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Becoming a Cognitive Scientist

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Abstract

My work in cognitive science has given me immeasurable pleasure for over 60 years. Here I trace how I have come to my current understanding of conceptual development. I emphasize the roles of accident and luck along my path as well as the importance of being able to deal with failures. I also place my career in context of the rest of my life.

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BEING OF ONE'S TIME—EARLY MENTORS

As a young child I observed my mother and my stepmother, who were both intelligent and capable stay-at-home moms, and saw unhappiness—even bitterness. In contrast my father seemed happy. He was a research engineer who studied how to optimize the structure of roads relative to their environment. I made the unwarranted induction that my father's going off to work every day explained this difference, and I wanted to be like my father. I thought perhaps it was necessary to be a scientist to be happy, and so I had a vague desire to become a scientist. I had no idea there might be gender-related barriers to this plan.

The Cold War was at its height when I was in junior high school in Washington, DC, where my father led the National Academy of Science's Highway Research Board. Instilling fear of the Soviet Union was a national propaganda goal, and we were trained to huddle under our desks with our arms around our heads in case of an atom bomb attack. When I was 12, our house in DC had a terrible fire. The adults were driving a guest home, and when I woke up in extreme heat and nobody answered my cries, I "knew" there had been an atom bomb attack and that everybody else in the house and the city was already dead, so I laid down to die. I thought perhaps I should pray, so I began "Dear God. . .if there is a God. . ." and passed out. Firefighters found me before I actually died of smoke inhalation and before the floor to my room burned through.

The Cold War indirectly had an impact on my intellectual path. When I was in high school in Ottawa, Illinois, the site of one my father's road tests, Russia launched Sputnik. The US government responded by declaring a crisis in science education and established university-based summer programs for high school students. My biology teacher, Mr. Alikonis, who had introduced me to bug and leaf collecting, bird watching, and molecular biology, sent me off to three of these. The first explored how the very recently discovered structure of DNA could shed light on the mechanisms underlying Darwinian natural selection. The second two, one on chemistry and one on group theory, consolidated my interests in science and math. But I did not yet know exactly what I wanted to study.

COLLEGE—EARLY MENTORS

In college and the 3 years after, I began a random-walk exploration to find what I was really interested in. I began with anthropology, taking advantage of a Harvard program that gave undergraduates experience in the field working with the Tzotzil, a Mayan people in Chiapas. Although a mind-opening experience, I learned an important lesson—you have to find work where you enjoy its day-to-day existence. Fieldwork requires an emotional adventuresomeness I lack. Casting away the grounding in one's own culture, even temporarily, to enable rich ethnography of another culture was not for me. I am not a good enough writer to be an ethnographer, and I struggle to learn new languages. It was not the right day-to-day fit. So when I was a sophomore, inspired by Mr. Alikonis, I decided I would concentrate in biology.

At that time at Harvard, sophomores had a year-long, one-on-one tutorial in their concentration. My tutor, a postdoctoral fellow, was working on the bodily mechanisms underlying biological clocks. While I enjoyed participating in studies that narrowed down the hypothesis space of possible metabolic or neural timing mechanisms, my tutor saw I was really interested in how we knew animals could tell time. The animals whose clocks we were studying were box-elder bugs (known to me from Illinois). My tutor showed me how we knew that box-elder bugs navigate by the sun, pointing out that one can't use the sun to set direction unless one knows what time it is.

My tutor guided me through the ethological literature on domain-specific adaptations underlying omnivores' learning what to eat, songbirds' learning of their song, imprinting, animal navigation, the dance of the bee, and so on—altogether, a glorious literature. A great mentor, he saw what *I* was interested in and led me through the science relevant to those interests. Years later, when I had enough experience to begin reflecting on mentoring, I realized that I should first encourage and help students articulate *their* interests, seeking at least a sliver of overlap with mine. Some students come to graduate school with developed interests, but it is not uncommon for a whole semester to go by before new students settle on the topic they will begin their research program on and for the bulk of the next semester to pass before a first study is planned.

At the end of the tutorial my tutor predicted that the deepest work on the issues that interested me would come from the newly emerging field of cognitive studies. He urged me to check out George Miller's and Jerome Bruner's introductory course. I can think of no single piece of advice that had a larger effect on my intellectual path. I took Miller and Bruner's Social Sciences 8 the fall of my junior year. Thirty-five years later, I tried to track down my tutor to thank him and to tell him what I had done with his advice, but I didn't remember his name and the biology department had no record of postdocs from 1961 to 1962. I also regret never having thanked Mr. Alikonis for his inspiration and guidance. Lessons: Listen to the advice of proven mentors and acknowledge inspiring teachers before you forget their names or they die.

Miller and Bruner started Social Sciences 8 with the historical nativist–empiricist debate, with readings from Berkeley, Descartes, Locke, and other philosophers. They then turned to the science of these issues from the late nineteenth century to the mid-twentieth century. To this day, I teach cognitive development the same way—although the empirical work we can appeal to today is incomparably richer than it was in the early 1960s. The mid-century giant in the field of cognitive development was Jean Piaget. Bärbel Inhelder, Piaget's long-time collaborator, gave a guest lecture in Social Sciences 8. A 5-year-old carefully made a "fair" division of juice between Inhelder's and his identical glasses. Inhelder then emptied her glass into a differently shaped glass, resulting in a much taller column of juice. As the child watched, he exclaimed "oooh," and when Inhelder asked, "Is it still fair? Do you have more to drink, do I have more to drink, or do we have the same amount of juice?" he answered that she had *way* more to drink.

I watched this entranced, thinking the child just didn't understand the question (I was right about that). I thought that if I had 10 minutes with that kid, I could clarify it and he'd demonstrate that he knew that you can't change the amount of juice in a glass by pouring it into a glass of another shape (I was both partly right and deeply wrong about that). Although my PhD dissertation, finished 10 years later, actually tested these ideas, I did not seriously start on my project of understanding structural changes in developing conceptual systems for another 7 years; the random walk was not over.

In the summer between my junior and senior years at Harvard I worked as a research assistant in the Center for Cognitive Studies, assisting Peter Wason, a visiting faculty member from University College London. Wason began modern work on mental logic and is known today for the Wason selection task. Wason's work introduced me to the first tool from the newly emerging discipline of cognitive psychology: mental chronometry. Inspired by Wason, my undergraduate thesis was a reaction-time study of the processing cost of negation.

Research teaching has come a long, long, way since the early 1960s. My official advisors on both my undergraduate thesis and my PhD thesis knew almost nothing about what I was doing and did not read my theses until after they were finished and submitted for the degree. PhD theses were often largely unmentored. They could be, and often were, rejected at the defense for fatal flaws that an advisor today would always catch.

BEING OF ONE'S TIME—CONTINUING THE RANDOM WALK

I came to adulthood in the 1960s. During college I was active in the civil rights movement and in the predecessor to the anti–Vietnam War movement. Upon finishing my BA, I decided that work in cognitive studies was too distant from the pressing social issues of the day and decided not to pursue a PhD. Still seeking adventure, I went to Tanzania as part of a group of nine Harvard students to work in a high school for refugees who had fled countries still not independent by 1964: Mozambique, South Africa, what would become Zimbabwe, and what would become Namibia.

At a social gathering for people connected to the school in Dar es Salaam, I noticed a light-skinned American Black man standing by himself and went to talk to him. A friend noticed and joined the conversation. I introduced her, "This is Matilde Zimmerman; my name is Susan Carey." He said, "Have you ever heard of Malcolm X?" I said, "Sure, what's your name?" He actually blushed.

Needless to say, all of the Harvard cohort gathered around him for the rest of the evening. He asked if we were free to talk further (of course we were), spent the day at our house in the outskirts of Dar es Salaam, and returned one more time before he left Tanzania. Near the end of our time with him, we all said how much it had meant to us to talk to him, and one of us asked whether he would have sought us out if he had met us in the United States. He said, "Well, I'd be much busier in the US, rather than at loose ends here. I'd tell you to look me up when you get back, but I'll be dead before then." We were shocked and accused him of (uncharacteristic) hyperbole. He assured us he was serious and explained that there was a hit ordered against him. He said he had been exploring whether there was something for him in the fight for independence in Africa, because he'd be safe there, but that his fight was in the United States, so he was going back home. He was assassinated shortly after returning to the United States.

I became fascinated by Africa through the eyes of my students and of the opposition party leaders in Dar. Each country they came from was so different from the others and from Tanzania, and I was ashamed that I had learned nothing of African history, either colonial or precolonial, before I went off to Tanzania. A Fulbright Scholarship allowed me to study African history at the School of Oriental and African Studies (SOAS) in London, where I began an MA program in African history in September 1965.

At SOAS, I indeed learned about African history. Excitingly, it had recently been shown that myths of origin, which were before then construed as aspects of religion alone, provided historical data. Collating across different linguistic families and across comments in the logs of Portuguese traders sailing along the East Coast of Africa, one could reconstruct aspects of precolonial African history. I decided to do work of this kind for my MA, capitalizing on my acquaintance with Mozambique. I read the relevant ship records in the British Museum's large collection, which were mind-numbingly dull. I was all set to go to Lisbon, where there was an even larger trove of these records in some warehouse in the suburbs. Before leaving, I realized something: There was nothing I would less like to do. The fruits of this research were exciting; all wedges into new knowledge are. But the day-to-day doing of that research, where I knew not a soul, spending my time alone in a warehouse rather than at the blue leather circular desk in the reading room on the top floor of the British Museum, culminating in fieldwork in Mozambique, where I knew neither the colonial language, Portuguese, nor the relevant African language, was not for me.

When I first arrived in London, I got in touch with Peter Wason. He invited me to attend his lab meetings. I gratefully agreed, because I so admired his work but also because I knew no Londoners except him and knew there would be interesting people involved. (This really was a random walk.) I *loved* the discussions, group efforts to understand data and argue about their theoretical upshot. The contrast between the collaborative nature of science and the solitary nature of history was vivid, and the day-to-day drudge work of science (painstakingly collecting and poring over data) is work I enjoy. Most importantly, the issues of cognitive psychology still grabbed me as much as they had when I was an undergraduate. I finally internalized the lesson of finding the right day-to-day fit. In fall 1967, I began graduate work at Harvard. Wason's work and my undergraduate thesis concerned cognitive psychology, logic, and language, and I planned to work with George Miller or Roger Shepard at Harvard. Miller left for Rockefeller between when I applied and when I began, and Shepard left for Stanford after my first year in graduate school. I had met Ned Block, my life partner, in the spring before I started my PhD studies and wasn't about to try to follow Miller or Shepard to an institution in another city.

Harvard had hired a wonderful mathematical psychologist, Lloyd Marlow, to fill the gap in cognitive psychology. I signed on to work with him. I read Marlow's thesis on mathematical models of perceived pattern complexity with interest. In my first meeting with him, he explained how he was generalizing this research to a theory of all perception. My takeaway was that I had a lot to learn about the science of perception (true; another glorious literature). In the next meeting, it was a theory of all cognition. In the third meeting, the next semester, he locked the door behind me when I came in, had clearly been sleeping in his office, and had a blackboard of equations that expressed his theory of the entire scientific universe. He expounded his ideas, which I finally recognized as gibberish, for 4 solid hours. I was afraid to try to leave and struggled to feign interest. At 7:30 pm, I persuaded him that he should tell Richard Herrnstein (the chair, for whom I was a teaching assistant that semester) about this work, and Marlow agreed to let me call him. Herrnstein lived in Wellesley and, thankfully, answered the phone. I told him that I thought he should come over *right now* to learn about Marlow's mathematical theory that unifies physics, astronomy, chemistry, biology, and sociology. Then I held my breath. He said, "I'll be there in half an hour," and half an hour later he knocked on the door, which Marlow unlocked. I fled. Herrnstein had him admitted to the mental health clinic of the Harvard Health Services, and that evening Marlow walked out and committed suicide by throwing himself under a subway train. This traumatizing experience convinced me to work with somebody I knew and felt comfortable with. The random walk was not over.

This left three wonderful cognitive scientists as potential advisors, all developmentalists: Jerome Bruner, Tom Bower, and Roger Brown. Brown was to become an important mentor; I attended his graduate seminar every year, and he was on my dissertation committee, but I was not interested in doing developmental psycholinguistics myself. Bower was obviously brilliant, but I had a "stay-away-from-him" instinct that turned out to be well-founded as he was later convicted of falsifying data, including while at Harvard. So I decided to return to my interest in conservation phenomena and asked Bruner to be my advisor. Bruner gave me free rein, lavishly supporting my research, but was not a collaborator. We never discussed my experiments, even after he read my thesis.

This time around, I actually read Piaget, namely his and Inhelder's *The Child's Construction of Quantities* (Piaget & Inhelder 1974). Each chapter documented age changes on tasks probing concepts of the weight, volume, and density of portions of matter, and each chapter then had two theoretical interpretations of the observed developmental changes. The first placed them within a developing theory of matter. The second interpretation explained the changes in children's theory of matter in terms of putative domain-general logical development from preoperational thought, to concrete operational thought, to formal operational thought. I saw that the first theoretical framework was sufficient for understanding all of the phenomena. After all, there are qualitative theory changes in the history of science, and nobody believes that Lavoisier exhibited more advanced logical capacities than did Priestley, let alone Galileo or Newton, who were great mathematicians and preceded both Priestley and Lavoisier by centuries.

Piaget scholarship in the United States at the time concerned his stage theory alone. I wanted, instead, to follow up on the *first* theoretical interpretation—that nonconservers had a different theory of matter from that of conservers—and explore whether their theory was responsive to counterevidence (testing by drinking). My unmentored dissertation, Are Children Little Scientists with False Theories of the World?, was a paradigm of juvenilia. It consisted of a single, never published (and unpublishable) experiment with around 50 5.5-to-6.5-year-olds, all nonconservers. These were the days before human subjects committees; the children all chose Coke as the liquid they would be drinking. I probed how they thought one could increase a given quantity of Coke. First, I simply asked them. Their answer was by adding more Coke and only by adding more Coke. They never suggested pouring the Coke into a different shaped glass, even when asked if they could think of a way to use such a glass to achieve the desired effect. I also probed this belief by performing magic tricks, looking for surprise reactions. I poured colored water from one glass to another such that a pint became one-quarter cup, even sometimes keeping the level constant in the new container as a pitcher of water was poured in. No reactions, just watching. However, when a given quantity started going up and down within a glass spontaneously, they all asked, "HOW IS THAT HAPPENING!?" They truly held no beliefs about what happens when one pours one quantity of liquid from glass to glass. At this point, I didn't distinguish implicit from explicit beliefs or beliefs from theories.

I taught the children what I meant by "more to drink" by having them tell which quantity was more just by drinking. Their answer was the one with more feels like more to drink, is harder to drink, and takes longer to drink. However, being nonconservers, they agreed that once their Coke had been poured into a thinner glass, it was more to drink than before. I then gave them a chance to test this hypothesis. They made two identical quantities in identical glasses and made one quantity three times more by adding more Coke, and I then poured the lesser quantity into a very thin glass. They judged the thinner glass as having more to drink than the other, and I reminded them what they had said about telling which had more by drinking and asked them to see if they were right by drinking. Most did not notice that the drinking provided evidence against the judgment they had just made, either affirming they had been right or simply changing their judgment with no comment. Finally, about 20% of the sample changed from being nonconservers to being stable conservers, and those were the children who reacted to the negative evidence upon drinking by saying, "Hey, what happened? Why was that one more? Let's do that again." They often drank so much Coke trying this again and again they had to go the bathroom. All of these children had epiphanies—articulating them beautifully: "Oh, I see, you added more to this one and you only poured this one into a different shaped glass. It only looks like more because that glass is so thin."

Finally, the only thing I could find in the data that predicted which children learned from the drinking experience was whether they had spontaneously distinguished the amount of juice from the appearance of the juice in the glass when describing two glasses of juice to a "blind person" in the next room.

I hope you are laughing at all the components of this one study with 50 children. Nonetheless, the data contained many hints about the ways children are like scientists. Scientists also ignore evidence they can't explain and fail to differentiate closely related physical variables. It also convinced me that the heart of nonconservation phenomena was children's concept of "amount of matter" (Piaget's and Inhelder's first theoretical summary), not their beliefs about how one changes amounts, or what happens to appearances when liquids are poured from one glass to another, or limits to their logical capacities (aspects of their second summary). While we are better at mentoring graduate students today, I bemoan the loss of juvenilia. My own set the stage for research I have been pursuing since that time, and amazingly, it (plus affirmative action) got me a job at MIT. Publications were not expected from graduate students at that time. Now students must have several publications even to become postdocs, and most students need serious mentoring to write three or more publishable papers as graduate students.

Lest you think I am exhibiting false modesty about my thesis, I did present it at one conference organized by Bruner. After my presentation, Tom Bever and Jacques Mehler, who had just started the journal *Cognition*, took me out to lunch. They said that I most probably had a career ahead of me, but I *must* abandon this work. They advised me that pursuing it would lead to certain failure as a scientist. I was wounded, realizing I had humiliated myself at the conference, but I didn't even consider following their advice. I had found the issues I really wanted to understand. Lesson: Be selective of the advice you follow, even from highly respected people who mean well.

MIT—MORE MENTORING

As a young professor at MIT, I began a new case study of the construction of an intuitive vitalist biology and continued to work on conceptual changes within intuitive theories of the material world. Soon after I was hired, another amazing mentor, Hans-Lukas Teuber, told me that the department thought my work on conceptual change was important but that it was a lifetime's work. He warned me that people were not going to understand what I was up to and it would be years until I could work out what the issues are well enough to begin to get a scientific purchase on them. He suggested that prior to tenure I should concentrate on writing journal articles that would be recognized as psychology. He suggested concentrating on my studies of word learning and also taking on a case study of something even farther from theory change: the development of face recognition. He said that my understanding of cognitive development would be deepened by studying very different aspects of the mind, and he even had a suggestion of where to start regarding face recognition.

I took his advice. He was right, both about how much work it would take before I understood theory change well enough to communicate with my peers and also about learning a lot from focusing on face recognition and word learning. The first four or five submissions to conferences of my work on intuitive biology were rejected, and because of my writings claiming there was no such thing as preoperational and concrete operational thought, both John Flavell and Harry Beilin wrote negative tenure reviews for me, as they each told me much later, both adding "I was wrong." Because of Teuber, my career was not in danger. Lesson (again): seriously consider the advice of trusted mentors.

I teamed up with Rhea Diamond, a research associate in the department, to study the development of face perception. We started where Teuber suggested, resulting in a *Science* article titled "From Piecemeal to Configurational Representations of Faces" (Carey & Diamond 1977). Diamond and I worked together for many years through three grants, unpacking different senses of "configurational representations." For example, one sense is an orientation-specific holistic representation of the first-order configuration of a face that survives normalization and averaging, with the result recognizable as a face. We showed that even preschoolers have configural representations of faces in that sense; subsequent work showed that the schema for a face is innate. In another sense, the second-order relational features that distinguish one face from another, the mental representations of configural features of faces undergo massive development well into adolescence.

Because of my work on word learning, George Miller asked me to come down to Rockefeller one day a week to consult on his new lexical development lab. He and Phil Johnson-Laird had just published *Language and Perception* (1976), in which they had analyzed the perceptual primitives that underlie lexical meanings in many domains. His vague idea, shared with other people working on lexical development at the time, was that the construction of lexical meanings in terms of these innately given primitives should leave a trace in the misanalyses children make as they are testing hypotheses about what words mean. In an era of unimaginable funding by today's standards, he had grant support for a small nursery school, with children wearing vests with microphones in them and four cameras in the corners recording all adult and child language during the day, plus funding for consultants, graduate students, and postdocs. His proposal, a true fishing expedition, would never be funded today.

At Rockefeller, Elsa Bartlett and I discovered the phenomenon of "fast mapping." We had a teacher introduce a new color word in a naturalistic context to each child in the classroom. Several weeks later (as many as six), a stranger tested the children in a laboratory, revealing that a lexical entry had been formed, retained, and mapped to a partial meaning that got the superordinate domain and part of the relevant distinguishing content right. This contrasted in a major way with the process of learning the meanings of "alive" or "heavy" (let alone "seven"; see below). I needed to understand why some concept or word learning is so easy and some so hard and am grateful to Miller for the opportunity to do this work, as well as for long discussions about the lexicon. I convinced him that his "component by component" construction model was a terrible way to think about lexical development (see Carey 1982). Rather, definitional primitives can also be learned (see Carey 2015). In *Spontaneous Apprentices* (Miller 1977), he credited me with influencing him to shut down his brief foray into studying language acquisition.

I have continued work on word learning. My interest is the processes underlying the emergence of the concepts that get mapped onto lexical entries. I abandoned my work on face recognition, but only because I saw that progress would require retooling in cognitive neuroscience and computational modeling. I wanted to concentrate on my central concerns: the nature of concepts and mental representations, innate representational content, intuitive theories, theory change, and constructivism as conceptual change. Tenure is a wonderful thing.

While Teuber was a wonderful *career* mentor, Jerry Fodor, in contrast, was one of the most important *intellectual* mentors in my life. Fodor was a philosopher in Teuber's "psychology department." In my early days at MIT, I would join him, along with graduate students, postdocs, and other faculty, for lunch and a beer, to argue about foundational issues in cognitive science, especially about the representational, computational theory of mind. The details of Fodor's views on the nature of concepts changed greatly over his long career, but throughout all of it, he maintained a deep commitment to the impossibility of learning new concepts (with emphasis on both *learning* and *new*). His views concerning the nature of concepts and the nature of learning led him to his argument that all concepts that underlie monomorphemic lexical items (around 500,000 of them in English) must be innate: that includes CARBURETOR, QUARK, GENE, ELEMENT... This is, of course, an absurd claim, but the problem is in stating what is wrong with his argument for it. See Carey (2015) for an explication of Fodor's argument and *my* last word in this career-long debate.

These debates with Fodor convinced me that whether the possibility of conceptual change can be ruled out a priori very much depends upon one's view of what concepts are. The philosophical literature of the day (especially that of Putnam and Kripke) suggested that the theories of concepts in the psychological literature (e.g., the classical view, prototype theories) were on the wrong track. Block (1986) argued for a "dual factor" theory of concepts, with meaning determined both by causal connections between mental symbols and the entities they refer to (wide content) and by conceptual role, relations among the symbols themselves (narrow content). I believe he is right; Fodor denied that conceptual role contributes to meaning. Over my career, I came to understand that word learning is easy when it is supported by innate input analyzers and innate conceptual role. Word learning is hard when the concepts and their conceptual role are co-constructed, as happens, for example, when a student takes a first physics course and acquires the Newtonian concept of force.

These debates also led to my formulation of the parts of a theory of conceptual development that I was working toward. The first is a characterization of the adult conceptual repertoire (the target the theory must explain, a specification of the nature of adult concepts). The second is a characterization of the nature of innate representations, those that arise through developmental processes that are not learning. The third is a characterization of the differences between the innate repertoire and the adult state. The fourth is a characterization of the mechanisms—learning or maturational—that underlie the transition from neonate to adults. That these are the components of a theory of conceptual development is a matter of logic. It is not meant to be controversial and should be shared between historical nativists and empiricists. The controversies concern how rich the innate repertoire is, the existence of innate learning mechanisms tailored to particular conceptual content, whether there are qualitative differences between the initial and final states, and the nature of the learning mechanism(s) that underlie conceptual development. Over my whole career, I have chipped away at these controversies. I began with the last, the question of whether conceptual development sometimes involves qualitative change.

QUALITATIVE CHANGES OVER DEVELOPMENT—CONCEPTUAL CHANGE

Conceptual Change in Childhood (Carey 1985) provided evidence that the Piagetian phenomenon of childhood animism, young children's highly reliable judgments that the sun, cars, lamps, and sometimes even tables are alive derives from the absence of an intuitive vitalist biology that is constructed over the years of 5–10 or so. I argued that the construction of vitalist biology requires conceptual change. Absent this theory, which has the concept ALIVE as the central theoretical construct, children lack the biological concepts ALIVE and DEAD . From the point of view of a vitalist theory, the meanings young children assign to the words "alive" (REAL-EXISTS-ACTIVE-AGENT) and "dead" (ASLEEP-INACTIVE-NONEXISTENT-ABSENT) are undifferentiated, incoherent, and self-contradictory. The child fails to distinguish the concepts of nonliving as applied to artifacts and people. The existence of undifferentiated concepts in this sense, and the process of differentiating among them in the course of theory change, provides a counterexample to Fodorian claims of the impossibility of conceptual construction. Differentiation, in this sense, is one answer to how individual concepts change in the course of theory constructions that involve incommensurability.

COLLABORATORS, INCLUDING STUDENTS

Fodor was a colleague who was an important intellectual mentor, although we never collaborated. But I owe an overwhelming debt to the colleagues I *did* collaborate with, such as Bartlett and Diamond mentioned above. All of the progress I have ever made in understanding conceptual development has been hammered out in discussions, often heated arguments, with graduate student collaborators, postdoc collaborators, and faculty and research associate collaborators.

I have returned to the case study of vitalist biology two separate times since the work of the 1970s and early 1980s. Susan Johnson and I collaborated on a study of the biological concepts that have been attained by adults with Williams Syndrome (Johnson & Carey 1998). Williams Syndrome is a rare genetic developmental disability resulting in low IQ scores and low scores on measures of executive function (EF) in the face of relatively preserved language skills and social and pragmatic competence. Our first participant (call her Judy) in the study was 21 years old. Unusually, she knew how to read and commented she was going to use part of her payment on the latest Anne Rice vampire novel. Johnson asked her, "What's a vampire?" Judy said (a direct quote), "A vampire is a man who climbs into ladies' bedrooms in the middle of the night and sinks his teeth into their necks." Susan asked, "Why do they do that?" Judy thought for a long time and replied (another direct quote), "Vampires must have an inordinate fondness for necks." Note the appropriate use of complex language, and also note that although Judy had read several vampire books, she did not really know what a vampire was!

When we took Judy through our battery of tasks that probe for the concepts within vitalist biology, she performed identically to normally developing 4-year-old controls, whereas on measures of vocabulary such as the Peabody Picture Vocabulary Test (PPVT) and on measures of factual knowledge about animals, she performed at the level of normally developing 13-year-olds. Exactly this pattern of results was found for each of the 10 Williams Syndrome adults we tested, ranging in age from 21 to 53. None had begun to construct the vitalist biology that children construct in the years of 5–10 or 12, despite having acquired factual knowledge comparable to the PPVT-matched control group, whose ages were all over 12 and all of whom had constructed a vitalist biology.

We offered this case study in support of the distinction between conceptual change and knowledge enrichment, and I stand by that argument. But this study leaves open exactly what impairment prevented conceptual change in this domain. It also provided no insight into the learning mechanisms that achieve conceptual change. Some 25 years later, I teamed up with Deborah Zaitchik to address the first of these questions. We began by replicating her finding that animistic errors on the Piagetian interview reemerge in the elderly. We showed that this was *not* due to the loss of the vitalist theory that our participants had constructed early in their school years. We found that measures of EF explained the effects of aging on judgments that the sun, wind, cars, or rivers are alive (Tardiff et al. 2017).

A series of studies found that measures of specific EFs, set-shifting and inhibition, predicted the progress children had made on the construction of vitalist biology, controlling for measures of fluid IQ, working memory (the EF most associated with measures of fluid IQ), accumulated factual knowledge, age, and receptive vocabulary. This confirms, at least, that EFs play a role in the *deployment* of a vitalist biology. Joined by Igor Bascandziev, we completed the work Susan Johnson had started. A training study showed that measures of set-shifting and inhibition predicted progress made from a bootstrapping curriculum that inched children along in the construction of vitalist biology. In contrast, PPVT predicted variance within the massive progress on fast-mapped, generic "fun facts," such as the fact that crickets' ears are on their legs and that the color of octopus blood is blue (Bascandziev et al. 2018). Learning the latter merely involves forming new beliefs stated in terms of available concepts. Conceptual construction and knowledge enrichment are indeed dissociable processes. EFs are cognitive control processes that undergo developmental changes throughout childhood and play a crucial role in conceptual construction itself. Piaget was right: There are domain-general changes over development that allow learning not achievable without them (as observed in the Williams Syndrome adults).

KUHN, WISER, AND SMITH

In the 1970s, in parallel with the case study on intuitive biology, I also continued my work on the acquisition in childhood of a framework theory of matter, developing two of the most important extended collaborations in my career, those with Marianne Wiser and Carol Smith. We showed that young children did not fail to distinguish the concepts WEIGHT and SIZE (as Inhelder and Piaget had claimed); rather, the important lack of differentiation was WEIGHT from DENSITY. Our challenge was to specify what, in representational terms, an undifferentiated concept WEIGHT-DENSITY could be like. Weight is an extensive variable, and density is an intensive one. In the adult intuitive theory they are interdefined; the density of a given portion of matter is weight divided by volume. An undifferentiated concept that conflates the two is incoherent from the point of view of the adult theory.

By this time I was reading the history of science seriously, and I came upon a one-line comment in a paper by Tom Kuhn that before the eighteenth century physicist Joseph Black, physics had not differentiated HEAT from TEMPERATURE. Heat is an extensive quantity and temperature an intensive one, and in every theory since Black, they are interdefined. An undifferentiated concept HEAT-TEMPERATURE is incoherent from the point of view of any theory since Black.

I asked Kuhn both how he knew that scientists before Black failed to differentiate the concept HEAT from the concept TEMPERATURE and how an undifferentiated concept that conflated the two could function in thought. He answered that he did not know; he was merely citing a claim made by other historians of science and, to his knowledge, nobody had seriously tackled these questions. He said I'd have to do the history of science myself and that the answer lay in the treatise *Experiments on the Thermometer and the Barometer*, published by the Florentine Academy in the seventeenth century. This book reported the first systematic research on thermal phenomena using the thermometer, a device invented by Galileo. Luckily the scientists in Boyle's Royal Society had made a literal word by word translation into English that I could work from.

Marianne Wiser, a graduate student in physics who had recently switched into cognitive science out of an interest in the psychology of theory construction, convinced me to accept Kuhn's challenge and agreed to lead the project (Wiser & Carey 1983). Reading the treatise together dozens of times (Wiser in Italian as well as English), we saw that the Experimenters (their own self-designation) were studying the physical consequences (expansion, contraction, freezing, melting, boiling) of adding heat or cold to other substances (they conceived of heat and cold as two different substances). At the beginning, we both took "degree of heat" and "degree of cold" to mean temperature, a property of the substance under discussion when the Experimenters were describing what happened when heat or cold was added to it. These terms were often connected to readings of the thermometer, but the tables presented in the text made no sense.

On the thirtieth reading or so, Wiser noticed that the thermometer was not actually in the substance whose changes were being described. The Experimenters were measuring the strength of the heat or the cold source. Furthermore, the actual number of degrees were meaningless—there were 400° thermometers and 100° thermometers, each calibrated, within-type, to each other, but none calibrated to any fixed point such as the freezing point or boiling point of water at sea level. The Experimenters often quantified relative strengths or degrees of the heat or cold by noting the rate of change in the thermometer. They did not have a measure of temperature and were centuries away from a measure of heat such as the calorie.

As Kuhn insisted (e.g., Kuhn 1982), the history of science (and I would add developmental cognitive science as well) is possible because we can learn the language of our predecessors. Importantly, doing so is *not* a matter of translating the terms of that language into terms of our own. We must simultaneously reconstruct the explanatory theory being deployed and, in doing so, construct the meanings of its theoretical terms of the conceptual role within that theory. Conceptual role is *partly* what provides their meaning. And as summarized in Carey (2009), "undifferentiated" in the case of WEIGHT-DENSITY and in the case of HEAT-TEMPERATURE means exactly the same thing, and the same kind of evidence supports the existence of such undifferentiated concepts in childhood and in mature scientists.

It was through this work, on intuitive biology, intuitive theories of matter, and scientific theories of thermal phenomena, that we came to formulate what incommensurability (theory-wide changes such that one conceptual system cannot be translated into the terms of the another) and conceptual changes (changes at the level of individual concepts) are. Incommensurability is one type of conceptual discontinuity.

BEING OF ONE'S TIME–ON THE FACULTY

I never stopped being a child of the 1960s. It was always important to me to have at least one ongoing activity motivated by some social issue. What these activities were changed over my life. When I started at MIT, there were almost no women on the faculty. Indeed, I had never been taught by a woman, either as an undergraduate or a graduate student. Only in graduate school did I begin to wonder whether these facts might have consequences for my prospects of a career in academia. A memorable conversation with Herrnstein, in which he explained to me why he argued that women should not be invited to join the all-male secret society, the Psychological Round Table, was one of many experiences that led me to be active in the fledgling women's liberation movement. He first told me that this society is for the rising stars in experimental psychology, explaining that one is kicked out once one is 40. I thought he was gearing up to invite me to join. Women should not be admitted, he said, because the young guys are shy and there are lots of dirty jokes, including the gavel for the meeting being a dildo, and obviously having women in attendance would dampen the fun. I was never invited to join, but within 10 years, women I knew were, and, of course, women like dirty jokes too. If this society still exists, I hope dirty jokes don't continue to play such a central role in its social fabric.

Other experiences suggesting I might not belong in the academy included being taken out to lunch with all the other first-year graduate students my first week in graduate school and being told about the "woman problem"—women are accepted but then get married and leave the field. And why did getting married require women to leave the field but not men? Near the end of my time in graduate school, two students of Roger Brown, by then the faculty member I was closest to, were on the job market. One student was a female superstar who had done a brilliant and ambitious thesis, Melissa Bowerman. The other was male and a solid student. Melissa got no job interviews, and the man got two job offers. I stormed into Roger Brown's office and demanded to know how this was possible, and he said that the man had a family to support, and Melissa had a husband who would support her and their family. I exploded, and by the end of the conversation he said, "You're right. I will never fall prey to that line of thinking again."

Times were a-changin'. As another example of the luck of the moment, my PhD coincided with affirmative action for women. At MIT, there were almost no women undergraduates when I began teaching, and some faculty members were famous for displaying nude pictures of women. A regular part of the freshman orientation week was viewing a hardcore pornographic movie, such as Deep Throat or Behind the Green Door. Talk about creating a hostile environment for the few women students! Some of us young women who were recently hired tried to get MIT to change these policies, and we were met with free speech arguments. Partly in response, Ruth Perry formed a Women's Studies Program and recruited at least one faculty member in every relevant discipline to teach in it. I volunteered to teach Psychology of Gender, a course totally unrelated to anything I had ever studied. Mentored by Virginia Valian [see her later book *Why So Slow* (Valian 1998)], this is where I first came into contact with social psychology, namely, the wonderful research on implicit bias.

Another activity driven by social concerns was to try to bring our understanding of conceptual construction to the sorry state of science education in this country. After Sputnik, many eminent scientists had become involved in K–12 education, working with educators to develop elegant curricula to teach physics, chemistry, and biology. The news in the science education literature of the 1970s and 1980s was that these curricula left the intuitive theories of many students (ranging from junior high school to college students) totally unchanged.

Wiser, Smith, and I all took part-time appointments at the Harvard Graduate School of Education, continuing our collaborations in this context. We joined the ranks of many science educators, such as Rosalind Driver, Jim Minstrell, and John Clement, to argue that what had been missed in this earlier round of educational innovation was an appreciation that the problem of science education is, at least in part, one of conceptual change. To make progress on this problem, we must confront the learning mechanisms that underlie conceptual change and make sure that our curricula engage them.

Wiser and Smith each developed curricular interventions that vastly outperformed the standard curricula in engendering conceptual change within intuitive theories of matter and thermal phenomena. They documented the crucial role of conceptual modeling in episodes of theory construction, inspired by case studies in the history of science (e.g., Nersessian 1992 on Maxwell). They created manipulable computerized visualizations of the relations between extensive variables (number of dots, number of boxes) and an intensive variable (number of dots per box) and engaged learners in exploring the relations between dots per box, number of boxes, and the total number of dots in the model. They then used these visualizations to model the relations between weight and density in different materials, or heat and temperature in the context of thermal phenomena. Their work inspired my later formulation of Quinian bootstrapping as a mechanism underlying conceptual change (Carey 2009).

The problems in American science education have a huge political component. There are many large-scale interventions that are based on solid science that actually lead to conceptual change, sometimes dramatically. Bewilderingly, these are not picked up elsewhere and are usually abandoned in the course of local school administration changes. Over the subsequent years I made several forays into doing something about this. I joined statewide science standard committees and committees sponsored by consortia of professional societies and the National Academy of Sciences that aimed to influence public policy. I learned what I should have known anyway, that effecting political change requires dedication and expertise. I also learned that the ratio of results to effort is way too low for me to tolerate. Ineffectiveness makes me extremely anxious. Although I am in awe of colleagues on these committees who somehow knew what steps could be taken *now*, and how to chip away at the problems, trying to affect social policy was not the right fit for me.

TAKING NATIVISM SERIOUSLY—SPELKE, GELMAN, AND GALLISTEL

I met Elizabeth Spelke when she was a visitor at MIT in the early 1980s. She and Renee Baillargeon had just developed the violation-of-expectancy methodology that revolutionized the study of infant cognition. Contra Piaget and the British empiricists, Spelke and her collaborators provided evidence for innate representations of permanent, distal objects. Spelke argued that an intuitive system of representations, with OBJECT as the central concept, was innate. She further argued that this intuitive system of representation structures everyday thought throughout life. She did not deny the very possibility of conceptual change, but argued it occurred only in the course of metaconceptually aware theory building by scientists. We both agreed that there are rich innate representational primitives that go beyond sensory or sensory-motor content and that humans are capable of conceptual change. But whereas she thought conceptual change is extremely rare, I thought continuity throughout the life span is extremely rare. Some 20 years later, Spelke and I teamed up to head the Laboratory of Developmental Studies at Harvard, continuing these conversations to today.

Engaging with Spelke convinced me that it was time to switch my attention to the innate starting point of conceptual development. Fortuitously, Rochel Gelman invited me to join a group she had put together at the Center for Advanced Study in the Behavioral Sciences in Palo Alto. Randy Gallistel was part of the group and was just finishing his landmark book, *The Organization of Learning* (Gallistel 1990). Returning to the ethological literature some 20 years after my sophomore tutorial, we spent a year discussing innate domain-specific learning mechanisms, seeing that these provide decisive counterexamples to the classical empiricist position (Gallistel et al. 1991). For each, there are innate input analyzers that ensure a causal connection between mental representations and entities in the world (wide content) and innate conceptual roles for the representations so tokened (narrow content).

Take imprinting in chicks, for example; there are two separate innate mechanisms for identifying Mom, an innate schema of a bird configuration and, absent input that meets that schema, a mechanism for identifying bird sized objects that move by themselves. These innate perceptual devices output representations causally connected to the chick's mother (wide content) and specify who the chick should to stay close to and learn from (innate conceptual role). This domain-specific learning device involves tokening a concept that goes well beyond anything specifiable in terms of the conceptual combination of sensory primitives, a concept we can gloss as "Mom."

Inspired and encouraged by Spelke, I opened my own infant lab at MIT in the late 1980s.

Mentoring PhD Students and Postdocs

My mentoring philosophy reflects the excellent mentoring I was lucky to receive: First, ascertain what the *students'* interests are. But postgraduate mentoring involves much more, because it is a collaboration. All collaborations require that each collaborator have an independent theoretical interest in the hypotheses under test. I never agree to mentor any project unless I see that the hard work of contributing to the student's developing research program will help me work out details of my own theoretical perspective. To illustrate, I describe a trio of such projects by Karen Wynn, Fei Xu, and Lisa Feigenson. Each transformed my thinking. If space allowed, I'd provide 20 more examples.

The Case Study of Number—Wynn, Xu, and Feigenson

Karen Wynn asked whether I would supervise a dissertation on the acquisition of integer concepts. I (internally) panicked since I knew almost nothing about this huge and beautiful literature. But I agreed because I suspected (rightly) that the process of acquisition of mathematical concepts would be deeply similar to the process of acquisition of concepts within intuitive theories and that there would also be important differences between the two.

We began by reading a bit of the literature on the philosophy of mathematics and studying Gelman & Gallistel's (1978) book, which argued that the details of 2-year-olds' learning to count supported the existence of an innate count list of mental symbols they called "numerons." Most researchers of early numerical cognition were skeptical. At stake in the debates was the cardinality principle (CP), that the last word reached in a properly executed count represents the cardinality of the set. Seeking convergent evidence for Gelman & Gallistel's hypothesis, Wynn developed the Give-N task in which children are simply asked to hand over a number of objects to the experimenter (Wynn 1990, 1992). She discovered that in spite of being able to count to 10, almost all 2-year-olds, as well as many 3- and 4-year-olds, do not know the numerical meanings of many numerals in their count list. English-learning American children learn what "one" means as 2-year-olds. They can give one fish if asked, but if asked for any other number, grab a handful and hand them over. Such a child is called a "one"-knower. It takes 6 months or more to then learn what "two" means and more months to learn the meanings of "three" and "four." It is not until even later that children learn how counting represents number. The work that followed Wynn's seminal studies motivated the hypothesis that the count list is the first representational system in ontogenesis with the capacity to represent integer values above 4 (for reviews, see Carey 2009, Carey & Barner 2019).

That there is no innate count list does not mean there are no innate representations with numerical content. The analog number system (ANS) has a long evolutionary history and is attested in human newborns. The ANS has numerical content; it represents the cardinal values of sets of individuals, albeit only approximately, and uses these representations to compare which of two sets has more individuals and to carry out computations such as addition and division. The first projects in my new infant lab, led by Fei Xu and Lisa Feigenson, respectively, discovered that a second, quite different, representational system, which I called "parallel individuation" (PI), also has numerical content and is attested early in infancy.

Fei Xu had asked whether I would supervise a PhD on kind sortal concepts in infancy. I (inwardly) panicked but again agreed. We began by reading some of the philosophical literature on sortals and by studying Spelke's work on the concept OBJECT as a sortal (e.g., Spelke et al. 1995). Spelke had shown that the criteria for individuation and numerical identity for objects were spatiotemporal. Xu discovered that it was not until age 12 months that basic-level kind concepts, such as DUCK and BALL , functioned as sortals (see Xu 1997). For example, seeing a yellow duck emerge from and return to behind a screen followed by a red car doing so does not lead a 10-month-old to infer there are two objects behind the screen (Xu & Carey 1996).

Particularly important to me, Xu's work was the first to suggest that young infants' object representations were part of the same system of representation as the mid-level object files of object-based attention, where the criteria for individuation and numerical identity are also largely spatiotemporal. Spelke's object representations are indeed continuous through life; they are Treisman's object-files, which in turn underlie Pylyshyn's multiple object tracking. This is a perceptual system of representation that allows attention to multiple individuals at once.

The object-files that are the output of mid-level vision articulate capacity-limited visual working memory. By 1996, I had moved to NYU because both Ned and I wanted to live in New York City. Lisa Feigenson came to work with me on infants' representations of number. Her work strongly confirmed the identification of core cognition of objects with mid-level vision's object files. She showed that the capacity limitations in number of objects in the mental models of small sets were based on absolute set size, not the ratios between sets being discriminated (the signature of the ANS). For example, when choosing between two sets of crackers put into two different

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buckets, one at a time, 10-to-12-month-old infants crawl to the larger set when the choices are one versus two, or two versus three, but are at chance if the choices are two versus four or even one versus four. Also, when reaching into a box to retrieve objects seen hidden there, they reach persistently until they have retrieved all of one, two, or three objects seen hidden, but when four were hidden, they are satisfied if they have retrieved only one (Feigenson & Carey 2003, 2005).

This work convinced me that systems of core cognition are not "theories." They are perceptual systems that represent the here and now with innate input analyzers that ensure the symbols tokened within them refer to relevant entities in the world (wide content). The symbols so tokened have an innate conceptual role (narrow content). Perception can have much more abstract content than one might initially think. Spelke is right; perception-like systems of core cognition do function throughout life. However, there are no innate input analyzers nor any innate conceptual roles relevant to the theoretical terms in most later theories (including vitalist biology), which are explicit, verbalizable, systems of representation. Furthermore, explicit theories do undergo radical change, both over history and ontogenesis. The symbols' conceptual roles and the causal connections between symbols and entities in the world are co-constructed in most framework theories.

PUTTING IT ALL TOGETHER

The ontogenetic origins of integer representations became the central case study in my 2009 book, *The Origin of Concepts* (TOOC) (Carey 2009), which characterized the nature of adult concepts and the nature of the innate representational repertoire, provided evidence for discontinuities over ontogenesis, and described one learning mechanism that can achieve them—Quinian bootstrapping. Armed with detailed, empirically supported characterizations of the initial conceptual systems with numerical content, the ANS and PI [conceptual systems 1 (CS1s)], along with detailed characterizations of later conceptual systems that express numerical content, tally systems and counting (CS2s), we can show how the earlier systems have less expressive power than the later ones. The ANS and PI each lack the expressive power to represent integers. Indeed, neither can represent the exact cardinal value of any set above four or five. The count list, the ontogenetically earliest CS2, when deployed in accord with Gelman & Gallistel's (1978) counting principles, implements the successor function and thus is a representation of as many integers as are expressed in a CP-knower's current count list.

These analyses make sense of the evidence that learning how counting represents number or acquiring a tally system is very hard (witness the extended subset-knower phase and the extended development of tally systems and counting over cultural history). They also explain why adults in cultures who lack either a tally system or a count list cannot think thoughts involving exact cardinal values of sets greater than four (witness studies of numerical cognition in home signers and in Amazonian peoples such as the Pirahã, Munduruku, and Tsimane).

With respect to a learning mechanism that can underlie the transition from the ANS or PI (or both) to a count-list representation of number, TOOC spells out how Quinian bootstrapping works to co-construct conceptual role and causal connections between new concepts and the world (see Carey 2009). TOOC illustrates the role of Quinian bootstrapping in historical cases of conceptual change, in the construction of natural and rational numbers in childhood, and in the conceptual changes within an intuitive matter (my very first case study).

Quinian bootstrapping involves formulating a system of representation that captures the structural relations among the target concepts directly, where the terms in that system do not yet express those target concepts. This system is a placeholder structure; the initial meanings of the words within it are exhausted by conceptual role within it. In the case of number, the child's initial count list is the placeholder structure; its content is exhausted by the order in the list. In Wiser and

Smith's curricula the dots per box visual models are the placeholders in their respective bootstrapping episodes. In parallel, the learner constructs beliefs about the entities in the relevant domain, formulated in terms of the concepts in CS1, which in turn ground out in systems of core cognition that are causally connected to the world, as all perception is.

The bootstrapping processes then involve modeling the phenomena in the domain as represented in CS1. Modeling processes include analogy and limiting case analyses, as well as abductive and inductive inference. In the Wiser and Smith curricula, this process is initiated and guided by the teacher. In the case of number, I suggested that it is initiated by a suspicious coincidence: When counting a small collection of objects, a "three-" or "four"-knower recognizes that the word you reach in a count (up to "four") always expresses the number of objects in the set. The analogical modeling involves creating a mapping between two very different "follows" relations: next on the list and next in a series of sets related by +1. This is followed by an inductive generalization that for any set whose cardinal value is expressed by a given verbal numeral *N*, adding an element results in a set whose cardinal value is expressed by the next number in the list. Through this induction, which may unfold in several steps, the child ends up with a symbolic system capable of expressing integers.

AFTER *THE ORIGIN OF CONCEPTS*—THE MCDONNELL NETWORK

I worked on TOOC for over 10 years and, intellectually exhausted, thought I would start no new research projects. I really did not know myself. There was a nagging problem in my account of number. The concept INTEGER in mathematics does not ground out in the ANS or PI. There are many proofs that all of arithmetic can be built from the concept NATURAL NUMBER (POSITIVE INTEGER), which in turn can be built from one numerical primitive and the resources of propositional and quantificational logic. I wondered what we knew about the logical resources available to infants and nonhuman animals and whether these may include all that is needed to represent natural number.

The question of the logical resources in nonlinguistic or prelinguistic thought has been debated in philosophy at least since Descartes, with some philosophers arguing that logically structured thought emerges only with human language both in ontogenesis and evolution (e.g., Davidson) and others that a logically structured language of thought underlies the behavior of all animals at least as complex as insects (e.g., Fodor). This is not an a priori issue to be decided from the philosopher's armchair. It's a straightforward scientific question. In 2010 or so, a few people were beginning to explore it, such as Josep Call and colleagues in animal work and Luca Bonatti and Justin Halberda and colleagues in developmental studies.

Bonatti organized a conference in Barcelona, out of which grew a McDonnell Foundation Network grant for workshops that would seek *scientific* progress on the question of the ontogenetic origins of abstract combinatorial thought. Over 50 years after my research assistantship with Peter Wason, I returned full time to the study of the logical resources in thought. My own case studies (in collaborations with graduate students and postdocs, especially Roman Feiman, Shilpa Mody, Brian Leahy, Ivan Kroupin, Stephen Ferrigno, Jean-Remy Hochmann, Masoud Jasbi, and Michael Huemer) have concerned logical connectives (e.g., or, not, possible), the abstract relations same and different, and recursively structured representations.

The last McDonnell plenary workshop was in Venice in early July 2022, right before I will retire from Harvard in June of 2023. I am sad to close my lab. Working with graduate students and postdocs has been, by far, my favorite part of a life in academia. But my family home is in New York City, and I want to spend the years I have left full time with the man I love most in the city I love most. Closing my lab does not mean ending my intellectual journey. My first project will be to pull together a book on what we have learned about the ontogenesis of logically structured thought through the work of the McDonnell Network.

LUCK, CHANCE, AND COPING WITH FAILURE

I have not detailed my many failures along my journey, including rejected papers, conference submissions, and grants. Writing is thinking, and these failures often resulted from errors in thought and always from failures to make the argument clear. It is crucial to take each rejection as a challenge to go back to the drawing board to clarify the issues, both for yourself and your readers. Peer review is an amazing institution. I am lucky to be in a field with such generous colleagues who put immense time into helping others improve their writing.

I am acutely aware of the role sheer luck has played in my career. I had a stepmother who aspired to Radcliffe for me and was able to pay for my expensive undergraduate education. I have had generous mentors my whole life. I am lucky that affirmative action landed me a job in the first Brain and Cognitive Science department in the country, that universities were expanding, and that funding was plentiful during my early days in academia. I am extremely lucky to have had jobs in three distinguished departments with low teaching loads, institutions that attracted great graduate students and provided full support for them and their research. I have always partially self-funded my lab, putting summer salary from grants and monetary prizes I have won into the research, and for the past 10 years, plowing my Social Security checks into it as well. I am lucky that I could afford to do that.

I have been exploring the same issues for 60 years, issues I came upon by accident. I did not choose my sophomore tutor, nor Inhelder's guest lecture in Social Science 8. I played no role in Wason's happening to live in the same city as SOAS and inviting me to his lab meetings when I had turned my back on cognitive science. I certainly did not choose that my attempts to find a mentor who worked on adult cognitive science would fail, leaving only developmental cognitive science at Harvard at the time. I ended where I was meant to be, or perhaps, more accurately, by simply keeping at it, I ended where I am thrilled to be.

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LITERATURE CITED

Bascandziev I, Zaitchik D, Tardiff N, Carey S. 2018. The role of domain-general cognitive resources in children's construction of a vitalist theory of biology. *Cogn. Psychol.* 104:1–28

Block N. 1986. Advertisement for a semantics for psychology. *Midwest Stud. Philos.* 10:615–78

Carey S. 1982. Semantic development: the state of the art. In *Language Acquisition: The State of the Art*, ed. E Wanner, LR Gleitman, pp. 347–89. Cambridge, UK: Cambridge Univ. Press

Carey S. 1985. *Conceptual Change in Childhood.* Cambridge, MA: MIT Press

Carey S. 2009. *The Origin of Concepts*. Oxford, UK: Oxford Univ. Press

Carey S. 2015. Why theories of concepts should not ignore the problem of acquisition. In *The Conceptual Mind: New Directions in the Study of Concepts*, ed. E Margolis, S Laurence, pp. 415–54. Cambridge, MA: MIT Press

Carey S, Barner D. 2019. Ontogenenetic origins of human integer representations. *Trends Cogn. Sci.* 23:823–35
Carey S, Diamond R. 1977. From piecemeal to configurational representation of faces. *Science* 195:312–13
Feigenson L, Carey S. 2003. Tracking individuals via object files: evidence from infants' manual search. *Dev. Sci.* 6(5):568–84
Feigenson L, Carey S. 2005. On the limits of infants' quantification of small object arrays. *Cognition* 97(3):295–313
Gallistel CR. 1990. *The Organization of Learning*. Cambridge, MA: MIT Press
Gallistel CR, Brown A, Carey S, Gelman R, Keil F. 1991. Lessons from animal learning for the study of cognitive development. In *The Epigenesis of Mind: Essays in Biology and Cognition*, ed. S Carey, R Gelman, pp. 3–36. Hillsdale, NJ: Lawrence Erlbaum Assoc.
Gelman R, Gallistel CR. 1978. *The Child's Understanding of Number*. Cambridge, MA: Harvard Univ. Press
Johnson SC, Carey S. 1998. Knowledge enrichment and conceptual change in folkbiology: evidence from Williams syndrome. *Cogn. Psychol.* 37:156–200
Kuhn T. 1982. Commensurability, comparability, communicability. In *Proceedings of the Biennial Meeting of the Philosophy of Science Association*, Vol. 2*: Symposia and Invited Papers*, ed. PD Asquith, T Nickles, pp. 669–88. East Lansing, MI: Philos. Sci. Assoc.
Miller GA. 1977. *Spontaneous Apprentices: Children and Language*. New York: Seabury Press
Miller GA, Johnson-Laird P. 1976. *Language and Perception*. Cambridge, MA: Harvard Univ. Press
Nersessian N. 1992. How do scientists think? Capturing the dynamics of conceptual change in science. In *Cognitive Models of Science*, ed. R Giere, pp. 3–44. Minneapolis: Univ. Minn. Press
Piaget J, Inhelder B. 1974. *The Child's Construction of Quantities*. London: Routledge & Kegan Paul
Spelke ES, Kestenbaum R, Simons D, Wein D. 1995. Spatiotemporal continuity, smoothness of motion and object identity in infancy. *Br. J. Dev. Psychol.* 13:113–142
Tardiff N, Bascandziev I, Sandor K, Carey S, Zaitchik D. 2017. Some consequences of normal aging for generating conceptual explanations: a case study of vitalist biology, *Cogn. Psychol.* 95:145–63
Valian V. 1998. *Why So Slow?* Cambridge, MA: MIT Press
Wiser M, Carey S. 1983. When heat and temperature were one. In *Mental Models*, ed. D Gentner, A Stevens, pp. 267–97. Hillsdale, NJ: Lawrence Erlbaum Assoc.
Wynn K. 1990. Children's understanding of counting. *Cognition* 36:155–93
Wynn K. 1992. Children's acquisition of the number words and the counting system. *Cogn. Psychol.* 24:220–51
Xu F. 1997. From Lot's wife to a pillar of salt: evidence that physical object is a sortal concept. *Mind Lang.* 12(3–4):365–92
Xu F, Carey S. 1996. Infants' metaphysics: the case of numerical identity. *Cogn. Psychol.* 30(2):111–53