding what is going on in the episode provides us with our first set of answers to the three questions about conceptual change:

What is changing in conceptual change? The idea is that students, such as J, begin with a theory—the impetus theory. This is the "stuff" that must change.

What are the processes of change? The process of change, at least at a high level, is one of *theory replacement*; the impetus theory must be replaced by Newtonian theory.

Why is this change hard? It seems reasonable to expect that replacing one theory with another would be difficult. In addition, we are told that the impetus theory is particularly stubborn and resistant to change.

Now I want to present an alternative perspective on what is going on in this episode with J, and it is one that is offered by diSessa himself (diSessa, 1996). diSessa's view is rooted in the idea that, in episodes such as this one, student explanations are often constructed, in the moment, built out of smaller ideas. In particular, he draws attention to a number of small schemas that he calls phenomenological primitives or, more affectionately, p-prims (diSessa, 1993). These p-prims are cued — brought to mind — for reasons that can depend sensitively on what is going on in the moment. They include, for examples, notions having to do with balancing and equilibrium, force and agency, and constraint phenomena.

Now let me play out diSessa's analysis of J's explanation of the tossed ball. The central idea is that asking J to focus on what is happening at the peak cues her to think about *balancing*. The top of the toss, for many learners, is strongly suggestive of balancing. Talking about the peak also probably cues a typical *overcoming* scenario. There are two competing influences, one of which ultimately prevails, overcoming the other. But this leaves J with a problem to solve. She only has one force, the force of gravity; thus, she needs a second force. Over the course of her response, she considers a few alternatives, but ultimately settles on the introduction of an impetus-like force.

diSessa's perspective yields another set of answers to our three questions:

What is changing in conceptual change? There is a large system of elements that includes, many p-prims, as well as other unnamed elements.

What are the processes of change? According to diSessa, this large system of elements gets tuned up so that we use the right p-prims at the right time. Some become less important, some more important. In addition, some p-prims are adapted to play special roles in formal physics.

Why is this change hard? The answer to this question is notably different than the one given by theory-theorists. Change, according to diSessa, is not difficult because there is a single large structure that is difficult to displace. Rather, it's difficult because many small coordinated changes must be made to a system consisting of numerous elements.

17.3.2 Conceptual Change in Astronomy

Intuitive astronomy is another domain that has played a central role in thinking about conceptual change. Again, I believe the reasons for its importance are partly historical; but there are also properties of the domain that have made it an interesting focus for study. As in the domain of physics, we have some everyday access to astronomical phenomena. We can see the sun and stars. However, our access seems somewhat more remote than our experience, for example, with projectiles. We cannot directly observe the shape of orbits, and there are profound issues of scale involved.

More importantly, social inputs at least *seem* to be more important in astronomy. Even when children have not yet learned about the stars and planets in school, they have likely heard about them as part of their everyday lives, in children's books and stories, and beyond.

I am going to again illustrate this part of my tour with a brief bit of transcript. Here I draw on some prior work done with colleagues at Northwestern University, where we asked U.S. middle school students to explain the causes of the Earth's seasons (Sherin et al., 2012). This interview topic has had a long history in science education research. Part of this question's power is that it *seems* mundane, and most people seem to feel that they *should* be able to answer accurately. But the answer is actually quite subtle. Here, I began by asking a student, Angela, "why it's warmer in the summer and colder in the winter." She answered:

> That's because the like sun is in the center and the Earth moves around the sun and the Earth is at one point like in the winter, it's on it's like farther away from the sun and towards the summer it's closer it's near, towards the sun.

In some ways, Angela's answer is unsurprising, and it is not too difficult to speculate about the origins of her answer. Angela knows that the sun is a source of warmth, and she certainly knows that if you're closer to a heat source, then you feel the heat of it more strongly. On top of that, she has probably heard that the Earth orbits the sun in an ellipse, which implies that it is sometimes closer to the sun and sometimes farther away. Thus, Angela's explanation is a very sensible construction, one that in fact includes many elements of the accepted explanation.

In my research group's interviews, Angela's answer was one among a variety of explanation that we observed, which in turn were among the larger set reported in the literature. We called explanations like Angela's *closer-farther* explanation. We also saw *side-based* explanations, in which seasons are explained by the fact that the Earth rotates, and the side facing the sun experiences summer, and tilt-based explanation, in which the hemisphere tilted toward the sun experiences summer.

An important point here is that many of these explanations seem like an amalgam of informally gained knowledge and school knowledge. For example, Angela's explanation combines everyday knowledge about heat sources with knowledge that the Earth's orbit is elliptical.

Now I want to turn to one prominent way in which conceptual change in astronomy has been understood. The work of Stella Vosniadou and colleagues provides an important reference point (Vosniadou & Brewer, 1992; Vosniadou & Skopeliti, 2014), and she was one of the contributors to the book from which this paper is descended. Here I am going to draw on some of her own words from the edited book (Vosniadou, 2018).

At the core of Vosniadou's account is what she calls a *framework theory*. Like McCloskey and others, she uses the term "theory." However, she means something quite different than these prior authors. She emphasizes that framework theories are not "well formed."

> A 'framework theory' is not a well formed, explicit and socially shared scientific theory. Rather, it is a skeletal conceptual structure that grounds our deepest ontological commitments and causal devices in terms of which we understand a domain.

Another of Vosniadou's central assumptions is that, in the moment of an interview, a framework theory can guide the construction of mental models. Thus, the notion of framework theory is consistent with the idea that, during an interview, a learner might do significant work to construct a response. This is something that I believe is often missed in critiques of Vosniadou's work.

Another hallmark of Vosniadou's perspective is the notion that students develop what she calls *synthetic* or *hybrid* concepts. These are the "amalgams" that I mentioned earlier, where prior knowledge is combined with new inputs, for example, when students encounter ideas in formal schooling. Finally, a last important characteristic of Vosniadou's perspective is the belief that students are sensitive to issues of systematicity and coherence. She believes that learners are driven to forge coherence in their ideas.

We can now sum up by looking at how these ideas from the study of intuitive astronomy suggest we should answer our three questions about conceptual change:

What is changing in conceptual change? The short answer is framework theories.

What are the processes of change? Here I'll again quote Vosniadou from the edited volume:

> Learning science requires many conceptual changes in the framework theory, such as changes in categorization, in representation, and in epistemology and the creation of new concepts and new reasoning processes.

Thus, a framework theory must change, as a whole, but there is apparently some sub-structure to framework theories that can be adjusted. In addition, along the way, we should expect to encounter synthetic conceptions.

Why is this change hard? Here the answer is somewhat different than the answers offered by diSessa or McCloskey; Vosniadou might be seen to occupy a midpoint between them. She says:

> The conceptualization of initial understandings as a framework theory, rather than as singular and isolated units, explains why conceptual change is not a sudden replacement of intuitive conceptions with scientific ones.

Thus, like diSessa, Vosniadou argues that conceptual change is difficult because many coordinated changes are required. But, for Vosniadou, some of the difficulty also arises from the fact that initial knowledge is organized into a structure with its own intrinsic coherence and which is resistant to change.

17.3.3 Intuitive Biology

Next I take up the domain of biology. The important touchstone here is the work of Susan Carey and I'm going to use as a point of reference her 1985 book, *Conceptual change in childhood* as well as a shorter summary of that work (Carey, 1985, 1988). In that book, Carey examines the acquisition of biological knowledge between the ages of 4 and 10. She argues that there is a profound restructuring of biological knowledge over these years, and in support of her argument she musters a wide variety of psychological evidence. Here I will give you only the tiniest taste of the larger argument she builds.

Some of Carey's strongest evidence comes from what she calls "patterns of attribution." For example, children of differing ages were asked what things in the world can breathe, sleep, eat, etc. The data showed that 4-year-old children attribute all of these behaviors to people, but that the attribution of these properties to other entities depended on their similarity to people. This was something that she argued across of range of data, that young children answer what we would think of as biological questions by reference to humans and the domain of human experience.

In contrast, 10-year-old children answer these questions essentially as we would expect adults to answer. They know, for example, that all animals eat, breath, and have babies, but they restrict bones and hearts to vertebrates. So, unlike the younger children, 10-year-olds do not answer biological questions by reference to the domain of human experience. They reason from what they know about biological categories, and the functions that all organisms must perform. Carey sums up the big point this way (Carey, 1988):

> If I am correct, there is no domain of phenomena that are strictly biological for the 4-year-old. Phenomena such as eating, breathing, and sleeping are part of the domain of human activities. They are phenomena of the same sort as playing bathing, talking. (p. 23)

> The whys and wherefores of these matters, as the child understands them, include individual motivation (hunger, tiredness, avoiding pain, seeking pleasure) and social conventions. Asked why people eat, 4-year-olds answer "because they are hungry," or "because it is dinner time." (p. 23)

Now let's lay out answers to our three questions for Carey, and the domain of biology:

What is changing in conceptual change? Carey argues that children possess "only a few theory-like cognitive structures, in which their notions of causality are embedded and in terms of which their deep ontological commitments are explicated" (p. 25).

What are the processes of change? The change process is a kind of theory emergence. The idea is that biology understanding emerges out of other pre-existing theory-like structures having to do with human activities and psychology.

Why is this change hard? She emphasizes that this type of conceptual change is hard, and takes time, because it requires the acquisition of specific knowledge about such things as internal organs and biological functions. It's the weight of accumulation of this garden-variety knowledge that leads intuitive biology to bud off from intuitive psychology.

17.3.4 The Ontological Perspective

The final perspective I consider is drawn primarily from the work of Michelene Chi and colleagues (Chi, 1992, 2005). Chi's work is animated by a question that is perhaps slightly different than those addressed by the researchers I have so far discussed. She asks *Why are some misconceptions robust?* Her answer, in a nutshell, is that misconceptions are robust when a concept tends to be treated as belonging to the wrong ontological category.

An ontological category is, essentially, a category of kind of entity in the world. Two categories are ontologically distinct when the predicates of one category cannot be sensibly applied to entities in the other. For example, we can ask about the duration of an *event*, but not of a *physical object*. Thus, events and physical objects are ontologically distinct categories.

Chi and her colleagues pin some of the most important problems in science learning on one particular ontological confusion, between what they call *sequential* and *emergent* processes. In a sequential process, there are agents of distinct types, playing distinct roles. An example is the human circulatory system. In contrast, in an emergent process there are many equivalent agents, and simultaneous local interactions among these agents that result in a macro-scale pattern. An example of an emergent process is diffusion.

The ontological perspective suggests that we should answer our three questions as follows:

What is changing in conceptual change? Ontological categories and the assignment of entities to these categories.

What are the processes of change? Chi and colleagues argue that, in some cases new ontologies will need to be created. In the case of emergence and sequential ontologies, for example, the problem is not only that student mis-categorize emergent processes as sequential, they may actually lack the emergent category.

Why is this change hard? The lack of an ontological category can be a strong barrier to conceptual change.

17.4 Why Are We Stuck: Impediments to Consensus

I now move to the second main part of this paper, in which I try to lay out some of the reasons we have been stuck, and what the impediments are to achieving consensus. At this point, the reader might be wondering if we really are all that far from a consensus view. The perspectives presented above might seem to employ similar ideas and with some significant areas of overlap. Nonetheless, these perspectives are generally taken to be at odds, and I myself believe that many of the apparent similarities do not go too much deeper than the terminology employed.

So what are the problems? I think that there are a number of impediments keeping us form moving quickly to a consensus. I'll lay them out in the rest of this section.

17.4.1 Different Target Phenomena

The easiest impediment to explain is the one I call the problem of *different targets*. The issue is a simple but important one; namely, we are not all working on the same question, and we are not always aware that this is the case.

For example, one difference is that we are often concerned with processes at different timescales. For example, most of the researchers discussed above are concerned with conceptual change that occurs during classroom science instruction. These changes occur over an instructional timescale that can span days, weeks, or occasionally months. Typically, researchers who study this flavor of conceptual change focus somewhere in the middle of this range of timescales, on changes that occur over weeks. The focus here is on changes driven by formal instruction.

However, not all conceptual change researchers are concerned with the instructional timescale. In particular, researchers such as Carey are concerned with changes that occur over developmental time —the months or years during which a child matures. These changes may be driven by a wide range of impetuses, including maturation, everyday experience in the world, cultural transmission, and formal instruction.

17.4.2 Theoretical Incommensurabilities

Some more subtle impediments pertain to what I'll call *theoretical incommensurabilities.* Our theoretical accounts do align in some places, and often sound similar. But in many cases this surface similarity masks deep differences. I will start with an easier point, and then work up to some more difficult ones.

To begin, it is clear that we are looking at analyses that are, in a sense, at different levels. Some researchers are focused on constructs that attempt to capture change at the level of small, constituent levels of knowledge. Others are focused on change at the level of larger systems–ensembles–of knowledge. Indeed, quite a few other researchers have made a similar point (Amin et al., 2014). This might not be too difficult a problem to fix; in fact, it might not even be a true incommensurability, just a difference in focus.

Another important contrast is between theoretical terms that capture entities constructed in-the-moment versus terms that capture established knowledge structures. In the interview with J that I described, we saw that her explanation evolved over the course of a few minutes, as she pieced together an explanation that made sense. In cases such as that, in which a student assembles an explanation or model in the moment, we can choose to focus either on the model that is constructed, or on the existing knowledge out of which that model was built. This seems to me to be a relatively straightforward point. But I believe that there is quite a lot of confusion that could be eliminated if we are simply careful in this way.

But there are some theoretical issues that are more thorny and subtle. One phrase I use to capture some of these issues is *ontological slippage*. I believe that the nature of our own theoretical constructs slides around in profound ways. I'm going to do my best to explain what I'm talking about here. But I'm going to have to meander a bit and hope that it comes together for the reader by the end.

To start, I'll back up some. The terms in our theories get their meaning–and thus their ontological kind–from the larger theories in which they participate. For example, in Newtonian physics, *force* can be understood to be defined by the roles that it plays within that theory, notably its role in the eq. F = ma.

Now, in one class of accounts of conceptual change, our theories are cognitive models, and the entities in our theories are mental representations. So, suppose that I were to talk, in one of my papers, about the "concept of mammal." What might I mean by that? The point is that terms such as *concept* get their meaning from the theoretical framework within which they participate. In a cognitive framework, mental entities (representations) get their meaning from how they participate in models of cognitive processes. Thus, a mental entity corresponding to the concept of mammal would be defined by its causal role in our models of cognition.

Another possibility might be something that we might call "socially-shared" ideas or "ideas in the air." When I talk about the concept of mammal, I might be talking about a shared idea that exists in a field of study and is captured in textbooks. Using the term "concept" in this way is quite reasonable. But it means something very different than a cognitive-concept-of-mammal, which takes its meaning from the role it plays in a cognitive model. If you are an author writing about science "concepts," and I am reading your paper, then I expect to be able to tell which of these meanings of "concept" (or some other meaning) you have in mind. If you slip between multiple meanings, then you are guilty of ontological slippage.

Now, consider the case in which we say that a student "has the impetus theory" or "has Newtonian theory." Note that such a statement generally does not say anything explicit about mental representations–elements of a cognitive model. Rather, I think it is best understood as saying that a learner behaves in a manner that is consistent with a particular set of ideas-in-the-air. It is thus a different kind of account than a cognitive account that seeks to identify elements that are defined by their roles in cognitive models. The entities are ontologically different. This isn't necessary a bad thing. But it is very bad if we are not aware of these differences.

Here is a last point that might be controversial: I believe that Chi's ontological distinction between emergent and sequential processes is primarily an analysis given in terms of ideas-in-the-air. The distinction, as I understand it, is intended to give us a way of opening up a science textbook, looking at the subject matter, and determining which topics will be difficult.

17.4.3 Empirical Differences

The last set of impediments are empirical differences. The interesting point is that it is actually quite difficult to find many true empirical disagreements in the literature, places where researchers have obtained contradictory experimental results. Where these contradictory results do exist, most of these relate to the infamous coherence debate (diSessa, 2013), which often seems to boil down to the question of whether learners hold consistent models and give consistent answers.

Even in these cases, however, I believe the differences often end up being theoretical rather than empirical. In many cases, for example, different experiments produce contradictory results because they use different methods; thus, they ultimately reduce down to questions about the right way to ask questions and interpret results.

In the end, empirical tests are not the right way to go about settling the debates here. This debate, and others, will be settled by looking at the differences in explanatory power of theoretical perspectives across a range of experimental results.

17.5 Toward a Synthesis and Further Progress

Is it possible for us to make progress toward a shared synthesis? Based on our past history, I must admit to not being particularly optimistic, but I do believe some small steps forward are possible. First, I believe we should recognize that the big problems are theoretical and not empirical. Our problem is conceptual clarity.

The "different targets" problem points to some easy steps we can take to begin to achieve this conceptual clarity. Forging a synthesis will mean, in some cases, recognizing that we are sometimes working on relatively distinct sub-parts of a larger endeavor. This does not necessarily mean that these sub-disciplines are unrelated, however. For example, work on developmental phenomena can constrain our cognitive models of the sort of conceptual change that occurs on a shorter timescale.

Theoretically speaking, there is something of an emerging consensus that we should acknowledge; we should recognize that some claims in the literature constitute theorizing about elements, whereas others constitute theorizing about systems of elements. (I like to use the terms "element" and "ensemble.") We should also recognize, and be very clear, that sometimes we are talking about entities constructed, by learner, in the moment. (I like the term "dynamic mental construct" or "DMC" as a name for these in-the-moment constructed mental entities.) It is not clear how far all of this very general language will get us. But we should settle whatever issues we can.

What can we say about why conceptual change is difficult? We saw that different researchers have given quite different answers to this question. As an initial step I think we should assume that the answer to this question will differ across domain and context. In some cases, conceptual change will be hard because it requires many small but coordinated changes. In other case, conceptual change might be had because there is some inherent coherence and resilience of existing knowledge. Yet another answer is one proposed by Carey, that conceptual change is hard because a significant quantity of new knowledge must accumulate. A last possibility is the one of which I'm most skeptical, that learners might be lacking a general and fundamental conceptual resource, such as an ontological category.

Thus, in sum, we can make progress toward more consensus in research on conceptual change if we:

- Recognize that our main problems are theoretical, not empirical.
- Recognize that we are, in some cases, working on quite different problems.
- Adopt some minimal consensus language (e.g., elements and ensembles, dynamic mental constructs)
- Assume that conceptual change will be difficult for a variety of reasons

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