Dædalus

Journal of the American Academy of Arts & Sciences

Winter 2004

Inside front cover: Megan, a fourteen-year-old chimpanzee, uses a simple tool to reach for an apple. "Surely chimpanzees form concepts about concrete things. . . . But what about more abstract concepts? Concepts like ghosts, gravity, and God?" See Daniel John Povinelli on *Behind the ape's appearance: escaping anthropocentrism in the study of other minds,* pages 29 – 41. Photograph courtesy of the Cognitive Evolution Group, University of Louisiana at Lafayette.

Dædalus is designed by Alvin Eisenman

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The labyrinth designed by Daedalus for King Minos of Crete, on a silver tetradrachma from Cnossos, Crete, c. 350 – 300 B.C. (35 mm, Cabinet des Médailles, Bibliothèque National, Paris). "Such was the work, so intricate the place, / That scarce the workman all its turns cou'd trace; / And Daedalus was puzzled how to find / The secret ways of what himself design'd." – Ovid, *Metamorphoses,* Book 8

Dædalus was founded in 1955 and established as a quarterly in 1958. The journal's namesake was renowned in ancient Greece as an inventor, scientist, and unriddler of riddles. Its emblem, a maze seen from above, symbolizes the aspiration of its founders to "lift each of us above his cell in the labyrinth of learning in order that he may see the entire structure as if from above, where each separate part loses its comfortable separateness."

The American Academy of Arts & Sciences, like its journal, brings together distinguished individuals from every field of human endeavor. It was chartered in 1780 as a forum "to cultivate every art and science which may tend to advance the interest, honour, dignity, and happiness of a free, independent, and virtuous people." Now in its third century, the Academy, with its more than four thousand elected members, continues to provide intellectual leadership to meet the critical challenges facing our world.

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Howard Gardner

What we do & don't know about learning

Suppose that we were commissioned to create a museum of learning. I don't mean a stuffy, hands-off collection of old manuscripts or films, but rather a state-of-the-art exploratorium that displayed the full spectrum of learning types and, in vivid form, everything that is known about learning.[1] Suppose, further, that we had a budget for consultants and were able to hire the seven experts whose essays are collected here. Presumably we would have in mind a number of guiding questions, among them: What examples of learning should we include? How should we conceptualize this enterprise? What progress has taken place in our understanding of learning in the last century or so, and how can these revolutionary insights inform the education of future generations? Finally, what puzzles remain?

In all probability our museum's first displays would show humans learning: infants crawling, walking, talking; toddlers engaged in rough-and-tumble or imaginative play; youngsters (or oldsters) at school, learning their 3Rs and going on to master the disciplines and perhaps engage in interdisciplinary work. Casting our net more widely, we might exhibit a child learning to play a musical instrument, an apprentice working alongside a master builder, a medical student attending rounds, a recruit in the military, a physically injured person recovering the ability to walk, a victim of a stroke learning to talk or reason again.

Casting our net more widely still, to encompass the full range of learning among animals, our museum might include exhibits of sea slugs that learn to move in certain directions while avoiding others; fish that 'imprint' on certain physical forms and trail after these privileged forms throughout development;

Howard Gardner, John H. and Elisabeth A. Hobbs Professor in Cognition and Education at the Harvard Graduate School of Education, is best known in educational circles for his theory of multiple intelligences, a critique of the notion that there exists but a single human intelligence that can be assessed by standard psychometric instruments. His most recent books are "Changing Minds" (2004), "Making Good" (2004), and "Good Work" (with Mihaly Csikszentmihaly and William Damon, 2001). Gardner has been a Fellow of the American Academy since 1995.

1 This supposition is not idle. At Harvard Project Zero, a research group concerned with learning with which I have been affiliated since 1967, we are planning to construct such a museum – at first virtually, ultimately in bricks and mortar.

rats that learn their way around complex natural and man-made mazes; pigeons that can play ping-pong, trace missiles, and recognize human beings in photographs; chimpanzees that can use sticks to wipe off termites or to hide treasures from their fellow chimps, and that seem able to learn some language-like systems.

We will also need to create displays that show the organic structure of the brain and explain how learning takes place at microscopic levels: in regions of the brain (like the hippocampus), in neural column networks (like those involved in recognizing lines of different orientation), and even in single nerve cells that form synaptic connections to other nerve cells and have those connections bolstered or weakened as a result of experience.

And our museum will also have to exhibit machines that learn and think, so to speak: smart machines that can play (and improve in) chess, that can understand much of natural language, interact with human beings, and engage in scientific problem solving.

As the range of exhibits and displays suggests, our imaginary museum, like the group of consultants it has retained, reflects a wide range of theories about learning and about the appropriate level at which to analyze and understand the phenomenon.

Still, as we plan our museum, it is important to step back and to provide both a definition and a little history. As a rough and ready approximation, learning occurs under the following condition: An organism or entity represents a certain amount of information or data at time X; at time Y it can represent new or additional or transformed information that brings it closer to a goal – either a goal of the entity's own choosing or a goal that is intended by another entity, which we may designate a teacher or trainer. Such a formulation allows us to distinguish learning experiences from the sheer accumulation of mud on a tire, on the one hand, and from a computer program that may accomplish amazing feats, but always in precisely the same way, on the other.

From studies of preliterate cultures and naive children, we can with some confidence delineate the major folk theories of learning: Human beings learn by observing others who are more knowledgeable and by imitating, implicitly or explicitly, what they do; asking questions of and listening to what more knowledgeable individuals say; practicing a skill and noting its improvement; receiving clear rewards or punishments that signal which course of behavior should be pursued and which should be eschewed. In literate cultures, the theories of learning expand to include the reading of texts and the taking of classes; and there are of course more idiosyncratic theories that call attention to the learning potential embodied in dreams, drugs, and deities. It is interesting to note the extent to which more formal theories of psychology and pedagogy venture beyond these 'commonsense' views of the learning process.

Before there was a formal psychology or biology of learning, it was philosophers who addressed issues of knowledge – perception, learning, memory, and the like. With Greek and Roman thinkers as background, the philosophers of the Renaissance and the Enlightenment carved out positions that continue to serve as points of orientation today. Descartes took a strong mentalistic position, arguing that the mind operated according to its own principles and that it came stocked with innate ideas. We see echoes of this perspective in the writings of Noam Chomsky and Jerry Fodor, self-identified nativists. The British empiricists, led by Locke, took an opposing perspective, according to which the mind was initially a blank slate; experience etched ideas onto the slate and these ideas become associated with one another. Twentieth-century behaviorists like the Russian physiologist Ivan Pavlov, the American psychologist B. F. Skinner, and the 'learning theorists' portrayed here by Jerome Bruner subscribed to this empiricist point of view.

In the eighteenth century, two new perspectives on learning took hold. Immanuel Kant described the basic epistemological categories – time, space, number, causality – that human beings necessarily imposed on their sensations and perceptions. Individuals did learn from experience, but that experience was necessarily apprehended in temporal, spatial, and causal ways. The Kantian problematic had a great effect on the research program of twentieth-century Swiss developmental psychologist Jean Piaget, who sought to describe the development in infants and young children of these categories of experience. Piaget was also influenced by the writing of his countryman Jean-Jacques Rousseau, who discerned genius in the mind of the child and believed that knowledge should be allowed to unfold within, rather than be imposed didactically upon, the child.

Studies of learning were influenced enormously by the rise of evolutionary thinking, chiefly emanating from the insights of Charles Darwin and, to a lesser extent, other British scholars like Alfred Wallace, Thomas Huxley, and Herbert Spencer. These writers all stressed the continuities between human beings and other animals, and the importance of mental capacities that allowed individual organisms to survive until reproduction. To be sure, instincts were crucial for lower organisms. But it was the vertebrates – and especially mammals – capable of problem solving and planning who emerged as victors in the struggle for survival. As soon as the implications of Darwin's writings became clear, his way of thinking came to dominate both the theories and the empirical work of scientists interested in learning.

The first generation of modern scholars of learning did not shrink from attending to the more complex forms of reasoning in human beings and other primates. But beginning in the early twentieth century, the territory of learning was largely ceded to those researchers who stressed the continuity of learning across the animal kingdom; avoided issues of language, consciousness, and higher-order ratiocination; and strove to explain any intellectual achievement in the most parsimonious and reductionist fashion. Interestingly, this was true not only for those experimentalists who worked primarily with rats and pigeons (the two most common 'model organisms') but also for those, like Edward Lee Thorndike, who studied the acquisition of skills in school-age children. For the first half of the twentieth century, this approach to learning held sway.

And indeed, it might still hold sway today had it not been for the development of high-speed computers and the complex programs that have permitted these electronic entities to compute and solve various kinds of human-scale problems. Once it became clear that computers could mimic human thought processes and – in the view of many – bootstrap themselves over time to a higher level of performance, scientists could no longer withhold such intellectual competences from human beings. Thus was born the cognitive revolution, an important intellectual movement among whose forefathers were the computer scientists Herbert Simon and Marvin Minsky, the linguist Noam Chomsky, and the psychologists George Miller and Jerome Bruner. I view the cognitive revolution as a contemporary interdisciplinary effort to provide scientific answers to long-standing epistemological questions, such as our present consideration – the nature of learning, why it is possible, how it occurs.

In a broad sense, all of the consultants to our museum – the contributors to this issue of *Dædalus,* and the majority of current workers on issues of learning – are offspring of this intellectual revolution of fifty years ago. They recognize the relationship between the long-standing philosophical agenda sketched above, on the one hand, and discoveries in psychology, linguistics, anthropology, neuroscience, cognitive science, and other relevant disciplines, on the other. And they believe that progress is being made in understanding the nature of various kinds of learning, though they may differ on how best to describe that learning and the nature of that progress.

Now that I've surveyed the historical context to our current understanding of learning, it is timely to suggest the major dimensions against which to evaluate the specific contributions of our consultants – as well as those of some other consultants who might have been retained.

Two dimensions seem particularly useful: the learning of species or entities to which these consultants compare human learning; and the type and extent of reductionism entailed in their efforts to explain all manner of learning. Continuing in the tradition laid out by Darwin and his successors, Daniel Povinelli and Michael Tomasello find it productive to delineate the nature of learning in chimpanzees. While both have documented the impressive capacities of chimpanzees, they elect in their essays here to focus on the fault line between chimpanzees and children. Povinelli claims that chimpanzees are incapable of abstract thought; that all of their achievement is the result of observations of concrete objects and events. Tomasello documents that chimpanzees have only the most meager capacities to imitate models, to infer the motives of others, and to transmit any kind of cultural knowledge. The chimpanzee emerges in their accounts as an organism that is incredibly skilled at making use of the information at hand, but that is unable either to conceptualize what is not present or to make use of the incidental knowledge attained by other individuals in its group. Here lies the huge fault line that separates chimpanzees from human children, who from early on can engage in pretend play, imitate elders, and rapidly assimilate the knowledge that earlier generations have accumulated.

Not represented in this collection but worthy of note is the recent claim by primatologist Marc Hauser and his colleagues Noam Chomsky and Tecumseh Fitch that they have identified a crucial capacity that is absent in nonhuman primates: the capacity for recursion.[2] Bonono apes are able to master language-like strings of symbols, provided that the syntax of the string does not depend on the capacity to embed one unit within another. For instance, an ape may appreciate the logic in the proposition "Mommy sleeps," but could be completely stymied by an expression like "Baby said that Mommy sleeps," let alone "Daddy said that Baby said that Mommy sleeps."

2 Marc Hauser, Noam Chomsky, and Tecumseh Fitch, "The Faculty of Language: What Is It, Who Has It, and How Did It Evolve?" *Science* 298 (2002): 1569 – 1579.

In her essay here Alison Gopnik focuses on the characteristics of young children, but she brings to bear an entirely different comparison group. Like several other contemporary developmental psychologists, Gopnik finds it useful to think of the child in comparison to the working scientist – a worker rather like herself. In making this analogy, Gopnik revisits a theme introduced decades ago by Jean Piaget (as well as themes first articulated by the philosopher of science Thomas Kuhn in his discussion of paradigmatic scientific revolutions). However, Gopnik goes well beyond Piaget, who tended to emphasize the limitations in childrens' thinking and the discontinuities between child tinkerer and adult scientist. She argues that, like scientists, very young children are capable of putting forth theories, carrying out experiments, observing the experiments of others, and discerning statistical patterns. In an accompanying essay, Susan Carey characterizes the conceptual growth from child to scientist as a bootstrapping operation; only through such self-constructing operations can children proceed, for example, from early intuitions about quantity to a full-blown sense of number.

Possibly because they are philosophers, Patricia Churchland and Clark Glymour are less concerned with the specifics of experiments involving infants or chimpanzees. Churchland discusses learning at the level of individual nerve cells, while Glymour describes the powerful operations that can be carried out by high-speed computers.

From my vantage point, the key issue their juxtaposed essays raise is the question of reductionism. To put it sharply, can human learning and thinking be adequately reduced to the operations of neurons, on the one hand, or to chips of silicon, on the other? Or is something crucial missing, something that calls for an explanation at the level of the human organism?

As I read her essay, Churchland stresses the importance of understanding the nervous system and chides those philosophers who do place the same value on it. But she does not feel the need to dispense with a psychological level of explanation. For most of his essay, Glymour embraces a tougher-minded reductionism: we should stop trying to solve problems that are too complicated for us and instead turn them over to those ever smarter computers. But in the end, Glymour acknowledges that the problems solved by computers are ones that human beings have formulated and that, at both ends of the process, we need human judgment after all.

As one who surveyed the cognitive sciences in some detail twenty years ago,[3] I am very impressed by the knowledge that has accumulated in the past few decades. Thanks to theoretical and empirical researchers like those represented here, we know a great deal more about the nature of early learning and understanding in human beings, and can point with far greater precision to the ways in which humans differ from their closest biological relatives. Our accumulating knowledge of the nervous system is even more impressive, and the bridges between cognitive science and neuroscience are sturdier. The accomplishments of computers are also striking; and while some of these accomplishments are achieved by methods quite remote from those used by Homo sapiens, we are beginning to have software and hardware that in important respects learn in ways that resemble our own

3 Howard Gardner, *The Mind's New Science: A History of the Cognitive Revolution* (New York: Basic Books, 1985).

learning. In fact I have heard that good chess masters now study computer games and learn new strategies from those inanimate models. Ultimately, of course, debates similar to those between the rationalists and the empiricists in the eighteenth century, and between the learning theorists and the Gestaltists in the first half of the twentieth century, are still being waged. Yet, as Jerome Bruner indicates in his essay, the current debates are conducted in much more sophisticated ways.

But as one who seeks to build a museum of learning, I am struck by the limitations, reflected in the essays collected here, of the still evolving cognitive sciences. Let me mention the principal ones.

First of all, these disciplines almost always deem the scientific mind as the proper model of human thought. The claim that all cognition, learning, development, and intelligence are best represented by those of the scientist was Piaget's great contribution, and also his weakness. And we see his sentiments in most of the essays here. But human thinking and learning is achieved as well by artists, musicians, politicians, businesspeople, inventors, religious leaders, and dreamers – we must understand their forms of learning and the ways in which they may differ from the cognition of the theoretical physicist or the benchtop chemist.

Second, the instances of learning that are most frequently examined typically take place over brief periods of time: nanoseconds in the case of computers, milliseconds in the case of nerve cells, an hour or less in the case of most experimental trials. Yet the forms of human thought that are most valued often require the investment of months or even years. What of the learning involved in Albert Einstein's coming up with the theory of relativity; Marcel Proust's penning *À la recherche de temps perdu*; Andrew Wiles's solution of Fermat's theorem; or Mahatma Gandhi's creation of peaceful nonviolence; or, indeed, Ramon y Cajal's studies of the nervous system, or John von Neumann's formulation of the nature of computer programming? Even the high-speed computers that can handle far more variables than ordinary mortals can do not illuminate the nature of original artistic, scientific, or political thought.

Moreover, the contributions of cognitive science to schooling – the chief institution devoted to learning – remain modest. As one who spends his life at a school of education and has devoted much time to school reform, I could not help but be struck by the virtual absence here of any reference to schools, formal teaching, the 3Rs, and the scholarly disciplines – in short, the realms that most individuals think of nowadays when they think of learning. Part of the explanation for this is undoubtedly that schools are very complex institutions and the processes of learning that are supposed to take place there over months or years are difficult to capture in scientific research.

Still, I think that more can be said about how our current understanding of learning might influence education, as well as the obstacles that make such applications difficult. In my own work I have recently focused on two lines of research. The first outlines the various misconceptions that readily arise in early childhood and that considerably complexify the mastery of the disciplines. It turns out that young children readily embrace creationist accounts of the origin of the species, a phenomenon that makes difficult the learning of evolutionary theory; embrace Aristotelian accounts of the behavior of physical matter, which render the mastery of Newtonian physics problematic; and readily embrace one-dimensional accounts of historical and political events, thus making it difficult to appreciate complex and multicausal accounts. Recognition of the early emergence and dogged persistence of these misconceptions is an essential component of effective pedagogy. Teachers must directly confront these misconceptions and give students ample opportunities to air their understandings and misunderstandings, to discover where they are inadequate and where they require revisions.[4]

I have also collected evidence against the contention that intelligence is a single, all-encompassing human capacity. I favor an alternative account: that all human beings possess a range of intelligences and that we differ from one another in our intellectual profiles. Throughout most of history, educators have ignored this possibility and have taught subjects in one way – thereby inevitably favoring students who are strong in linguistic and logical ways of thinking. It is possible to reverse this uniform approach, and so reach more students, by presenting materials in a multitude of ways and giving students options in how they may convey their own understandings.

Alas, even if we had exquisitely detailed and powerful theories of learning, this would be no guarantor that they would be adopted widely in schools. As David Olson argues in his recent book, schools are bureaucratic organizations that respond principally to political pressures and institutional imperatives.[5] This, in short, is the reason that politicians in America talk incessantly about test scores and international comparisons, and rarely if ever mention what has been learned about learning. Independent schools have somewhat greater latitude in what they prescribe but they are by no means immune from these social pressures. Only in home-schooling or individual tutoring can the student readily benefit from our growing understanding of learning. And only when ways are found to bridge the gap between the knowledge being accumulated by scholars and the typical operations of schools in the nation-state will the pipeline between research and practice be opened.

Finally, I believe that there is also a conceptual gap that needs to be addressed – both by our consultants and by our hypothetical museum of learning.

That gap concerns the fact that human beings are social, cultural, and historical creatures as much as we are neurological, psychological, and computational creatures. We evolved to do many things well – but we did not evolve to create calculus or write the U.S. Constitution or compose classical music or invent airplanes and the pill. Nor could anyone have anticipated, even fifty years ago, the civil rights revolution, or the feminist revolution, or the fall of communism, or the proliferation of nuclear weapons, or the rise of the World Wide Web in an increasingly globalized civilization. Yet somehow individuals growing up in the early twenty-first century must be able to master these bodies of

4 See Howard Gardner, *The Unschooled Mind* (New York: Basic Books, 1991) and *The Disciplined Mind* (New York: Penguin Putnam Books, 2000).

5 David Olson, *Psychological Theory and Educational Reform* (New York: Cambridge University Press, 2003).

knowledge and cope with these events of historical significance. Little in our sciences of learning addresses issues of this scale; our cultural, historical, and literary studies do not make much contact with our scientific approaches; an interdisciplinary span across these broad disciplinary terrains still eludes us.

In short, there will be a great deal of interest on display in our hypothetical museum of learning. But there will also be a large number of empty rooms – for there is still so much that we have to learn about learning.

Jerome Bruner

A short history of psychological theories of learning

Learning remains an elusive topic, despite the endless research lavished on it. And what we mean by it, of course, is shaped by how we choose to study it. Concentrate on how children master their native language and you arrive at a very different conception of learning than had you researched how undergraduates memorize nonsense syllables. Does learning to finger a Bach cello sonata tap the same learning processes as learning to trace your way through a finger maze? Is all learning alike, reducible to a common set of principles?

Two learning tasks are said to be alike if mastering one makes mastering the other easier – the so-called transfer criterion. But what is transferred? Is it responses? Rules? Or do we simply learn how to learn, as when with enough practice we become exam-wise or tax-form-wise? How do we learn the lay of the land? How do we learn to concentrate our attention?

And then there are questions about differences in how learning occurs. Do all species learn in the same way and do the bright and the dull go about it in like manner? And what about external inducements, rewards, and punishments? Are all learning situations comparable?

I used to give the star performers of the experiments I'd just completed to my young daughter. These rats seemed to develop a more open curiosity under her magnanimous care. What, indeed, does domestication do to an animal's approach to learning? Were those insights achieved by Wolfgang Koehler's pampered chimpanzees – their figuring out how to rake in an out-of-reach banana by putting two sticks together, for instance – simply the result of the leisurely tutelage they received on that German island of Tenerife?[1] It used to be said, only half jokingly, that Yale stimulus-response-reinforcement learning theory was different from more cognitive California theory because Clark Hull in New Haven taught his graduate students that rats "should get on with

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1 Wolfgang Koehler, *The Mentality of Apes* (New York: Harcourt Brace, 1926). This was originally published in German in 1917.

it," while Edward Tolman counseled his at Berkeley that rats need time enough to pause at the choice points in a maze.

And finally, do we learn for learning's sake, or must we be extrinsically motivated to do so? Assuming the latter, the Yerkes-Dodson law tells us that too much or too little motivation reduces learning. I checked that out once myself and got a surprise. I found that very hungry and just moderately hungry rats learned to find their way through a succession of pairs of doors. The correct path through was marked redundantly in two ways: follow a left-right-left-right path, or just choose the darker door at each choice point. The hungry rats learned only one of the two cues; the moderately hungry rats learned both. The less hungry rats had a more open curiosity – like my daughter's pets.

Given all this, it is natural enough that scientists would want somehow to simplify what we mean by 'studying learning.' And, of course, the standard way of doing that is to agree on some paradigm that would make it possible to compare results. That is exactly what happened at the very start of learning research. But, as often happens, rival paradigms came into existence and, alas, this research soon became a war of would-be paradigms. Indeed, the learning theory wars that resulted came to dominate the psychological research scene from the latter nineteenth century until a decade after World War II, with various 'schools' devising clever experiments to demonstrate how well their paradigm worked, or how poorly rival ones fared.

There were two competing paradigms from the start, each with its variants. The principal one, a child of its times, was molecular associationism, a metaphoric extension of the atomism of nineteenth-century physics. (As the quip goes, psychology is forever subject to physics envy.) The atomism of learning theory embodies the notion that learning consists of the association of ideas, memories, sensations, whatever; at its heart is the conception of the associative bond, the linkage that co-occurrence or spatial proximity produces between two sensations or ideas. While associationism is of ancient provenance, it had more recent philosophical adherents as well – not only Aristotle, but Locke, Berkeley, Hume, and pére et fils Mills. Indeed, by the mid-nineteenth century, philosopher-psychologist Johann Friedrich Herbart had proclaimed the associative bond as the keystone of the new psychology.

This paradigm found further, if indirect, support in the newly burgeoning brain physiology of those times. As the nineteenth century entered its last quarter, the older phrenology of the days of Gall and Spurzheim was reformulated in terms of newly discovered cortically localized 'centers' in the cerebral cortex, each dedicated to a particular function. Perhaps the most compelling localization study was the one conducted in 1870 by the German physiologists Fritsch and Hitzig. In their study, electrical stimulation of different spots in the medial-lateral cortex produced particular, quite finite motor responses: stimulating one spot produced flexion of a monkey's forearm, another would turn his eyes upward, still another would turn them downward.[2] If the brain were organized in this localized punctate way, psychologists asked, why not the mind as well? One needs to remember that the prevailing philosophical view among those

2 The classic article was Gustav Fritsch and Eduard Hitzig, "Ueber die elektrische Erregbarkheit des Grosshirns," *Archiv der Anatomie und Physiologie* (1870): 300 – 332.

scholars was psychophysical parallelism, which held that mind and brain move along parallel tracks.

Their critics, however, championed another model – that of molar configurationism. This paradigm took as its major premise that mind and brain alike operate as integral systems controlling the functioning of component parts. Like its rival, it too rested its case on brain physiology, for there was already plenty of evidence that overall cortical processes controlled localized centers – the neural 'mass action' holism represented by the renowned Pierre Flourens.

The brain's mass action was analogous to the phenomenology of everyday life – that ordinary experience transcends its bits and pieces. The 'urban scene,' after all, is more than just a collection of taxis, buildings, pedestrians; its properties as a whole shape the elements that make it up. Gestalt psychology was, of course, the most direct expression of this view, and it had much to say about how learning was a matter of overall organization rather than of local associative linkages.

Consider now the rise of the associationist paradigm. That closing quarter of the nineteenth century was a time of many new studies of learning – mostly concerned with the memorization of lists of words or pairs of words to be associated. But it was the nonsense syllable principally that gave associative bonding its scientific flavor. Hermann Ebbinghaus used nonsense syllables in order to rule out past experience and 'meaning' in explanations of learning. Ebbinghaus's 1885 *Ueber das Gedaechtnis* is a tedious account of learning lists of nonsense syllables (with Ebbinghaus himself as the subject of most of the experiments). His findings – for example, that nonsense syllables in the middle of the list are more slowly learned than ones at the beginning or end – are easily reproducible.[3]

But the associative bond, even between nonsense syllables, soon came to seem mentalistic, too fragile to suit the scientific taste of the times. So by the turn of the century it was replaced by Pavlov's more scientifically solid 'conditioned reflex.' Pavlov's paradigm physicalized associationism, turning its content into something more measurable while preserving its associative form intact. All his paradigm required was linking and relinking stimuli and responses: a salivary reflex, once produced by food, was now evoked by a bell signaling the coming of food. Pavlov's Nobel Prize in physiology seemed to clinch the triumph of physicalism. But Pavlov himself was not altogether pleased, as we'll see later.

Now turn to configurationism, which had no shortage of psychologists to support it, dubious as many were of associationism's abstractness and its remoteness from ordinary experience. Configurationism had the support of brain research as well, with the holistic neurology of the indomitable Flourens still very much in vogue. Also in those fin de siècle times there was a rising tide of interest in how language and culture shaped mind, with figures like Emile Durkheim and Max Weber in the neighboring discipline of sociology urging that culture – not just individual encounters with the world of physical nature – also forms mind.

3 Ebbinghaus's 1885 classic is available in English only in brief, but representative excerpts may be found in Wayne Dennis, *Readings in the History of Psychology* (New York: Appleton-Century-Crofts, 1948), 304–313. Interestingly enough, Ebbinghaus's original monograph was published in its entirety in English translation in 1913 by Teachers College, Columbia University – very much in keeping with the then dominant emphasis on rote learning in American education. It has long been out of print.

Gestalt theory was the prime exemplar of the configurationist trend in those early years, though it hit its full stride only after World War I. Its credo was that all systems – physical, biological, and mental – have the intrinsic character of controlling the local elements that compose them. Field theory in physics was its model, and its proclaimed maxim was "The whole is greater than the sum of its parts," which the Gestaltists proceeded to confirm with a steady stream of clever studies on human perception. The Koehler chimpanzee studies on Tenerife were intended to make the same point where learning was concerned: There was no way in which those chimpanzees could turn a pair of sticks into a reaching tool by the simple 'association' of elements. It took an act of insight to do so, a way of configuring the whole situation.

Koehler had a deep belief in the ubiquitousness of configurationism in all of nature. He launched one of his first major attacks on associationism by arguing the insufficiency of atomism, in a book bearing the forbidding, if telltale, title *Ueber die physische Gestalten im ruhe und im stationaren Zustanden* (On physical configurations at rest and in stationary states). If atomism was insufficient even in physics, Koehler asked, how could it serve as a paradigm for psychology?[4] He applied a phenomenon in visual perception to make an analogy that would drive home his point: When two nearby points of light are briefly flashed one after the other, the eye perceives pure apparent movement, not the light points moving. The whole, then, is indeed different from a sum of its parts.

Now as it happens, Pavlov himself came to advocate a kind of linguistic configurationism. How does the conditioned response square with an ordered phenomenon such as language? Does language change how stimuli are interpreted, how a conditioned stimulus is substituted for an unconditioned one in the case of human beings? Troubled by such issues in his later years, Pavlov proposed a Second Signal System whose stimuli were not raw physical inputs, but language imbedded in codes and categories. Thus linguistic synonymy influenced stimulus substitution in ordinary conditioning.

Some say that Pavlov was driven to his new views by communist ideologues with prematurely Gramscian leanings, but in fact his Second Signal System was quite in keeping with the European tradition of human studies, *Geisteswissenschaft*, rather than with *Naturwissenschaft* – a well-revered tradition among the Russian intelligentsia. Still, structuralism was virtually the hallmark of the lively Russian literary and linguistic scene of Pavlov's day, and the Second Signal System was certainly, to some degree, a response to that scene. I recall flying to Moscow from Paris in the 1960s with the celebrated Russian emigré linguist Roman Jakobson. He laughed when I told him about Pavlov's later turn and about the accusation that he had knuckled under to the *nomenklatura*. "No, no, Jerry, communist ideologues weren't needed, just being Russian was enough. And being a Russian intellectual besides! Not even Pavlov could live with the idea that language makes no difference, that people learn like dogs!"

Small wonder that cultural theorists like Vygotsky and Luria took over after

4 For Koehler's philosophical allegiances, see Mary Henle, ed., *The Selected Papers of Wolfgang Koehler* (New York: Liveright, 1971). Perhaps the best and most accessible account of Gestalt psychology's empirical accomplishments (mostly before Hitler's rise to power) is Kurt Koffka, *Principles of Gestalt Psychology* (New York: Harcourt Brace, 1935).

Pavlov and that many of the post-Pavlovian young studied Gestalt psychology at the Institute of Psychology in Berlin in the years after.[5]

The climax of the rivalry between associationism and configurationism came in America in the years before World War I. Nourished by the imposing Edward Lee Thorndike of Teachers College, Columbia University, the associative paradigm had flourished in the United States. Thorndike had been a postdoctoral student at one of the major centers of associationism in Germany. On his return to America (and Teachers College) he popularized practice and repetition as the routes to proficient school learning: practice and repeat as you would were you memorizing nonsense syllables.[6]

But the associationist research program soon changed in America under the influence of Pavlov. J. B. Watson, the founder of American behaviorism, popularized Pavlov and gave his findings an American twist, by stressing how all learning occurred through stimulus and response. I sometimes wonder whether it was Watson's oversimplifications that eventually drove American associationist learning theorists to their zealous rigor in exploring Pavlov's ideas. It was the energy and determination of their research that made America for half a century the home of later Pavlovianism, a half century dominated by the likes of Walter Hunter, Clark Hull, Edward Guthrie, B. F. Skinner, Kenneth Spence – all distinguished, self-professed stimulus-response learning theorists.

Their forte was the well-designed animal experiment: maze running, discrimination learning, operant conditioning à la Skinner box, and the like – mostly with rats as subjects, but sometimes pigeons, and occasionally monkeys. Undergraduates were used as well, but again, mostly in rote learning experiments – in what was referred to in my graduate student days at Harvard as 'dustbowl empiricism.' It was in these days that Pavlov's dog became a metaphor for American know-nothing anti-intellectualism.

The burden of the behaviorists' findings, taken collectively, was that repetition of a task, with suitable reinforcement for completing each trial, improved performance. There were subtleties, to be sure – like the deleterious effects of massing trials rather than spacing them, creating interference by setting positive and negative reinforcement in a conflicting relationship, and the like. But the overall outcome of the work, where ordinary everyday learning was concerned, was, I believe, much as I've stated it. I'll return to this matter later.

But, as in Europe earlier, a contrarian configurationism soon came into being. Partly it was influenced by Gestalt theorists, now in America and sparking the opposition, but it had American roots as well, nourished particularly by Edward Tolman, who was sympathetic to the work of Koehler and was a close friend of Kurt Lewin, a latter-day leader in the Berlin Gestalt group. Tolman's brother Richard, moreover, was a distinguished nuclear physicist and shielded him well from old-fashioned atomistic notions – and, indeed, from physicalistic temptations. Tolman, from the start, was a cognitivist.

5 Lev Vygotsky, *Thought and Language* (Cambridge, Mass.: MIT Press, 1962); Aleksandr Romanovich Luria, *The Role of Speech in the Regulation of Normal and Abnormal Behavior* (New York: Liveright, 1961).

6 Edward L. Thorndike's classic is the three-volume *Educational Psychology*, which appeared in 1913 – 1914.

Tolman's first major book appeared in 1932 and it quickly gained adherents among the discontented, and there were plenty of them. His students – notably David Krech, but many others as well – also joined the battle against associationism. By World War II, there was virtually open conflict in America between configurationist and associationist learning theorists – the first holding that learning is principally a task of organizing knowledge from the top down, the second insisting that it is accreting it from the bottom up. The configurationists, though still a minority, had been officially well received on the American scene when they fled Hitler's Europe. Koehler was invited to deliver the William James Lectures at Harvard, and Kurt Lewin became virtually a cult figure in social psychology. The displaced members of the old Gestalt group were soon well placed in leading American universities. They made commonsense phenomenology seem commonsense rather than arcane, an achievement given the hold of behaviorist American psychology. Learning began to be understood as grasping things in context, not in bits.

Take Edward Tolman's research as an example. He taught that learning is like mapmaking and that to learn is to organize things in the light of their utility for achieving ends. In "Cognitive Maps in Rats and Men," his still renowned Research Lecture to the Berkeley faculty in 1947, Tolman claimed that trial and error is not so much acting out habits to discover which are effective, but rather a looking back and forth to get the lay of the land in order to construct a solution. That is why he urged his graduate students not to rush their rats through the maze.[7] He believed that our cognitive maps are not mirrors of the happenstance of our encounters with the world, but a record of our strivings and what has proved relevant to their outcome. His views in this sense were basically pragmatist, perhaps because of his years of exposure as a psychology graduate student to Harvard's pragmatist philosophers, particularly C. I. Lewis, whom he greatly admired. Following Tolman's lead, David Krech went to the extent of proposing that learning is hypothesis driven, not just passive registration. Even rats, Krech tried to show, generate hypotheses.[8]

It's revealing to compare Tolman with the leading, perhaps most radical associationist behaviorist of the same period, B. F. Skinner. Skinner was surely as compelling in defense of operant conditioning as Tolman was of cognitive map theory. His central concept was the operant response – an act not initially under the direct control of some particular feature of the immediate environment. An example of an operant response is provided by a starting pigeon in a Skinner box whose pecking of the button on the box's wall either produces or fails to produce a reinforcement (a grain of seed, say). Any reinforcement increases the likelihood of the operant response occurring again, the level of likelihood depending upon whether the reinforcement always follows the response or does so only sometimes, and whether it does so regularly (periodically) or irreg-

7 Tolman's most influential book was *Purposive Behavior in Animals and Men* (New York: Century, 1932). His Berkeley lecture was later elaborated in his International Congress address, "Cognitive Maps in Rats and Men," *Psychological Review* 55 (4) (1948): 189 – 208.

8 Krech's bold study (written under his original name) is I. Krechevsky, "'Hypothesis' versus 'Chance' in the Presolution Period in Discrimination Learning," *University of California Publications in Psychology* 6 (1932): 27 – 44.

ularly (aperiodically). Partial aperiodic reinforcement, for example, evokes a rather more persistent response than one might expect, though Skinner would scoff at interpreting such persistence as hope springing eternal. Learning, in Skinner's austere terms, is under the sole control of schedules of reinforcement: reinforcement can only be positive; punishment does not affect learning. And that is about it. As Skinner would sometimes say, a bit ironically, learning scarcely needs a theory.[9]

Not all behavioral associationists, to be sure, shared Skinner's disdain for theory. Clark Hull at Yale, indeed, elaborated his theory into a highly refined set of axioms about what constitutes positive and negative reinforcement, what makes a conditioned stimulus generalize along a certain gradient, how organisms anticipate reinforcers, and the like – all in rather exquisite and specialized detail. His first books – the 1943 *Principles of Behavior* and the more triumphally titled 1952 *A Behavior System* – bristle with tables and idealized learning curves and with abstract formulae for relating those findings to his central axioms – perhaps a prophetic effort to devise a mathematical model of learning, the preoccupation of computational psychologists a generation later.[10]

The conflicts between Hull and Skinner, and between both of them and Tolman, were the last battles of the learning theory wars. Learning theory in the classic sense died around 1960 – though there are still Skinnerians who stalwartly continue to publish operant findings, mostly for each other. I know of no more Tolmanians or Hullians.[11]

It was the cognitive revolution that brought down learning theory or, perhaps, focused attention elsewhere. After 1960, say, stimulus-response learning theory seemed quaintly stunted, hemmed in by its own self-denial. As for more molar, cognitive learning theories, many of their ideas were restated and absorbed into general cognitive theories such as Newell and Simon's on problem solving, or Bruner, Goodnow, and Austin's on thinking, or Miller, Galanter, and Pribram's on planning.[12] By the latter 1960s, learning was being translated into the concepts of information processing, with no compulsion to elevate one kind of learning over another in terms of its 'basic' properties. Certainly, the old wars were over. And so, interestingly, were the old rat labs and their ubiquitous mazes.

As I reflect on the transition period, I think that it was the study of language and particularly of language acquisition that precipitated learning theory's decline. Language use and its acquisition are too out of reach of piecemeal S-R learning: efforts to bring them into the fold soon become absurd, and linguists have mostly dismissed them as such.

9 B. F. Skinner, "Are Theories of Learning Necessary?" *Psychological Review* 57 (1950): 193 – 216.

10 Clark L. Hull, *Principles of Behavior* (New York: Appleton-Century-Crofts, 1942) and *A Behavior System: An Introduction to Behavior Theory Concerning the Individual Organism* (New Haven, Conn.: Yale University Press, 1952).

11 The most detailed and authoritative volume on the classic learning theories is Ernest R. Hilgard, *Theories of Learning*, 2d ed. (New York: Appleton-Century-Crofts, 1956).

12 Alan Newell and Herbert A. Simon, *Human Problem Solving* (Englewood, N.J.: Prentice Hall, 1972); Jerome Bruner, Jacqueline Goodnow, and George A. Austin, *A Study of Thinking* (New York: Wiley, 1956); George A. Miller, Eugene Galanter, and Karl Pribram, *Plans and the Structure of Behavior* (New York: Holt, Rinehart, and Winston, 1960).

The contemporary linguistic assault on associationist learning theory began with Noam Chomsky's gloves-off critical review of Skinner's *Verbal Behavior*.[13] But the mentalist, problem-solving emphasis it introduced has now expanded beyond language as such. One now asks whether cultural codes are learned in some language-like way. Neither psycholinguistics nor cultural psychologists think of learning in the old-fashioned learning-theory way.

I think it would be fair to say that, under this new dispensation, more has been learned during the last three decades about language acquisition than in any prior century – more, indeed, than in all of them combined. And it's well to remember that the flood of research that made this possible was precipitated by the linguist Chomsky, not by a learning theorist.

The turn to language, moreover, has shifted learning-related research away from many of the older, artificial experimental paradigms – mazes, paired-associate word lists, nonsense syllables, and the rest. Let me give an example: the prediction that children must be so early tuned to the structure of their native language that they pick up its phonemic distinctions in parental talk even before they learn to understand or talk the language proper. It is a prediction that grows out of linguistic and developmental theory. And you can test it in context *directly* – by seeing whether childrens' prelinguistic babbling has a higher frequency of native-language phoneme sounds than of foreign ones. And so it does: French babies babble in French, Spanish in Spanish, etc. With such experiments, one tests in context, not in a maze, and knows without extrapolation whether the experiment has any bearing on real learning by real people in real life.

Shall we conclude, then, that three-quarters of a century of warfare between associationist and configurational learning theories taught us little or nothing about the real nature of learning? That would be a mistake.

Both Pavlov's dogs and Koehler's chimpanzees did, in fact, learn, though in different ways and in different circumstances. And we have ample reason to suspect that neither of their approaches can be reduced to the other. In the next turn of things perhaps we will figure out how to put them together. But of one thing at least I am quite convinced. You cannot strip learning of its content, nor study it in a 'neutral' context. It is always situated, always related to some ongoing enterprise. Perhaps there is no such thing as 'learning in general' – and perhaps that is what we should learn from Pavlov's dogs, Koehler's chimps, and the disputes over learning that they once symbolized.

13 B. F. Skinner, *Verbal Behavior* (Cambridge, Mass.: Harvard University Press, 1947). Chomsky's structuralist-mentalist views were first published in his *Syntactic Structures* (The Hague: Mouton, 1957). His frontal attack on Skinner's *Verbal Behavior* (in *Language* 35 [1959]: 26 – 34) only two years later came as a rather unexpected, though scarcely a conceptual, surprise.

Alison Gopnik

Finding our inner scientist

In 1946, the philosopher of science Karl Popper had a fateful meeting with the philosopher of language Ludwig Wittgenstein at the Cambridge Philosophy Club. In a talk to the Club, with Wittgenstein in the audience, Popper described several "philosophical problems" – important, difficult questions that he thought would one day be answered. Here Popper was issuing a direct challenge to Wittgenstein, who had argued that philosophy could only analyze linguistic puzzles – not solve any real problems.

The visit has become most famous for the subsequent controversy among eyewitnesses over whether or not Wittgenstein's response to this challenge was to angrily brandish a fireplace poker at Popper.

But there is a more interesting aspect to the story. One of the problems Popper described was the problem of causal induction: How is it possible for us to correctly infer the causal structure of the world from our limited and fragmentary experience? Popper claimed that this problem would one day be solved, and he turned out to be right. Surprisingly, at least part of the solution to the problem comes from a source about as far removed from the chilly Cambridge seminar room of fifty years ago as possible – it comes from babies and young children.

The past thirty years have been a golden age for the study of cognitive development. We've learned more about what babies and young children know, and when they know it, than we did in the preceding two thousand years. And this new science has completely overturned traditional ideas about what children are like.

The conventional wisdom, from Locke to Freud and Piaget, had been that babies and young children are irrational, egocentric, pre-causal, and solipsistic, governed by sensation rather than reason, and impulse rather than intention. In contrast, the last thirty years of research have taught us that even the youngest infants – literally newborns – already know a great deal about a wide range of subjects. Moreover, we have been able to chart consistent changes

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in children's knowledge of the world as they grow older. Those changes suggest that even the youngest babies are solving Popper's problem: somehow they accurately learn about the causal structure of the world from their experience.

Consider how children come to understand one particularly important aspect of the world – the fact that other people have emotions, desires, and beliefs and that those mental states cause their behavior. All of us know that other people have minds in spite of the fact that we only see the movements of their physical bodies. This raises another ancient philosophical question: How do we come to know other minds?

In the last fifteen years, a great deal of empirical research has begun to illuminate the intuitive psychology of even the youngest human beings. Infants seem to be born believing that people are special and that there are links between their own internal feelings and the internal feelings of others. For example, newborns can imitate facial expressions: when an experimenter sticks his tongue out at the baby, the baby will stick out her own tongue; when he opens his mouth, she will open hers; and so on. In order to do this, newborns must be able to link their own internal kinesthetic sensations, the way their mouth feels from the inside, to the facial gestures of another person – that pink thing moving back and forth in the oval in front of them.[1]

By a year, babies seem to understand that mental states can be caused by external objects. For example, fourteen-month-olds saw an experimenter make a disgusted face as she looked inside one box, and a happy face when she looked inside another box. Then she gave the children the boxes. The children cheerfully opened the 'happy' box but kept the 'disgusted' box shut.[2] In another experiment, infants seemed to predict that a hand that had reached toward an object would continue to reach toward it even when it was placed at a new location – just as their own hands would. (They did not, however, make this same prediction about a stick that had made contact with an object.)[3]

By two, children seem to understand that their own desires may differ from the desires of others. And by two and a half, they extend this understanding to perception. In one study, the experimenter demonstrated disgust toward a food that the baby liked (goldfish crackers) and happiness toward a food that the baby did not like (raw broccoli), and then asked the baby to "give [her] some." Fourteen-month-olds always gave her the crackers, but eighteen-month-olds gave her the broccoli.[4] In another experiment, thirty-month-old children could accurately predict that someone on one side of an opaque screen would see a toy placed there, but someone on the other side of the screen would not.[5]

1 Andrew N. Meltzoff and Wolfgang Prinz, eds., *The Imitative Mind: Development, Evolution, and Brain Bases* (New York: Cambridge University Press, 2002).

2 Betty M. Repacholi, "Infants' Use of Attentional Cues to Identify the Referent of Another Person's Emotional Expression," *Developmental Psychology* 34 (5) (September 1998): 1017 – 1025.

3 Amanda L. Woodward, Jessica A. Sommerville, and Jose J. Guajardo, "How Infants Make Sense of Intentional Action," in Bertram F. Malle and Louis J. Moses, eds., *Intentions and Intentionality: Foundations of Social Cognition* (Cambridge, Mass.: MIT Press, 2001), 149 – 169.

4 Betty M. Repacholi and Alison Gopnik, "Early Reasoning About Desires: Evidence from 14- and 18-Month-Olds," *Developmental Psychology* 33 (1) (January 1997): 12 – 21.

5 John H. Flavell, Barbara A. Everett, and Karen Croft, "Young Children's Knowledge

By four, children can understand that beliefs, as well as desires and perceptions, may differ, and that beliefs may be false. For example, you can show children this age a candy box that, much to their surprise, turns out to be full of pencils. Three-year-olds will say that they always thought that there were pencils in the box, and that everyone else will think that there are pencils inside, too. But four-year-olds understand that they and others may falsely believe that there are candies in the box.[6]

By six, children start to understand that beliefs may be the result of interpretation, and that different people may interpret the world differently. When you give five-year-olds a small glimpse of a picture – a triangular fragment that might imply a sailboat, or a witch's hat, or many other things – they don't understand at first that people might interpret this fragment in different ways. But by six or so they get this right.[7]

At each point in development children know some quite abstract and sophisticated things about how the mind works, knowledge that leads them to surprisingly accurate and wide-ranging predictions and explanations. They seem to understand something about how events in the world cause different mental states, and about the way these mental states in turn cause particular human actions. Yet they fail to understand other aspects of the causal structure of mental life – misunderstandings that lead to surprisingly inaccurate but consistent predictions and explanations. As they get older, the misconceptions fade away and their causal knowledge becomes more extensive and precise.

Evidence seems to play an important role in these developments. For example, younger siblings from large families, who have a lot of experience with a variety of other minds, develop this understanding more quickly than solitary only children.[8] We can also show that giving young children relevant evidence can actually accelerate their developing understanding of the mind. For example, we can, shades of Popper, set out to show children who do not yet understand false beliefs that their predictions about another person's actions can be systematically falsified; we can show them that someone who sees the closed box will, in fact, say there are candies inside of it. A month later, children who saw evidence that they were wrong were more likely to understand how false beliefs really work than children who did not.[9]

We can tell very similar stories about children's developing causal knowledge of everyday physical phenomena, like gravity and movement, and everyday biological phenomena, like illness and growth. These patterns of development have led many of us to draw an analogy between children's learning and the historical development of scientific theo-

about Visual Perception: Further Evidence for the Level 1 – Level 2 Distinction," *Developmental Psychology* 17 (1) (January 1981): 99 – 110.

6 Josef Perner, Susan R. Leekam, and Heinz Wimmer, "Three-Year-Olds' Difficulty with False Belief: The Case for a Conceptual Deficit," *British Journal of Developmental Psychology* 5 (2) (June 1987): 125 – 137.

7 Marjorie Taylor, "Conceptual Perspective Taking: Children's Ability to Distinguish What They Know from What They See," *Child Development* 59 (3) (June 1988): 703 – 718.

8 Jennifer M. Jenkins and Janet Wilde Astington, "Cognitive Factors and Family Structure Associated with Theory of Mind Development in Young Children," *Developmental Psychology* 32 (1) (January 1996): 70 – 83.

9 V. Slaughter and Alison Gopnik, "Conceptual Coherence in the Child's Theory of Mind," *Child Development* 67 (6) (1996): 2967 – 2989.

ries, an analogy I've called the theory theory. Like scientists, children seem to develop a succession of related intuitive causal theories of the world, theories that they expand, elaborate, modify, and revise in the light of new evidence.

There is only one problem with the theory theory, and it harks back to Popper's talk at Cambridge. We have had almost no idea how scientists learn about the world; when we 'theory theorists' turned to philosophers of science to find out about scientific learning mechanisms, we got the runaround. Philosophers knew that insofar as a theory was a deductive system, you could say something about how one part of the theory should follow from another; and they knew something, though much less, about how evidence could confirm or falsify a hypothesis that had been generated by a theory (this, of course, was where Popper made his contribution).

But they knew almost nothing about what has been called the logic of discovery – the way that experience itself might lead to the generation of new theories or hypotheses. And notoriously, they knew even less about what psychologists call conceptual changes (and what the rest of the world, ad nauseam, calls paradigm shifts), in which the very vocabulary of a theory seems to change in the light of new evidence. Some philosophers said that to answer questions about discovery and conceptual change you would have to go talk to psychologists. Others, even more discouragingly, said the questions were simply unanswerable. And if there were no accurate learning mechanisms that underlaid science, if Wittgenstein was right that the problems of induction, discovery, and conceptual change were not solvable, then the whole enterprise of science was in doubt.

So philosophers of science and developmental psychologists have been in the same unfortunate boat, convinced that the scientists and children they study are getting to the truth, perhaps even suspecting that they may be using some of the same learning mechanisms to get there, but unable to determine how. So both groups have mostly ended up waving their hands and talking vaguely about paradigm shifts and constructivism.

Ten years ago I would have said that this sad state of affairs was irremediable, at least for the immediate future. Our generation of scientists would have to labor over the details of the empirical natural history of learning and leave it to the next generation to develop precise and convincing explanations of learning. But, rather remarkably, age has made me more optimistic. Though we are still very far from having the whole story, I think there is a new line of work that is actually on the right track. We are beginning to understand not only what babies (and scientists) know when – but also *how* they learn it and *why* they get it right.

The general structure of the explanation comes from an entirely different part of cognitive science: the study of vision.[10] Indeed, the study of vision has been the most striking, though unheralded, success story in cognitive science – a case of real rather than just-so evolutionary psychology. Although we don't typically think of vision as a kind of learning, there is a sense in which the two processes are quite similar. The visual system takes a pattern of retinal input and generates accurate representations of three-dimensional objects moving through space. It has to solve what has been called the inverse problem: the three-dimensional world produces cer-

10 Stephen E. Palmer, *Vision Science: Photons to Phenomenology* (Cambridge, Mass.: MIT Press, 1999).

tain patterns at the retina and the brain has to work backward to accurately recreate the world from that information. We have a remarkably good understanding of the computations, and even the neurological mechanisms, that are involved in this process.

The visual system solves the inverse problem by making certain very abstract and general assumptions about how the three-dimensional world creates patterns on the retina. And we can explain the way the system works by describing it in terms of these assumptions, and in terms of knowledge, rules, and inferences – just as we can explain how my computer works in this way. For example, the visual system seems to assume that the images at the retina of each eye are projections of the same three-dimensional objects in the world, and that the discrepancies between them are the result of geometry and optics. We can show mathematically that, given these assumptions, only some three-dimensional configurations of objects, and not others, will be compatible with a particular set of retinal patterns. This enables us to also say mathematically whether a visual system (human, animal, or robotic) generates the right representations of the spatial world from a particular pattern of data. In fact, the human visual system seems to be about as good at getting the right representations as it could possibly be.

The assumptions that allow these inferences to take place are themselves contingent and sometimes may be violated. For example, the View-Master toys and 3-D glasses of my youth and their modern virtual reality equivalents artificially create retinal images that normally would be generated by three-dimensional objects, and the visual system gets it wrong as a result. We see a three-dimensional Taj Mahal or oncoming train rather than two slightly different two-dimensional photographs.

But the consequences of those assumptions are deductive. It is not always true that retinal images are generated by light reflecting off the same three-dimensional object onto two separate retinas. But if it is true, then we can say, as a geometrical fact, that only certain kinds of images will result. In fact, of course, in real life, without the demonic View-Master to confuse things, the assumptions of the visual system will almost always be correct. That's why the designers of computer vision systems build those assumptions into their programs, and presumably that's why evolution built those assumptions into the design of the visual cortex.

In learning, as in vision, our brains may be performing computations that we can't perform consciously. We see a three-dimensional world or know about a causal one, without having to bother about the implicit computations that let us generate that world from the data. In vision science, we figure out which computations the brain performs by giving people particular patterns of retinal data and recording what they see. In the same way, we can give babies and young children patterns of statistical data and record what they learn.

When trained scientists do statistics, we make certain very general assumptions about what the underlying causal structure of the world is like, and how that structure leads to particular patterns of data. The data we consider are patterns of dependence and independence among variables. Just looking at a single dependency between two variables may not tell us a great deal about causal structure, just as looking at a small piece of a picture won't tell us much about a spatial scene. But by looking at the entire pattern of dependence and independence among several types of variables, we can zero in on the right causal structure, and eliminate incorrect hypotheses. Sometimes we can even use these patterns to add to the vocabulary of the theory. For instance, if we find otherwise unexplained dependencies between two variables, we may decide that there is a hidden unobserved variable that influences them both. Recently, philosophers of science, computer scientists, and statisticians working with what is called the Bayes net formalism have begun to provide a precise mathematical account of these kinds of inferences (see Clark Glymour's essay in this issue).

It turns out that even very young babies, as young as eight months old, are sensitive to patterns of dependency. We can play babies strings of syllables in various probabilistic combinations with particular patterns of dependency – for example, 'ba' may usually precede 'da,' but rarely precede 'ga.' The babies can use these patterns of probabilities to infer which combinations of syllables are likely to occur together, and they can also detect similar statistical patterns among musical tones or aspects of a visual scene. Babies also seem able to map those probabilities onto representations of the external world. They don't, for example, just notice that certain syllables tend to go together; they assume that these regularities occur because these combinations of syllables constitute words in the language they hear around them. In the example above, they would assume that 'bada' is more likely to be a word than 'baga.'[11]

We have shown that, at least by the time they are two and a half, children can also use patterns of conditional probability to make genuinely causal inferences. To do this, we show children a machine called the blicket detector. The machine is a square box that lights up and plays music when particular blocks are placed on top of it. The blocks are all different from one another, so the job for children is to identify which blocks are blickets, that is, which blocks will cause the machine to light up. We can present the children with quite complex patterns of contingency between the activation of the detector and various combinations of blocks. We can ask them which blocks are blickets, and we can ask them to activate the machine or get it to stop. And their answers are almost always correct. They make the right inferences about the causal powers of the blocks. They make the sort of statistical inferences a scientist would make and, according to the Bayes net formalism, should make. In similar experiments, we can even show that children postulate unobserved variables to deal with otherwise inexplicable patterns of data.[12]

In order to make inferences about the causal structure of the world and causal relations among variables, the scientist performs experiments. The scientist intentionally intervenes on a variable in the world, forcing it to have a particular value and then observing what happens to the values of other variables. Again Bayes nets provide a precise mathematical account of such inferences.

In a similar way, even the youngest babies are particularly sensitive to the consequences of their interventions on

11 Richard N. Aslin, Jenny R. Saffran, and Elissa L. Newport, "Computation of Conditional Probability Statistics by 8-Month-Old Infants," *Psychological Science* 9 (4) (July 1998): 321 – 324.

12 Alison Gopnik, Clark Glymour, David Sobel, Laura Schulz, Tamar Kushnir, and David Danks, "A Theory of Causal Learning in Children: Causal Maps and Bayes-Nets," *Psychological Review* 111 (1) (2004): 1 – 30.

the world. For example, with a ribbon we can attach a mobile to a three-month-old baby's leg; the baby will regard her influence over the mobile with fascination, systematically exploring the contingencies between various limb movements and the movements of the mobile.[13] By the time they are a year old, babies will systematically vary the kinds of actions they perform on objects, as they simultaneously observe the consequences of those actions. And they may watch the further consequences of the action 'downstream' and use that information to design new actions. Give a one-year-old a set of blocks and you can see her trying different combinations, placements, and angles, and gauging which of these will produce stable towers and which will end in equally satisfying crashes.

We have shown that by the time children are four they will intervene in the world in a way that lets them uncover causal structure. My student Laura Schulz's gear toy tests show how children learn about causal structure. This toy, like the blicket detector, presents children with a new causal relation that they must infer from evidence about contingencies. It is a square box with two gears on top and a switch on the side. When you flip the switch the gears turn simultaneously. If you remove gear A and then flip the switch, B turns by itself; if you remove gear B and flip the switch, A doesn't turn. With both of these pieces of evidence you can conclude that B is making A move. We tell the children that one of the gears makes the other one move, and then leave them alone with the toy and a hidden camera. The children swiftly produce the right set of experimental interventions with gear and switch to determine which gear moves the other.

Of course these observations will not surprise anyone who has spent much time with infants or young children, who are perpetually 'getting into things.' In this sense, we may think of toddlers as causal learning machines. They are small human versions of the Mars rovers that roam about getting into things on the red planet – except that children are also mission control, interpreting the data they collect.

Somewhere between statistical observation and active experimentation, scientists and babies alike learn from the interventions of others. Scientists read journals, go to talks, hold lab meetings, and visit other labs – and all those conferences surely have some function beyond assortative mating. We scientists make the assumption that the interventions of others are like our own interventions, and that we can learn similar things from both sources.

By at least nine months, human infants seem to make the same assumption. For example, in one study babies see an experimenter enter the room and touch the top of his head to a box that then lights up. A day later, babies return to the room, see the box, and then immediately touch their heads against the top of it.[14]

We have shown that by four, children can use information about the interventions of others appropriately to make new causal inferences. Consider the gear toy experiment described above. Children will also solve this task if they simply see an adult perform the right experiments on the toy. They not only learn

13 Carolyn Rovee-Collier and Rachel Barr, "Infant Learning and Memory," in Gavin Bremner and Alan Fogel, eds., *Blackwell Handbook of Infant Development* (Malden, Mass.: Blackwell Publishers, 2001), 139 – 168.

14 Andrew N. Meltzoff, "Infant Imitation and Memory: Nine-Month-Olds in Immediate and Deferred Tests," *Child Development* 59 (1) (1988): 217 – 225.

about the causal consequences of adult actions, but also about the causal relations among the objects upon which adults perform those actions.

Indeed, the three techniques of causal inference that I have described – analyzing statistics, performing experiments, and watching the experiments of others – may give both scientists and children their extraordinary learning powers. Elements of the first two techniques are probably in place even in nonhuman animals. In classical conditioning, animals calculate dependencies among particularly important events, like shock and food. In operant conditioning, animals calculate the consequences of their actions. This is not surprising given the importance of causal knowledge for survival.

However, as Mike Tomasello and Danny Povinelli point out in this issue, there is much less clear evidence of the third type of learning – learning from the actions of others – in other animals. And there is no evidence that other animals combine all three types and assume that they provide information about the causal structure of the external world. By contrast, human children, at least by age three or four, do seem to put these types of information together in this way. This ability may, in fact, be one of the crucial abilities that give human beings their unique intellectual capacities. It allows them to learn far more about the world around them than other animals, and to use that knowledge to change the world.

My guess is that many of the mistakes that children and adults make in learning don't happen because they make the wrong deductions from assumptions and evidence, but rather because they make assumptions that are unwarranted under the particular circumstances.

For example, children tend to assume that the samples of evidence they collect are representative of the data. Similarly, they seem to assume that their own actions and the actions of others have all the formal characteristics of an ideal experimental intervention. The self-conscious methodological canons of formal science – the courses on statistics and experimental design – are intended to make these assumptions explicit rather than implicit and so ensure that they are correct in particular cases. For children, however, the assumptions may be close enough to the truth most of the time, and the evidence may be sufficiently rich, so that they mostly get things right anyway.

If we want children, and lay adults, to understand and appreciate science, we may need to make more connections between their intuitive and implicit causal inference methods and the self-conscious and explicit use of these methods in science. We may need, literally, a sort of scientific consciousness-raising.

Popper's quarrel with Wittgenstein reflected a larger argument between the view that science and philosophy tell us new things about the world, and the view that all they do is reflect social arrangements and linguistic conventions.

If we could put children in touch with their inner scientists, we might be able to bridge the divide between everyday knowledge and the apparently intimidating and elite apparatus of formal science. We might be able to convince them that there is a deep link between the realism of everyday life and scientific realism. And if we were able to do that, then we might win Popper's argument for him – without having to resort to pokers.

Daniel John Povinelli

Behind the ape's appearance: escaping anthropocentrism in the study of other minds

Look at Megan. Not just at her distinctively chimpanzee features – her accentuated brow ridge, her prognathic face, her coarse black hair – but at the totality of her being: her darting eyes, her slow, studied movements, the gestures she makes as her companion, Jadine, passes nearby. Can there be any doubt that behind certain obvious differences in her appearance resides a mind nearly identical to our own? Indeed, is it even possible to spend an afternoon with her and *not* come to this conclusion? Upon reflection, you will probably acknowledge that her mind is not identical to ours. "But surely it's not qualitatively different, either," you will still insist. "I mean, it's obvious from watching her that we share the same *kind* of mind."

Faced with the overwhelming similarity in the spontaneous, everyday behavior of humans and chimpanzees, how can someone like me – someone who has dedicated his life to studying these remarkable animals – entertain the possibility that their minds are, in profound respects, radically different from our own? How can I challenge the received wisdom of Darwin – confirmed by my own initial impressions – that the mental life of a chimpanzee is best compared to that of a human child?

Actually, it's easy: I have learned to have more respect for them than that. I have come to see that we distort their true nature by conceiving of their minds as smaller, duller, less talkative versions of our own. Casting aside these insidious assumptions has been difficult, but it has allowed me to see more clearly that the human mind is not the gold standard against which other minds must be judged. For me it has also illuminated the possibility of creating a science that is less contaminated by our deeply anthropocentric intuitions about the nature of other minds.

The best available estimates suggest that humans and chimpanzees originated from a common ancestor about five or six million years ago.[1] This is reflected

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1 Galina V. Galzko and Masatoshi Nei, "Estimation of Divergence Times for Major Lineages of Primate Species," *Molecular Biology and Evolution* 20 (2003): 424 – 434.

in estimates of our genetic similarity: we share, on average, about 98.6 percent of our total nucleotide sequence in common. This statistic seems impressive. After all, such biological affinity would appear to be the final nail in the coffin of the notion that there could be any radical mental differences between them and us: if chimpanzees and humans share 98.6 percent of their genetic material, then doesn't it follow that there ought to be an extraordinarily high degree of mental similarity as well? This idea has been paraded so frequently through the introductory paragraphs of both scholarly journal articles and the popular press alike that it has come to constitute a melody of sorts; an anthem that if not sung raises doubts as to one's allegiance to the cause of defending the chimpanzee's dignity.

But what does this 98.6 percent statistic really mean? It should be of immediate interest that it is almost invariably misreported. We do not share 98.6 percent of our genes in common with chimpanzees; we share 98.6 percent of our nucleotide sequence. A single nucleotide difference in a string of four hundred may code for a different allele. Furthermore, as the geneticist Jonathan Marks has pointed out in lucid detail, the 98.6 percent statistic has so little grounding in the average mind that confronts it, as to render it essentially meaningless.[2] We might, after all, share 50 percent of our nucleotide sequences in common with bananas and broccoli. But what on earth does it mean to say that we are 50 percent the same as a vegetable? I don't know about you, but I doubt my mind is 50 percent identical to that of the garden pea. And so what would it mean, exactly, if we discovered that our minds were 75 percent chimpanzee?

No, such coarse genetic comparisons will hardly suffice to help us understand the complex similarities and differences that exist between the mental lives of humans and chimpanzees. However, in a climate where certain highly visible experts have radically anthropomorphized chimpanzees,[3] such statistics are heralded as establishing once and for all that chimpanzees are, at the very least, mentally equivalent to two- or three-year-old human children, and should therefore be granted human rights.[4]

A few obvious biological facts may be worth noting here. To begin, it was the human lineage, not the chimpanzee one, that underwent radical changes after our respective geneologies began to diverge from their common ancestor. Since this split, humans have resculpted their bodies from head to toe – quite literally, in fact; as our lineage became bipedal, the pelvis, the knee, and the foot were all drastically reshaped, with modifications in the hand (including new muscles) soon following. To top it all off, we ultimately tripled the size of our brain, with disproportionate increases probably occurring in the seat of higher cognitive function, the prefrontal cortex. Oh yes, and at some point during all of this (no one knows exactly when), natural language – perhaps the most notice-

2 Jonathan Marks, *What It Means to Be 98% Chimpanzee* (Berkeley: University of California Press, 2002).

3 For examples, see Sue Savage-Rumbaugh, *Kanzi: The Ape at the Brink of the Human Mind* (New York: John Wiley & Sons, 1994); Jane Goodall, *Through a Window* (Boston: Houghton Mifflin, 1990); Roger Fouts, *Next of Kin* (New York: William Morrow and Co., 1997).

4 Steven M. Wise, *Rattling the Cage: Toward Legal Rights for Animals* (Cambridge, Mass.: Perseus Books, 2000); Paola Cavalieri and Peter Singer, eds., *The Great Ape Project: Equality Beyond Humanity* (New York: St. Martin's Press, 1993).

able of human adaptations – emerged as well.

In contrast, chimpanzees have probably changed relatively little from the common ancestor they shared with us about five million years ago. Indeed, of all of the members of the great ape/human group who shared a common ancestor about fifteen million years ago, none, indeed, has diverged as much as humans. A simple thought experiment may help to put this point into perspective: line up all of the species in question – gorillas, orangutans, chimpanzees, bonobos, humans – and one of them immediately stands out. Guess which one?

In fact, the more we compare humans and chimpanzees, the more the differences are becoming apparent. Even geneticists are starting to catch up with the reality of these differences. New research has shown that rough similarity in our nucleotide sequences obscures the fact that the same genes may have dramatically different activity levels in the two species. So even where humans and chimpanzees share genes in common, it turns out that there are what can only be described as major differences in gene *expression* – that is, whether, when, and for how long genes are actually working to produce the proteins for which they code.[5] This is the *real* stuff of genetic comparison, and it casts our crude genetic similarity to the garden pea in a wholly different light.

What makes these differences in gene expression significant is that they ultimately manifest themselves as differences in the bodies – including the brains – of humans and chimpanzees. So, exactly how similar are the brains of humans and chimpanzees? After all, if we knew *that*, couldn't we directly address the question of their mental similarity? Well, it would be a start, anyhow. Unfortunately, comparisons of the brains of humans and apes have traditionally been limited to gross considerations such as size and surface features (such as lobes and sulcus patterns). Remarkably, the details of the internal organization of human and great ape brain systems and structures have been largely ignored, in part because it's so difficult to study these brains, but also because most neuroscientists have frequently assumed that despite great differences in size, all mammalian brains are organized pretty much the same.

Fortunately, even this is beginning to change. For example, Todd Preuss, working at the University of Louisiana, recently made a startling discovery while comparing the brains of humans and chimpanzees. Turning his attention away from the frontal lobes, his previous area of research, Preuss decided to take a look at the primary visual cortex (V1), the area of the cerebral cortex that is the first way station into the processing of visual information. The organization of this area of the brain has been assumed to be nearly identical across primates. But there, in the middle of V1, Preuss and his colleagues uncovered a distinctively human specialization – a kind of neural architecture not found even in chimpanzees.[6] Preuss speculates that this specialization involves modifications of the pathways related to spatial vision and motion processing. But, regardless of what it is for, it suggests that

5 Wolfgang Enard et al., "Intra- and Interspecific Variation in Primate Gene Expression Patterns," *Science* 296 (2002): 341 – 343; Mario Cáceres et al., "Elevated Gene Expression Levels Distinguish Human from Non-Human Primate Brains," *Proceedings of the National Academy of Sciences* 100 (2003): 13030 – 13035.

6 Todd M. Preuss et al., "Distinctive Compartmental Organization of Human Primary Visual Cortex," *Proceedings of the National Academy of Sciences* 96 (1999): 11601 – 11606.

we need to rethink brain evolution in a way that's consistent with neo-Darwinian theory: similarity and difference among species as comfortable bedfellows; a state of affairs accomplished by weaving in new systems and structures alongside the old. "If we find such differences in the middle of the primary visual cortex," Preuss recently remarked to me, "just imagine what we're going to find when we start looking elsewhere."

Some may be surprised (or even afraid) to learn of such differences between humans and our nearest living relatives. After several decades of being fed a diet heavy on exaggerated claims of the degree of mental continuity between humans and apes, many scientists and laypersons alike now find it difficult to confront the existence of radical differences. But then, in retrospect, how viable was the idea of seamless mental continuity in the first place? After all, it tended to portray chimpanzees as watered-down humans, not-quite-finished children. Despite the fact that aspects of this notion can be traced straight to Darwin, it is an evolutionarily dubious proposition, to say the least.

If there are substantial differences between the mental abilities of humans and chimpanzees, in what areas are they likely to exist? Over the past couple of thousand years, many potential rubicons separating human and animal thinking have been proposed. Some of these have been particularly unhelpful, such as the radical behaviorists' forgettable proposition that animals don't 'think' at all (of course, these behaviorists were even skeptical about the existence of human thought!). And, unfortunately, in the popular imagination the question still appears to be, "*Can* animals think?"[7] as opposed to, "How does thinking differ across species?" (the latter being a decidedly more evolutionarily minded question).

Assuming that chimpanzees and other species have mental states (a point I take for granted), it seems to me that a more productive question to ask is, "What are their mental states about?" Or, put another way, "What kinds of concepts do they have at their disposal?" It would stand to reason that the mental states of chimpanzees, first and foremost, must be concerned with the things most relevant to their natural ecology – remembering the location of fruit trees, keeping an eye out for predators, and keeping track of the alpha male, for instance. And so surely chimpanzees form concepts about concrete things – things like trees, facial expressions, threat vocalizations, leopards, and the like. But what about more abstract concepts? Concepts like ghosts, gravity, and God?

Admittedly, to use the term 'concept' as loosely as I have will require the indulgence of certain scholars. But perhaps some progress can be made by noting that every concept is at least somewhat abstract if it extends beyond a particular example. For instance, if one has a notion of an apple that is not limited to a single instance of that apple, then one has made a generalization, and thus a kind of abstraction. Given that it has been known for decades or more that chimpanzees and many other species form such abstractions,[8] this cannot be a defining feature of human thinking.

7 Eugene Linden, "Can Animals Think?" *Time*, 22 March 1993.

8 Suzette L. Astley and Edward A. Wasserman, "Object Concepts: Behavioral Research with Animals and Young Children," in William T. O'Donohue, ed., *Learning and Behavior Therapy* (Boston: Allyn and Bacon, 1997), 440 – 463; Tom R. Zentall, "The Case for a Cognitive Approach to Animal Learning and Behavior," *Behavioural Processes* 54 (2001): 65 – 78.

At the risk of oversimplification, let me instead propose a distinction between concepts that refer to objects and events that can be directly observed (that is, things that can be detected by the unaided senses), versus hypothetical entities and processes (things that are classically unobservable). Thus, I wish to separately consider all concepts that refer to theoretical things: all the things that are not directly registered by the senses, but are merely posited to exist on the basis of things we can observe.

Such concepts permeate our commonsense way of thinking: we explain physical events on the basis of things like 'forces' (supernatural or otherwise) that we have never actually witnessed, and account for the behavior of other humans on the basis of mental states we have never seen (e.g., their beliefs, desires, and emotions). These concepts serve as the bedrock for some of our most fundamental explanations for why the world works the way it does.

Meanwhile, we can directly contrast these sorts of concepts with ones that are derived from things that can be directly observed: apples and oranges, trees, flashes of lightning, facial expressions – even the raising of a hand or the sound of a train whistle blowing in the distance. Concepts about these things share at least one property in common: they are all derived from the world of macroscopic entities with which the primary senses directly interact. Without additional justification, I am therefore asserting a distinction between concepts that refer to observable objects and events, and ones that refer to strictly hypothetical ones.

So, here's a proposal: the mental lives of humans and chimpanzees are *similar*, in that both species form innumerable (and in many cases, identical) concepts about observable things; but, at the same time, are *radically different*, in that humans form additional concepts about inherently unobservable things.[9]

Now, I realize that most people would not be surprised if it were established beyond doubt that chimpanzees lack a concept of God. But what about other, seemingly more prosaic concepts that infest our way of thinking about the world? Consider the way in which we think about the social realm. In interacting with each other (and with animals, for that matter), we use a dual system of representation: we understand other beings both as part of the observable world (they engage in particular movements of their hands and feet, and their lips form particular contortions as sounds emerge from their mouths), and as entities with mental properties – unobservable attributes like emotions, intentions, desires, and beliefs.

The proposal is that, in contrast to humans, chimpanzees rely strictly upon observable features of others to forge their social concepts. If correct, it would mean that chimpanzees do not realize that there is more to others than their movements, facial expressions, and habits of behavior. They would not understand that other beings are repositories of private, internal experience. They would not appreciate that in addition to things that go on in the observable world, there are forever hidden things that go on in the private life of the mind. It would mean that chimpanzees do not reason about what others think, believe, and feel – precisely because they do not form such concepts in the first place.

9 This discussion extends several previous descriptions of this hypothesis, for example, my article with Jesse Bering and Steve Giambrone, "Toward a Science of Other Minds: Escaping the Argument by Analogy," *Cognitive Science* 24 (2000): 509 – 541.

Before we get too much further, let me be honest: I recognize that this proposal has troubling implications. For one thing, if chimpanzees do not reason about unobservable entities, then we would frequently need distinctly different explanations for human and chimpanzee behavior – even in situations where the behavior looks almost identical. Mind you, we would not need completely different explanations, just ones that are distinctive enough to capture the proposed difference. Nonetheless, each time we witnessed a chimpanzee engage in a complex social behavior that resembles our own, we would have to believe that, unlike us, the chimpanzee has only one conceptual system for encoding and reasoning about what is happening: a system that invokes concepts derived from observable features of the world. Thus, when chimpanzees deceive each other (which they do regularly), they would never be trying to manipulate what others believe, nor what others can see or hear, for constructs like 'believing,' 'seeing,' and 'hearing' are already deeply psychological. No, in deciding what to do, the chimpanzee would be thinking and reasoning solely about the abstracted statistical regularities that exist among certain events and the behaviors, postures, and head movements (for example) of others – what we have called 'behavioral abstractions.'[10]

I should note that humans, too, rely heavily upon behavioral abstractions in their day-to-day interactions. We *must* be doing so: otherwise upon what basis could we attribute additional, psychological states to others? First, we recognize the turn of the head and the direction of the eyes (observable features), then we ascribe the internal experience of 'seeing' (unobservable feature). So, the proposal isn't that chimpanzees use one system and humans use another; both species are purported to rely upon concepts about the observable properties of others. Instead, the proposal is that chimpanzees don't form additional concepts about the *un*observable properties of other beings (or the world in general, for that matter).

So, at face value, the proposal I have made is worrying. In interpreting what would appear to be the exact same behaviors in humans and chimpanzees in different ways, I seem to be applying a double standard.

But is this implication really problematic, or does it just seem problematic because it runs counter to some of our most deeply engrained – but fundamentally flawed – ways of thinking?

Assume, for a moment, that you have traveled back in time to a point when there were no chimpanzees on this planet – and no humans, either. Imagine further that you have come face to face with members of the last common ancestor of humans and chimpanzees. Let's stipulate that these organisms are intelligent, thinking creatures who deftly attend to and learn about the regularities that unfold in the world around them. But let us also stipulate that they do not reason about unobservable things; they have no ideas about the 'mind,' no notion of 'causation.'

As you return to your time machine and speed forward, you will observe new lineages spring to life from this common ancestor. Numerous ape-like species will emerge, then disappear. As you approach the present day, you will even witness the evolutionary birth of modern orangutans, chimpanzees, and gorillas. But amid all of this your attention will be drawn to one particular offshoot of this

10 Daniel J. Povinelli and Jennifer Vonk, "Chimpanzee Minds: Suspiciously Human?" *Trends in Cognitive Science* 7 (2003): 157 – 160.

process, a peculiar genealogy that buds off numerous descendent species. This particular lineage has evolved an eye-catching trick: it habitually stands upright; it walks bipedally. And some of its descendants build upon this trick, capitalizing upon the new opportunities it offers. For reasons that we may never fully know, tool use and manufacture increase exponentially, language emerges, brain size triples, and, as more time passes, human material and social culture begins to accrete upon the shoulders of the lineage's last surviving member: *Homo sapiens sapiens*. Now, imagine that as part of this process, this lineage evolved new conceptual structures (intimately connected to the evolution of language) that allow them to reason about things that cannot be observed: mental states, physical forces, spiritual deities.

I have stipulated all of this so we can confront the following question: If evolution proceeded in this quite plausible manner, then how would we expect the spontaneous, everyday behavior of humans to compare to that of chimpanzees? The answer, I think, is that things would look pretty much the way they do now. After all, humans would not have abandoned the important, ancestral psychological structures for keeping track of other individuals within their groups, nor jettisoned their systems for noticing that something very different happens when Joe turns his head toward so-and-so, just depending on whether or not his hair is standing on end. No, in evolving a new psychological system for reasoning about hypothetical, internal mental states, humans would not have (indeed, could not have!) abandoned the ancient systems for reasoning about observable behavior. The new system by definition would depend upon the presence of older ones.

Now, is it really troubling to invoke a different explanation for what on the surface seem to be identical units of behavior in humans and chimpanzees? If the scenario I have outlined above is correct, then the answer must be, no. After all, for any given ability that humans and chimpanzees share in common, the two species would share a common set of psychological structures, which, at the same time, humans would augment by relying upon a system or systems unique to our species. The residual effect of this would manifest itself in numerous ways: some subtle (such as tightly constrained changes in the details of things to which our visual systems attend), others more profound (such as the creation of cultural artifacts like the issue of *Dædalus* in which you are now reading these words).

Behind the ape's appearance

So much for theory. What about the empirical evidence; does it support the proposal I have just offered? Although it will not surprise you to learn that I think it does, I have not always been of this opinion; I used to believe that any differences between humans and chimpanzees would have to be trivial. But the results of over two hundred studies that we have conducted during the past fifteen years have slowly changed my mind. Combined with findings from other laboratories, this evidence has forced me to seriously confront the possibility that chimpanzees do not reason about inherently unobservable phenomena.

Let me briefly illustrate this evidence with three simple examples: one from the social domain, one from the domain of physics, and one from the domain of numerical reasoning.

First, what does the experimental evidence suggest about whether chimpanzees reason about mental states? Although the opinions of experts differ (and have swung back and forth over the past several years), I believe that at present there is no direct evidence that chimpanzees conceive of mental states, and considerable evidence that they do not. As an example, consider the well-studied question of whether chimpanzees reason about the internal, visual experiences of others, that is, of whether they know anything about 'seeing.'

To begin, no one doubts that chimpanzees respond to, reason about, and form concepts related to the movements of the head, face, and eyes of others; these are aspects of behavior that can be readily witnessed.[11] But what about the idea that another being 'sees' things, that others are loci of unobservable, visual experiences?

Over the past ten years we have conducted dozens of studies of juvenile, adolescent, and adult chimpanzees to explore this question. Perhaps the most straightforward of these studies involved examining how chimpanzees understand circumstances under which others obviously can or cannot see them.[12] In these studies, chimpanzees were exposed to a routine in which they would approach a familiar playmate or caretaker to request a food treat using their species-typical begging gesture. Simple enough. But on the crucial test trials, the chimpanzees were confronted with two individuals, only one of whom could see them. For example, in one condition, one caretaker had a blindfold covering her mouth, whereas the other had a blindfold covering her eyes. The question was to whom would the chimpanzee gesture.

Not surprisingly, in our trials with human children, even two-year-olds gestured to whoever had the blindfold over her mouth (versus the eyes), probably because they could represent her inner, psychological state ("She can *see* me!"). In striking contrast, our chimpanzees did nothing of the kind. Indeed, in numerous studies, our chimpanzees gave virtually no indication that they could understand 'seeing' as an internal experience of others.

With enough trials of any given condition the chimpanzees were able to *learn* to select whoever was able to see them; after enough trials of not being handed a banana when gesturing to someone with a bucket over her head, the chimpanzees figured out to gesture to the other person. Did this mean that they had finally discerned what we were asking them? In numerous transfer tests in which we pitted the idea that the chimpanzees were learning about the observable cues (i.e., frontal posture, presence of the face or eyes) against the possibility that on the basis of such cues they were reasoning about who could 'see' them, the chimpanzees consistently insisted (through their behavior) that they were reasoning about observable features, not internal mental states, to guide their choices.

In addition to what they learned in these tests, it also became apparent that chimpanzees come pre-prepared, as it

11 See Daniel J. Povinelli and Timothy J. Eddy, "Chimpanzees: Joint Visual Attention," *Psychological Science* 7 (1996): 129 – 135; Shoji Itakura, "An Exploratory Study of Gaze-Monitoring in Nonhuman Primates," *Japanese Psychological Research* 38 (1996): 174 – 180; Michael Tomasello, Brian Hare, and Josep Call, "Five Primate Species Follow the Visual Gaze of Conspecifics," *Animal Behaviour* 58 (1998): 769 – 777.

12 Our laboratory's empirical research of chimpanzees' understanding of 'seeing' has been summarized in my article, "The Minds of Humans and Apes are Different Outcomes of an Evolutionary Experiment," in Susan M. Fitzpatrick and John T. Bruer, eds., *Carving Our Destiny: Scientific Research Faces a New Millennium* (Washington, D.C.: National Academy of Sciences and John Henry Press, 2001), 1 – 40.

were, to make sense of certain postures. For instance, in our tests they immediately knew what to do when confronted with someone facing them versus someone facing away, and this finding has been replicated in several other laboratories.[13] "But if they make *that* distinction," you wonder, "then why do they perform so differently on the other tests? Is it just because they're confused? How are we to make sense of such a puzzling pattern of findings?"

Actually, these results are not puzzling at all if the ability to reason about mental states evolved in the manner that I suggested earlier – that is, if humans wove a system for reasoning about mental states into an existing system for reasoning about behavior. After all, if the idea is correct, then chimpanzees may well be born predisposed to attend to certain postures and behaviors related to 'seeing' – even though they know nothing at all about such mental states *per se* – precisely because overt features of behavior are the tell-tale indicators of the future behavior of others. But when such features are carefully teased apart to probe for the presence of a mentalistic construal of others, the chimpanzees stare back blankly: this is not part of their biological endowment. Thus, if the evolutionary framework I have sketched is correct, neither the chimpanzees nor the results are 'confused'; that epithet may fall squarely upon the shoulders of we human experimenters and theorists who are so blinded by our own way of understanding the world that we are not readily open to the chimpanzee's way of viewing things.

Of course, some have challenged this conclusion, arguing that we need to turn up the microscope and develop more tests that will allow chimpanzees to express their less well-developed understanding of such concepts.[14] So, for example, researchers at Emory University recently conducted tests in which a dominant and a subordinate chimpanzee were allowed to fight over food that was positioned in an enclosure between them.[15] On the critical trials, two pieces of food were positioned equidistant from the animals. The catch was that one piece of food was placed behind an opaque barrier so that only the subordinate could see it. The researchers report that when the subordinate was released into the enclosure, he or she tended to head for the food that was hidden from the dominant's view, suggesting, perhaps, that the subordinate was modeling the visual experience of his or her dominant rival.

But do such tests really help?[16] Do they reveal some weaker understanding

13 For example, see Autumn B. Hostetter et al., "Differential Use of Vocal and Gestural Communication by Chimpanzees (*Pan troglodytes*) in Response to the Attentional Status of a Human (*Homo sapiens*)," *Journal of Comparative Psychology* 115 (2001): 337 – 343.

14 Michael Tomasello et al., "Chimpanzees Understand Psychological States – The Question Is Which Ones and to What Extent," *Trends in Cognitive Science* 7 (2003): 153 – 156, esp. 156.

15 Brian Hare et al., "Chimpanzees Know What Conspecifics Do and Do Not See," *Animal Behaviour* 59 (2000): 771 – 785; see also M. Rosalyn Karin-D'Arcy and Daniel J. Povinelli, "Do Chimpanzees Know What Each Other See? A Closer Look," *International Journal of Comparative Psychology* 15 (2002): 21 – 54.

16 In a recent analysis of the diagnostic potential of these and other tests, Jennifer Vonk and I (see footnote 10) argued that the logic of current tests with chimpanzees (and other animals) cannot, in principle, provide evidence that uniquely supports the notion that they are reasoning about mental states (as opposed to behavior alone), and we advocated a new paradigm of tests that may have such diagnostic power. An alternative point of view is provided in the companion piece by Tomasello and col-

of mental states in chimpanzees? These are precisely the situations in which chimpanzees will be evolutionarily primed to use their abilities to form concepts about the actions of others to guide their social behavior. So, for example, they can simply know to avoid food that is out in the open when a dominant animal is about to be released. "But still," the skeptic within you asks, "that's pretty smart, isn't it? The chimpanzees would have to be paying attention to who's behind the door, and what that other individual is going to do when the door opens, right?"

Fair enough. But that, in the end, is the point: chimpanzees can be intelligent, thinking creatures even if they do not possess a system for reasoning about psychological states like 'seeing.' If it turns out that this is a uniquely human system, this should not detract from our sense of the evolved intelligence of apes. By way of analogy, the fact that bats echolocate but humans don't, hardly constitutes an intellectual or evolutionary crisis.

In the final analysis, the best theory will be the one that explains both data sets: the fact that chimpanzees reason about all the observable features of others that are associated with 'seeing' – and yet at the same time exhibit a striking lack of knowledge when those features are juxtaposed in a manner that they have never witnessed before (i.e., blindfolds over eyes versus over the mouth). I submit that, at least for the time being, the evolutionary hypothesis I have described best meets this criterion.

leagues. However, I believe that this view dramatically underestimates the representational power of a psychological system that forms concepts solely about the observable aspects of behavior.

Table 1
Theoretical causal constructs and their observable 'ambassadors'

Theoretical concept	Paired observable 'ambassador'
gravity	*downward object trajectories*
transfer of force	*motion-contact-motion sequences*
strength	*propensity for deformation*
shape	*perceptual form*
physical connection	*degree of contact*
weight	*muscle/tendon stretch sensations*

A second example of the operation of what may be a uniquely human capacity to reason about unobservables comes from comparisons of humans' and chimpanzees' commonsense understanding of physics. Humans – even very young children – seem disposed to assume that there's more to the physical world than what meets the eye. For example, when one ball collides with another, stationary one, and the second speeds away, even quite young children are insistent that the first one caused the second to move away. Indeed, as Michotte's classic experiments revealed, this seems to be an automatic mental process in adult humans.[17] But what is it, exactly, that humans believe *causes* the movement of the second ball? As Hume noted long ago, they do not merely recognize that the objects touched; that's just a re-description of the observed events.[18] No, the first one is seen as hav-

17 Albert Michotte, *The Perception of Causality* (New York: Basic Books, 1963).

18 David Hume, *Treatise of Human Nature*, vols. 1–2, ed. A. D. Lindsay (London: Dent, 1739; 1911).

ing transmitted something to the second object, some kind of 'force.' But where is this force? Can it be seen? No, it is a theoretical thing.

In an initial five-year study of 'chimpanzee physics,' we focused our apes' attention on simple tool-using problems.[19] Given their natural expertise with tools, our goal was to teach them how to solve simple problems – tasks involving pulling, pushing, poking, etc. – and then to use carefully designed transfer tests to assess their understanding of why the tool objects produced the effects they did. In this way, we attempted to determine if they reason about things like gravity, transfer of force, weight, and physical connection, or merely form concepts about spatio-temporal regularities. To do so, we contrasted such concepts with their perceptual 'ambassadors' (see table 1), much in the same way that we had contrasted the unobservable psychological state of 'seeing' against the observable behavioral regularities that co-vary with 'seeing.'

To pick just one example: we explored in detail the chimpanzee's understanding of physical connection – of the idea that two objects are bound together through some unobservable interaction such as the force transmitted by the mass of one object resting on another, or the frictional forces of one object against another; or conversely, the idea that simply because two objects are physically touching does not mean there is any real form of 'connection.' We presented our chimpanzees with numerous problems, but consider one test in which we first taught them to use a simple tool to hook a ring in order to drag a platform with a food treat on it toward them. Although they learned to do so, our real question was whether, when confronted with two new options, they would select the one involving genuine physical connection as opposed to mere 'contact.' Consistent with our findings in other tests, they did not. Instead, 'perceptual contact' seemed to be their operating concept. The observable property of contact (of any type) was generally sufficient for them to think that a tool could move another object.

Finally, consider the chimpanzee's numerical understanding. Over the past decade or so, it has become apparent that many species share what Stanislas Dehaene has called a 'number sense' – the ability to distinguish between larger and smaller quantities, even when the quantities being compared occupy identical volumes.[20]

In an attempt to explore the question of numerical reasoning in animals, several research laboratories have trained apes to match a specific quantity of items (say, three jelly beans) with the appropriate Arabic numeral.[21] That they can accomplish this should not be the least bit surprising: humans and chimpanzees (and many other species) share the ability to visually individuate objects. After extensive training, furthermore, the most apt of these pupils have gone on to exhibit some understanding of *ordinality* (the idea that 5 represents a

19 Daniel J. Povinelli, *Folk Physics for Apes* (Oxford: Oxford University Press, 2000).

20 Stanislas Dehaene, *The Number Sense* (Oxford: Oxford University Press, 1997).

21 For this discussion, I rely heavily on the detailed results from Ai, a twenty-five-year-old chimpanzee whose numerical abilities have been studied since she was five by a team led by Tetsuro Matsuzawa in Kyoto, Japan. See Dora Biro and Tetsuro Matsuzawa, "Chimpanzee Numerical Competence: Cardinal and Ordinal Skills," in Tetsuro Matsuzawa, ed., *Primate Origins of Human Cognition and Behavior* (Tokyo: Springer, 2001), 199 – 225.

larger quantity than 4, for example). So, isn't this evidence that chimpanzees have a solid grasp of the notion of the number?

Let us scratch the surface a bit, to look at these findings from the perspective I have been advocating. First, do these chimpanzees possess a dual understanding of numbers – both as associates of real object sets and as inherently theoretical things – such that every successive number in the system is exactly '1' more than the previous number? The training data even from Ai, the most mathematically educated of all chimpanzees, suggests that they do not. For example, each time the next numeral was added into her training set, it took her just as long to learn its association with the appropriate number of objects as it took with the previous numeral. In other words, there appeared to be little evidence that Ai understood the symbols as anything other than associates of the object sets. Furthermore, even her dedicated mentors suggest that she was not 'counting' at all: with quantities of up to three or four objects, she performed like humans, using an automatic process ('subitizing') to make her judgments; but with larger quantities, instead of counting, it appears as if she was simply estimating 'larger' or 'smaller.'

What about ordinality? When first tested for her understanding of the relative ordering of numbers, Ai exhibited no evidence that this was part of her conceptual structure. That is, when presented with pairs of numbers, 1 versus 8, for example, she did not seem to have any notion that the value of 1 is smaller than the value of 8 – even though she had been correctly matching these numerals to object sets for years! Of course, after extended training, Ai did eventually exhibit evidence of this ability, and now, after more than fifteen years of training, when confronted with a scrambled array of the numerals 1 to 9, she has the remarkable ability to select them in ascending order.

But what does it mean that under the right training regime we can guide a chimpanzee like Ai into a performance that looks, in many but not all respects, like human counting? One possibility is that a basic number sense – a system grounded to individual macroscopic objects – is widespread among animals, and that apes (and other animals) can use this ability (in concert with their other cognitive skills) to figure out ways to cope with the 'rules' that humans establish in their tests. In contrast, the human system for counting (as well as other mathematical ideas) could be seen as building upon these older systems by reifying numbers as things in their own right – theoretical things. This may seem like a subtle and unimportant distinction for some tasks, but it may be one that leaves the ape mystified when facing questions that treat numbers as things in their own right.

As a striking example of the distinction I have been trying to draw, consider zero, surely one of the purest examples that exists of an inherently unobservable entity. If I am right, then zero ought to be virtually undetectable by the chimpanzee's cognitive system. And indeed, the data seem to bear this out.[22] For all of her training, even Ai does not appear to have learned to understand zero in this sense. True, she (and other animals) have quickly learned to pick the numeral 0 in response to the absence of objects (something easily explained by associative learning processes). But tests of or-

22 Dora Biro and Tetsuro Matsuzawa, "Use of Numerical Symbols by the Chimpanzee (*Pan troglodytes*): Cardinal, Ordinals, and the Introduction of Zero," *Animal Cognition* 4 (2001): 193 – 199.

dinality involving zero (choosing whether 0 is greater or lesser than 6, for example) have consistently revealed what I believe might be best described as the virtual absence of the concept. Although this training has gradually forced her 'understanding' of zero into a position further and further down the 'number line,' even to this day, after thousands of trials, Ai still reliably confuses 0 with 1 (and in some tasks, with 2 or 3 as well). However one wishes to interpret such findings, they are certainly not consistent with an understanding of the very essence of zero-ness.[23]

Our work together is done. To the best of my ability I have laid out the case for believing that chimpanzees can be bright, alert, intelligent, fully cognitive creatures, and yet still have minds of their own. From this perspective, it may be our species that is the peculiar one – unsatisfied in merely knowing *what* things happen, but continually driven to explain *why* they happen, as well. Armed with a natural language that makes referring to abstract things easy, we continually pry behind appearances, probing ever deeper into the causal structure of things. Indeed, some tests we have conducted suggest that chimpanzees may not seek 'explanations' at all.[24]

23 One might retort that the numeral 0 appeared quite late in human history. But here's a thought experiment. Return to our imaginary time machine (see above) and travel back to those civilizations that predate the invention of the numeral 0. How difficult would it be to teach those adult humans the position occupied by the symbol for zero?

24 Daniel J. Povinelli and Sarah Dunphy-Lelii, "Do Chimpanzees Seek Explanations? Preliminary Comparative Investigations," *Canadian Journal of Comparative Psychology* 55 (2001): 187 – 195.

Behind the ape's appearance

And yet I cannot help but suspect that many of you will react to what I have said with a feeling of dismay – perhaps loss; a sense that if the possibility I have sketched here turns out to be correct, then our world will be an even lonelier place than it was before. But for the time being, at least, I ask you to stay this thought. After all, would it really be so disappointing if our first, uncontaminated glimpse into the mind of another species revealed a world strikingly different from our own; or all that surprising if the price of admission into that world were that we check some of our most familiar ways of thinking at the door? No, to me, the idea that there may be profound psychological differences between humans and chimpanzees no longer seems unsettling. On the contrary, it's the sort of possibility that has, on at least some occasions, emboldened our species to reach out and discover new worlds with open minds and hearts.

Patricia Smith Churchland

How do neurons know?

My knowing *anything* depends on my neurons – the cells of my brain.[1] More precisely, what I know depends on the specific configuration of connections among my trillion neurons, on the neurochemical interactions between connected neurons, and on the response portfolio of different neuron types. All this is what makes me *me*.

The range of things I know is as diverse as the range of stuff at a yard sale. Some is knowledge how, some knowledge that, some a bit of both, and some not exactly either. Some is fleeting, some enduring. Some I can articulate, such as the instructions for changing a tire, some, such as how I construct a logical argument, I cannot.

Some learning is conscious, some not. To learn some things, such as how to ride a bicycle, I have to try over and over; by contrast, learning to avoid eating oysters if they made me vomit the last time just happens. Knowing how to change a tire depends on cultural artifacts, but knowing how to clap does not.

And *neurons* are at the bottom of it all. How did it come to pass that we know *anything*?

Early in the history of living things, evolution stumbled upon the advantages accruing to animals whose nervous systems could make predictions based upon past correlations. Unlike plants, who have to take what comes, animals are movers, and having a brain that can learn confers a competitive advantage in finding food, mates, and shelter and in avoiding dangers. Nervous systems earn their keep in the service of prediction, and, to that end, map the *me-relevant* parts of the world – its spatial relations, social relations, dangers, and so on. And, of course, brains map their worlds in varying degrees of complexity, and relative to the needs, equipment, and lifestyle of the organisms they inhabit.[2]

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1 Portions of this paper are drawn from my book *Brain-Wise: Studies in Neurophilosophy* (Cambridge, Mass.: MIT Press, 2002).

2 See Patricia Smith Churchland and Paul M. Churchland, "Neural Worlds and Real Worlds," *Nature Reviews Neuroscience* 3 (11) (November 2002): 903 – 907.

Thus humans, dogs, and frogs will represent the same pond quite differently. The human, for example, may be interested in the pond's water source, the potability of the water, or the potential for irrigation. The dog may be interested in a cool swim and a good drink, and the frog, in a good place to lay eggs, find flies, bask in the sun, or hide.

Boiled down to essentials, the main problems for the neuroscience of knowledge are these: How do structural arrangements in neural tissue embody knowledge (the problem of representations)? How, as a result of the animal's experience, do neurons undergo changes in their structural features such that these changes constitute knowing something new (the problem of learning)? How is the genome organized so that the nervous system it builds is able to learn what it needs to learn?

The spectacular progress, during the last three or four decades, in genetics, psychology, neuroethology, neuroembryology, and neurobiology has given the problems of how brains represent and learn and get built an entirely new look. In the process, many revered paradigms have taken a pounding. From the ashes of the old verities is arising a very different framework for thinking about ourselves and how our brains make sense of the world.

Historically, philosophers have debated how much of what we know is based on instinct, and how much on experience. At one extreme, the rationalists argued that essentially all knowledge was innate. At the other, radical empiricists, impressed by infant modifiability and by the impact of culture, argued that all knowledge was acquired.

Knowledge displayed at birth is obviously likely to be innate. A normal neonate rat scrambles to the warmest place, latches its mouth onto a nipple, and begins to suck. A kitten thrown into the air rights itself and lands on its feet. A human neonate will imitate a facial expression, such as an outstuck tongue. But other knowledge, such as how to weave or make fire, is obviously learned postnatally.

Such contrasts have seemed to imply that everything we know is either caused by genes or caused by experience, where these categories are construed as exclusive and exhaustive. But recent discoveries in molecular biology, neuroembryology, and neurobiology have demolished this sharp distinction between nature and nurture. One such discovery is that normal development, right from the earliest stages, relies on both genes and epigenetic conditions. For example, a female (XX) fetus developing in a uterine environment that is unusually high in androgens may be born with male-looking genitalia and may have a masculinized area in the hypothalamus, a sexually dimorphic brain region. In mice, the gender of adjacent siblings on the placental fetus line in the uterus will affect such things as the male/female ratio of a given mouse's subsequent offspring, and even the longevity of those offspring.

On the other hand, paradigmatic instances of long-term learning, such as memorizing a route through a forest, rely on genes to produce changes in cells that embody that learning. If you experience a new kind of sensorimotor event during the day – say, for example, you learn to cast a fishing line – and your brain rehearses that event during your deep sleep cycle, then the gene *zif*-268 will be up-regulated. Improvement in casting the next day will depend on the resulting gene products and their role in neuronal function.

Indeed, five important and related discoveries have made it increasingly clear just how interrelated 'nature' and 'nurture' are, and, consequently, how inadequate the old distinction is.[3]

First, what genes do is code for proteins. Strictly speaking, there is no gene for a sucking reflex, let alone for female coyness or Scottish thriftiness or cognizance of the concept of zero. A gene is simply a sequence of base pairs containing the information that allows RNA to string together a sequence of amino acids to constitute a protein. (This gene is said to be 'expressed' when it is transcribed into RNA products, some of which, in turn, are translated into proteins.)

Second, natural selection cannot directly select particular wiring to support a particular domain of knowledge. Blind luck aside, what determines whether the animal survives is its behavior; its equipment, neural and otherwise, underpins that behavior. Representational prowess in a nervous system can be selected for, albeit indirectly, only if the representational package informing the behavior was what gave the animal the competitive edge. Hence representational sophistication and its wiring infrastructure can be selected for only via the behavior they upgrade.

Third, there is a truly stunning degree of conservation in structures and developmental organization across all vertebrate animals, and a very high degree of conservation in basic cellular functions across phyla, from worms to spiders to humans. All nervous systems use essentially the same neurochemicals, and their neurons work in essentially the same way, the variations being vastly outweighed by the similarities. Humans have only about thirty thousand genes, and we differ from mice in only about three hundred of those;[4] meanwhile, we share about 99.7 percent of our genes with chimpanzees. Our brains and those of other primates have the same organization, the same gross structures in roughly the same proportions, the same neuron types, and, so far as we know, much the same developmental schedule and patterns of connectivity.

Fourth, given the high degree of conservation, whence the diversity of multicellular organisms? Molecular biologists have discovered that some genes regulate the expression of other genes, and are themselves regulated by yet other genes, in an intricate, interactive, and systematic organization. But genes (via RNA) make proteins, so the expression of one gene by another may be affected via sensitivity to protein products. Additionally, proteins, both within cells and in the extracellular space, may interact with each other to yield further contingencies that can figure in an unfolding regulatory cascade. Small differences in regulatory genes can have large and far-reaching effects, owing to the intricate hierarchy of regulatory linkages between them. The emergence of complex, interactive cause-effect profiles for gene expression begets very fancy regulatory cascades that can beget very fancy organisms – us, for example.

Fifth, various aspects of the development of an organism from fertilized egg to up-and-running critter depend on where and when cells are born. Neurons originate from the daughter cells of the last division of pre-neuron cells. Whether such a daughter cell becomes a glial (supporting) cell or a neuron, and which type of some hundred types of neurons

3 In this discussion, I am greatly indebted to Barbara Finlay, Richard Darlington, and Nicholas Nicastro, "Developmental Structure in Brain Evolution," *Behavioral and Brain Sciences* 24 (2) (April 2001): 263 – 278.

4 See John Gerhart and Marc Kirschner, *Cells, Embryos, and Evolution* (Oxford: Blackwell, 1997).

the cell becomes, depends on its epigenetic circumstances. Moreover, the manner in which neurons from one area, such as the thalamus, connect to cells in the cortex depends very much on epigenetic circumstances, e.g., on the spontaneous activity, and later, the experience-driven activity, of the thalamic and cortical neurons. This is not to say that there are no causally significant differences between, for instance, the neonatal sucking reflex and knowing how to make a fire. Differences, obviously, there are. The essential point is that the differences do not sort themselves into the archaic 'nature' versus 'nurture' bins. Genes and extragenetic factors collaborate in a complex interdependency.[5]

Recent discoveries in neuropsychology point in this same direction. Hitherto, it was assumed that brain centers – modules dedicated to a specific task – were wired up at birth. The idea was that we were able to see because dedicated 'visual modules' in the cortex were wired for vision; we could feel because dedicated modules in the cortex were wired for touch, and so on.

The truth turns out to be much more puzzling.

For example, the visual cortex of a blind subject is recruited during the reading of braille, a distinctly nonvisual, tactile skill – whether the subject has acquired or congenital blindness. It turns out, moreover, that stimulating the subject's visual cortex with a magnet-induced current will temporarily impede his braille performance. Even more remarkably, activity in the visual cortex occurs even in normal seeing subjects who are blindfolded for a few days while learning to read braille.[6] So long as the blindfold remains firmly in place to prevent any light from falling on the retina, performance of braille reading steadily improves. The blindfold is essential, for normal visual stimuli that activate the visual cortex in the normal way impede acquisition of the tactile skill. For example, if after five days the blindfold is removed, even briefly while the subject watches a television program before going to sleep, his braille performance under blindfold the next day falls from its previous level. If the visual cortex can be recruited in the processing of nonvisual signals, what sense can we make of the notion of the dedicated vision module, and of the dedicated-modules hypothesis more generally?

What is clear is that the nature versus nurture dichotomy is more of a liability than an asset in framing the inquiry into the origin of plasticity in human brains. Its inadequacy is rather like the inadequacy of 'good versus evil' as a framework for understanding the complexity of political life in human societies. It is not that there is nothing to it. But it is like using a grub hoe to remove a splinter.

An appealing idea is that if you learn something, such as how to tie a trucker's knot, then that information will be stored in one particular location in the brain, along with related knowledge – say, between reef knots and half-hitches. That is, after all, a good method for storing tools and paper files – in a particular drawer at a particular location. But this is not the brain's way, as Karl Lashley first demonstrated in the 1920s.

5 See also Steven Quartz and Terrence J. Sejnowski, *Liars, Lovers, and Heroes* (New York: William Morrow, 2002).

6 See Alvaro Pascual-Leone et al., "Study and Modulation of Human Cortical Excitability with Transcranial Magnetic Stimulation," *Journal of Clinical Neurophysiology* 15 (1998): 333–343.

Lashley reasoned that if a rat learned something, such as a route through a certain maze, and if that information was stored in a single, punctate location, then you should be able to extract it by lesioning the rat's brain in the right place. Lashley trained twenty rats on his maze. Next he removed a different area of cortex from each animal, and allowed the rats time to recover. He then retested each one to see which lesion removed knowledge of the maze. Lashley discovered that a rat's knowledge could not be localized to any single region; it appeared that all of the rats were somewhat impaired and yet somewhat competent – although more extensive tissue removal produced more serious memory deficit.

As improved experimental protocols later showed, Lashley's non-localization conclusion was essentially correct. There is no such thing as a dedicated memory organ in the brain; information is not stored on the filing cabinet model at all, but distributed across neurons.

A general understanding of what it means for information to be distributed over neurons in a network has emerged from computer models. The basic idea is that artificial neurons in a network, by virtue of their connections to other artificial neurons and of the variable strengths of those connections, can produce a pattern that represents something – such as a male face or a female face, or the face of Churchill. The connection strengths vary as the artificial network goes through a training phase, during which it gets feedback about the adequacy of its representations given its input. But many details of how actual neural nets – as opposed to computer-simulated ones – store and distribute information have not yet been pinned down, and so computer models and neural experiments are coevolving.

Neuroscientists are trying to understand the structure of learning by using a variety of research strategies. One strategy consists of tracking down experience-dependent changes at the level of the neuron to find out what precisely changes, when, and why. Another strategy involves learning on a larger scale: what happens in behavior and in particular brain subsystems when there are lesions, or during development, or when the subject performs a memory task while in a scanner, or, in the case of experimental animals, when certain genes are knocked out? At this level of inquiry, psychology, neuroscience, and molecular biology closely interact.

Network-level research aims to straddle the gap between the systems and the neuronal levels. One challenge is to understand how distinct local changes in many different neurons yield a coherent global, system-level change and a task-suitable modification of behavior. How do diverse and far-flung changes in the brain underlie an improved golf swing or a better knowledge of quantum mechanics?

What kinds of experience-dependent modifications occur in the brain? From one day to the next, the neurons that collectively make me what I am undergo many structural changes: new branches can sprout, existing branches can extend, and new receptor sites for neurochemical signals can come into being. On the other hand, pruning could decrease branches, and therewith decrease the number of synaptic connections between neurons. Or the synapses on remaining branches could be shut down altogether. Or the whole cell might die, taking with it all the synapses it formerly supported. Or, finally, in certain special regions, a whole new neuron might be born and begin to establish synaptic connections in its region.

And that is not all. Repeated high rates of synaptic firing (spiking) will deplete the neurotransmitter vesicles available for release, thus constituting a kind of memory on the order of two to three seconds. The constituents of particular neurons, the number of vesicles released per spike, and the number of transmitter molecules contained in each vesicle, can change. And yet, somehow, my skills remain much the same, and my autobiographical memories remain intact, even though my brain is never exactly the same from day to day, or even from minute to minute.

No 'bandleader' neurons exist to ensure that diverse changes within neurons and across neuronal populations are properly orchestrated and collectively reflect the lessons of experience. Nevertheless, several general assumptions guide research. For convenience, the broad range of neuronal modifiability can be condensed by referring simply to the modification of synapses. The decision to modify synapses can be made either globally (broadcast widely) or locally (targeting specific synapses). If made globally, then the signal for change will be permissive, in effect saying, "You may change yourself now" – but not dictating exactly where or by how much or in what direction. If local, the decision will likely conform to a rule such as this: If distinct but simultaneous input signals cause the receiving neuron to respond with a spike, then strengthen the connection between the input neurons and the output neurons. On its own, a signal from one presynaptic (sending) neuron is unlikely to cause the postsynaptic (receiving) neuron to spike. But if two distinct presynaptic neurons – perhaps one from the auditory system and one from the somatosensory system – connect to the same postsynaptic neuron at the same time, then the receiving neuron is more likely to spike. This joint input activity creates a larger postsynaptic effect, triggering a cascade of events inside the neuron that strengthens the synapse. This general arrangement allows for distinct but associated world events (e.g., blue flower and plenty of nectar) to be modeled by associated neuronal events.

The nervous system enables animals to make predictions.[7] Unlike plants, animals can use past correlations between classes of events (e.g., between red cherries and a satisfying taste) to judge the probability of future correlations. A central part of learning thus involves computing which specific properties predict the presence of which desirable effects. We correlate variable rewards with a feature to some degree of probability, so good predictions will reflect both the expected value of the reward and the probability of the reward's occurring; this is the expected utility. Humans and bees alike, in the normal course of the business of life, compute expected utility, and some neuronal details are beginning to emerge to explain how our brains do this.

To the casual observer, bees seem to visit flowers for nectar on a willy-nilly basis. Closer observation, however, reveals that they forage methodically. Not only do bees tend to remember which individual flowers they have already visited, but in a field of mixed flowers with varying amounts of nectar they also learn to optimize their foraging strategy, so that they get the most nectar for the least effort.

Suppose you stock a small field with two sets of plastic flowers – yellow and blue – each with wells in the center into which precise amounts of sucrose have

7 John Morgan Allman, *Evolving Brains* (New York: Scientific American Library, 1999).

been deposited.[8] These flowers are randomly distributed around the enclosed field and then baited with measured volumes of 'nectar': all blue flowers have two milliliters; one-third of the yellow flowers have six milliliters, two-thirds have none. This sucrose distribution ensures that the mean value of visiting a population of blue flowers is the same as that of visiting the yellow flowers, though the yellow flowers are more uncertain than the blues.

After an initial random sampling of the flowers, the bees quickly fall into a pattern of going to the blue flowers 85 percent of the time. You can change their foraging pattern by raising the mean value of the yellow flowers – for example, by baiting one-third of them with ten milliliters. The behavior of the bees displays a kind of trade-off between the reliability of the source type and the nectar volume of the source type, with the bees showing a mild preference for reliability. What is interesting is this: depending on the reward profile taken in a sample of visits, the bees revise their strategy. The bees appear to be calculating expected utility. How do bees – *mere* bees – do this?

In the bee brain there is a neuron, though itself neither sensory nor motor, that responds positively to reward. This neuron, called VUMmx1 ('vum' for short), projects very diffusely in the bee brain, reaching both sensory and motor regions, as it mediates reinforcement learning. Using an artificial neural network, Read Montague and Peter Dayan discovered that the activity of vum represents prediction error – that is, the difference between 'the goodies expected' and 'the goodies received this time.'[9] Vum's output is the release of a neuromodulator that targets a variety of cells, including those responsible for action selection. If that neuromodulator also acts on the synapses connecting the sensory neurons to vum, then the synapses will get stronger, depending on whether the vum calculates 'worse than expected' (less neuromodulator) or 'better than expected' (more neuromodulator). Assuming that the Montague-Dayan model is correct, then a surprisingly simple circuit, operating according to a fairly simple weight-modification algorithm, underlies the bee's adaptability to foraging conditions.

Dependency relations between phenomena can be very complex. In much of life, dependencies are conditional and probabilistic: *If* I put a fresh worm on the hook, and *if* it is early afternoon, then *very probably* I will catch a trout *here*. As we learn more about the complexities of the world, we 'upgrade' our representations of dependency relations;[10] we learn, for example, that trout are more likely to be caught when the water is cool, that shadowy pools are more promising fish havens than sunny pools, and that talking to the worm, entreating the trout, or wearing a 'lucky' hat makes no difference. Part of what we call intelligence in humans and other animals is the capacity to acquire an increasingly complex understanding of dependency relations. This allows us to distinguish

8 This experiment was done by Leslie Real, "Animal Choice Behavior and the Evolution of Cognitive Architecture," *Science* (1991): 980–986.

9 See Read Montague and Peter Dayan, "Neurobiological Modeling," in William Bechtel, George Graham, and D. A. Balota, eds., *A Companion to Cognitive Science* (Malden, Mass.: Blackwell, 1998).

10 Clark N. Glymour, *The Mind's Arrows* (Cambridge, Mass.: MIT Press, 2001). See also Alison Gopnik, Andrew N. Meltzoff, and Patricia K. Kuhl, *The Scientist in the Crib* (New York: William Morrow & Co., 1999).

fortuitous correlations that are not genuinely predictive in the long run (e.g., breaking a tooth on Friday the thirteenth) from causal correlations that are (e.g., breaking a tooth and chewing hard candy). This means that we can replace superstitious hypotheses with those that pass empirical muster.

Like the bee, humans and other animals have a reward system that mediates learning about how the world works. There are neurons in the mammalian brain that, like VUM, respond to reward.[11] They shift their responsiveness to a stimulus that predicts reward, or indicates error if the reward is not forthcoming. These neurons project from a brainstem structure (the ventral tegmental area, or 'VTA') to the frontal cortex, and release dopamine onto the postsynaptic neurons. The dopamine, only one of the neurochemicals involved in the reward system, modulates the excitability of the target neurons to the neurotransmitters, thus setting up the conditions for local learning of specific associations.

Reinforcing a behavior by increasing pleasure and decreasing anxiety and pain works very efficiently. Nevertheless, such a system can be hijacked by plant-derived molecules whose behavior mimics the brain's own reward system neurochemicals. Changes in reward system pathways occur after administration of cocaine, nicotine, or opiates, all of which bind to receptor sites on neurons and are similar to the brain's own peptides. The precise role in brain function of the large number of brain peptides is one of neuroscience's continuing conundrums.[12]

These discoveries open the door to understanding the neural organization underlying prediction. They begin to forge the explanatory bridge between experience-dependent changes in single neurons and experience-dependent guidance of behavior. And they have begun to expose the neurobiology of addiction. A complementary line of research, meanwhile, is untangling the mechanisms for predicting what is nasty. Although aversive learning depends upon a different set of structures and networks than does reinforcement learning, here too the critical modifications happen at the level of individual neurons, and these local modifications are coordinated across neuronal populations and integrated across time.

Within other areas of learning research, comparable explanatory threads are beginning to tie together the many levels of nervous system organization. This research has deepened our understanding of working memory (holding information at the ready during the absence of relevant stimuli) spatial learning, autobiographical memory, motor skills, and logical inference. Granting the extraordinary research accomplishments in the neuroscience of knowledge, nevertheless it is vital to realize that these are still very early days for neuroscience. Many surprises – and even a revolution or two – are undoubtedly in store.

Together, neuroscience, psychology, embryology, and molecular biology are teaching us about ourselves as *knowers* – about what it is to know, learn, remember, and forget. But not all philosophers embrace these developments as progress.[13] Some believe that what we call

11 See Paul W. Glimcher, *Decisions, Uncertainty, and the Brain* (Cambridge, Mass.: MIT Press, 2003).

12 I am grateful to Roger Guillemain for discussing this point with me.

13 I take it as a sign of the backwardness of academic philosophy that one of its most esteemed living practitioners, Jerry Fodor, is widely sup-

external reality is naught but an idea created in a nonphysical mind, a mind that can be understood only through introspection and reflection. To these philosophers, developments in cognitive neuroscience seem, at best, irrelevant.

The element of truth in these philosophers' approach is their hunch that the mind is not just a passive canvas on which reality paints. Indeed, we know that brains are continually organizing, structuring, extracting, and creating. As a central part of their predictive functions, nervous systems are rigged to make a coherent story of whatever input they get. 'Coherencing,' as I call it, sometimes entails seeing a fragment as a whole, or a contour where none exists; sometimes it involves predicting the imminent perception of an object as yet unperceived. As a result of learning, brains come to recognize a stimulus as indicating the onset of meningitis in a child, or an eclipse of the Sun by the Earth's shadow. Such knowledge depends upon stacks upon stacks of neural networks. There is no apprehending the nature of reality except via brains, and via the theories and artifacts that brains devise and interpret.

From this it does not follow, however, that reality is *only* a mind-created idea. It means, rather, that our brains have to keep plugging along, trying to devise hypotheses that more accurately map the causal structure of reality. We build the next generation of theories upon the scaffolding – or the ruins – of the last. How do we know whether our hypotheses are increasingly adequate? Only by their relative success in predicting and explaining.

But does all of this mean that there is a kind of fatal circularity in neuroscience – that the brain necessarily uses itself to study itself? Not if you think about it. The brain I study is seldom my own, but that of other animals or humans, and I can reliably generalize to my own case. Neuroepistemology involves many brains – correcting each other, testing each other, and building models that can be rated as better or worse in characterizing the neural world.

Is there anything left for the philosopher to do? For the neurophilosopher, at least, questions abound: about the integration of distinct memory systems, the nature of representation, the nature of reasoning and rationality, how information is used to make decisions, what nervous systems interpret as information, and so on. These are questions with deep roots reaching back to the ancient Greeks, with ramifying branches extending throughout the history and philosophy of Western thought. They are questions where experiment and theoretical insight must jointly conspire, where creativity in experimental design and creativity in theoretical speculation must egg each other on to unforeseen discoveries.[14]

ported for the following conviction: "If you want to know about the mind, study the mind – not the brain, and certainly not the genes" (*Times Literary Supplement*, 16 May 2003, 1 – 2). If philosophy is to have a future, it will have to do better than that.

14 Many thanks to Ed McAmis and Paul Churchland for their ideas and revisions.

Michael Tomasello

Learning through others

Learning is a biological adaptation. The majority of organisms on Earth learn little or nothing during their individual lifetimes. On the other hand, many mammals are born in a highly immature state and so they must individually learn things crucial for their survival. In order to find food reliably, youngsters of foraging species must learn the spatial layouts of their local environments. In order to distinguish friends from enemies, youngsters of social species must learn to recognize the individuals who make up their social groups.

For several decades, behaviorists attempted to find the laws of learning that applied equally to all species, for any and all tasks, and that did not involve to any significant degree processes of cognition. But the modern view that learning assumes diverse forms in different species and behavioral domains, and operates in concert with cognitive processes that may be specific to particular species or domains, has for the most part suspended that search.

For social species such as humans and other mammals, an especially important form of learning is social learning. Observing the activities of others and learning about the world from or through them enables individuals to acquire information with less effort and risk than if they were forced to learn on their own. For instance, many species of rats learn which foods to eat and which to avoid by observing what other rats eat and then seeing what happens to them subsequently – clearly a safer strategy than always trying out new foods for oneself.[1]

Despite an overall similarity in the function of learning in the lives of different species of mammals, social learning, like individual learning, comes in many different forms. In our empirical work over the past fifteen years, we have investigated forms of social learning that

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1 Bennett G. Galef, Jr., "Social Influences in Food Choices of Norway Rats and Mate Choices of Japanese Quail," *International Journal of Comparative Psychology* 14 (1–2) (2001): 1–24.

human beings share with other primate species, as well as forms that are uniquely human. The unique forms mostly derive, we believe, from some social-cognitive processes that only humans possess.[2]

In brief, because human beings perceive the behavior of others in intentional terms – that is, because they perceive a person 'cleaning the table' or 'opening the drawer,' rather than simply moving her limbs in a particular way – they learn from the behavior of others in unique ways. We have called this process 'cultural learning' to distinguish it from processes of social learning in general, and also to highlight the crucial role of culture in the acquisition of many human skills. My colleagues and I have distinguished three kinds of cultural learning: imitative learning, instructed learning, and collaborative learning.[3] The ability of individuals to imagine themselves in the 'mental shoes' of other people, to understand conspecifics as beings like themselves who have intentional and mental lives like themselves, enables these types of cultural learning. Most of our empirical work has focused on only one type of cultural learning – imitation in children before about two years of age. So that will be my focus here.

The recognition of others as intentional beings like oneself is crucial in human learning, most importantly because artifacts and practices – exemplified prototypically by the use of tools and linguistic symbols – invariably point beyond themselves to the phenomena for which they have been designed. To learn the conventional use of a tool or a symbol, an individual must therefore come to understand why, toward what outside end, another individual is using it.[4]

Chimpanzees, humans' nearest primate relatives, do not learn from one another in this same way. In 1996, I reviewed all of the experimental studies of chimpanzee tool use, and I concluded that chimpanzees are very good at learning from others about the dynamic affordances of objects, but are not skillful at learning from others new behavioral strategies or intentional activities per se.[5] For example, if a mother rolls over a log and eats the insects underneath, her child will very likely follow suit. From her mother's act the child has learned that there are insects under this particular log – but she did not learn from her mother how to roll over a log or how to eat insects; she could have learned these on her own. Thus the youngster would have learned the same thing if the wind, rather than her mother, had exposed the ants under the log. This is an instance of 'emulation learning,' which concerns changes of state in the environment rather than a conspecific's intentional activity or behavioral strategy.

In some circumstances, emulation learning is a more adaptive strategy than learning by imitation. For example, Kathy Nagell, Kelly Olguin, and I presented chimpanzees and two-year-old human children with a rake-like tool and

2 Michael Tomasello, Ann C. Kruger, and Hillary H. Ratner, "Cultural Learning," *Behavioral and Brain Sciences* 16 (1993): 495 – 552; Michael Tomasello, "Do Apes Ape?" in Cecilia M. Heyes and Bennett G. Galef, Jr., eds., *Social Learning in Animals: The Roots of Culture* (San Diego, Calif.: Academic Press, 1996); Michael Tomasello, *The Cultural Origins of Human Cognition* (Cambridge, Mass.: Harvard University Press, 1999).

3 Tomasello, Kruger, and Ratner, "Cultural Learning."

4 Tomasello, *The Cultural Origins of Human Cognition.*

5 Tomasello, "Do Apes Ape?"

an out-of-reach object.[6] The tool could be used in either of two ways leading to the same end result of obtaining the object. Within each species, one group of subjects observed a demonstrator employ a relatively inefficient method of tool use, while another group observed a more efficient method of tool use. The result: human children in general copied the method of the assigned demonstrator (imitative learning), while chimpanzees used the same methods to obtain the object no matter which demonstration they observed (emulation learning). The interesting point is that many children insisted on reproducing adult behavior even if it seemed inefficient – leading to a less successful performance than that of the chimpanzees. Imitation is thus not a 'higher' or 'more intelligent' learning strategy than emulation; it is simply a more culturally mediated strategy – which, in some circumstances and for some behaviors, has some advantages.

Chimpanzees are very creative in using tools, and intelligent about understanding changes in the environment brought about by the tool use of others. But they do not seem to understand the instrumental behavior of conspecifics in the same way as humans do. Humans perceive the demonstrator's apparent intention as centrally important, and they understand this goal as something separate from the various behavioral means that may be used to accomplish it. In the absence of this ability to understand goal and behavioral means as separable in the actions of others, chimpanzees focus on the changes of state (including changes in the spatial position) of the objects during the demonstration, perceiving the actions of the demonstrator just, in effect, as other physical motions. The intentional states of the demonstrator, and thus her behavioral methods as distinct entities, are simply not a part of their experience.

A similar story may be told about the gestural communication of chimpanzees. In a series of studies, we explored whether youngsters acquire their gestural signals by imitative learning or by a process of ontogenetic ritualization.[7] In ontogenetic ritualization, two organisms devise a communicatory signal through repeated instances of a social interaction. For example, an infant may initiate nursing by going directly for the mother's nipple, perhaps grabbing and moving her mother's arm in the process. So in some future encounter the mother might sense, and respond to, her infant's hunger at the first touch of her arm, leading the infant to abbreviate her signal for hunger even further the next time. This is presumably analogous to the way that most human infants learn the 'arms over head' gesture to request that adults pick them up – first as a direct attempt to crawl up the

6 Katherine Nagell, Raquel Olguin, and Michael Tomasello, "Processes of Social Learning in the Tool Use of Chimpanzees (*Pan troglodytes*) and Human Children (*Homo sapiens*)," *Journal of Comparative Psychology* 107 (1993): 174 – 186.

7 Michael Tomasello, Barbara George, Ann C. Kruger, Jeff Farrar, and Andrea Evans, "The Development of Gestural Communication in Young Chimpanzees," *Journal of Human Evolution"* 14 (1985): 175 – 186; Michael Tomasello, Deborah Gust, and Thomas Frost, "A Longitudinal Investigation of Gestural Communication in Young Chimpanzees," *Primates* 30 (1989): 35 – 50; Michael Tomasello, Josep Call, Katherine Nagell, Raquel Olguin, and Malinda Carpenter, "The Learning and Use of Gestural Signals by Young Chimpanzees: A Transgenerational Study," *Primates* 37 (1994): 137 – 154; Michael Tomasello, Josep Call, Jennifer Warren, Thomas Frost, Malinda Carpenter, and Katherine Nagell, "The Ontogeny of Chimpanzee Gestural Signals: A Comparison Across Groups and Generations," *Evolution of Communication* 1 (1997): 223 – 253.

adult's body, and then, as the adult anticipates the baby's desire and picks her up, as an abbreviated, ritualized version of this crawling activity performed for communicative purposes only.[8]

All available evidence suggests that ontogenetic ritualization, not imitative learning, is responsible for chimpanzees' acquisition of communicative gestures. Individual chimpanzees use a number of idiosyncratic signals that must have been individually invented and ritualized – a finding that longitudinal analyses have confirmed.[9] Significantly, captive youngsters raised in peer groups that have no opportunity to observe older conspecifics frequently use many of the same gestures that are common among other chimpanzee youngsters. In an experimental study, colleagues and I removed an individual from the group and taught her two different arbitrary gestures she could use to obtain desired food from a human.[10] When she returned to her group and used these signals to obtain food from a human, not even one chimpanzee reproduced either of the new gestures – even though all of the other individuals observed the gesturer and were highly motivated for the food.

Chimpanzee youngsters thus acquire the majority, if not the totality, of their gestures by individually ritualizing them with one another. The explanation for this learning process is analogous to the explanation for emulation learning in the case of tool use. Like emulation learning, ontogenetic ritualization does not require individuals to analyze the behavior of others in terms of ends and means in the same way as does imitative learning. Imitatively learning an arm touch as a solicitation for nursing would require that an infant observe another infant using an arm touch and understand that other infant's goal. Ritualizing the arm touch, on the other hand, only requires the infant to anticipate the future behavior of a conspecific in a context in which the infant already has the goal of nursing. Ontogenetic ritualization is thus, like emulation learning, a very useful learning process that is important in all social species – but it is not a learning process by which individuals attempt to reproduce the intentional activities or behavioral strategies of others; it is not cultural learning the way humans practice it.

Human beings begin to learn through imitation at around the first birthday. But it takes clever experimentation to distinguish the unique features of this form of learning from those of another. For example, if an adult takes the top off of a pen and a child then does the same, there are many possible explanations, including emulation and mimicking (copying movements without knowing what they are for). Researchers have therefore devised ingenious techniques for analyzing the different components of what the child perceives, understands, and reproduces in a demonstrated act.

For example, according to the technique Andy Meltzoff devised, fourteen-month-old infants saw an adult illuminate a box by bending down and touching her head to the top of it.[11] Although

8 Andrew Lock, "The Emergence of Language," in Andrew Lock, ed., *Action, Gesture, and Symbol: The Emergence of Language* (New York: Academic Press, 1978).

9 See Jane Goodall, *The Chimpanzees of Gombe: Patterns of Behavior* (Cambridge, Mass.: Harvard University Press, 1986).

10 Tomasello, Call, Warren, Frost, Carpenter, and Nagell, "The Ontogeny of Chimpanzee Gestural Signals."

11 Andrew Meltzoff, "Infant Imitation After a One-Week Delay: Long-Term Memory for

infants could more easily have solved this task by emulation (e.g., by touching the box with their hand), they instead chose to use the same means as the adult, unusual as it was. These infants could have been mimicking the adult's unusual action without understanding the goal of turning on the light. But if they had been copying this action with the same goal in mind, their behavior would have been an instance of imitative learning.

In order to determine which of these two mechanisms was at work, Malinda Carpenter, Nagell, and I tested nine- to fifteen-month-old infants on a modified version of this task: we delayed the illumination of the light after the infants' reproduction of the action, and noted whether they looked in anticipation to the light.[12] We found that infants twelve-months and older looked to the light in anticipation before it came on. If the light did not come on, they often repeated their action or looked quizzically to the people in the room. This suggests that they were adopting the adult's means in order to achieve the same goal of turning on the light. Infants thus were not just mimicking the adult's action, but were engaging in imitative learning of a novel means to achieve a perceived end.

In another experiment, infants were shown identical actions that produced identical results, but with different expressed intentions. Carpenter, Nameera Akhtar, and I showed fourteen- to eighteen-month-olds a series of two actions on objects.[13] For each object, the pair of actions was followed by a striking result – the sudden illumination of colored lights, for example. In the key experiment, one of the demonstrator's paired actions was marked verbally as intentional ("There!") while the other was marked verbally as accidental ("Woops!"), but otherwise the actions looked very similar. Instead of mimicking both of the actions they observed, even the youngest infants reproduced the action marked as intentional significantly more often than the one marked as accidental.

Another study demonstrated that infants were able to imagine the goal toward which the adult was acting, even though they never actually saw any concrete results. In a 1995 experiment, Meltzoff showed eighteen-month-olds an adult either successfully completing a task (pulling apart two halves of a dumbbell) or trying but failing to do so (because the adult's hands slipped off the ends of the dumbbell). Infants were able to complete the task whether or not they had seen an adult successfully complete it. Yet these eighteen-month-olds were not able to achieve the same result when they watched a machine either successfully completing the same task or trying, or failing, to do so. Francesca Bellagamba and I replicated the basic findings of this study with twelve- and eighteen-month-old infants, but we found that twelve-month-olds could not reproduce the adult's intended action when they only saw her trying unsuccessfully to perform it.[14]

Novel Acts and Multiple Stimuli," *Developmental Psychology* 24 (1988): 470 – 476.

12 Malinda Carpenter, Katherine Nagell, and Michael Tomasello, "Social Cognition, Joint Attention, and Communicative Competence from 9 to 15 Months of Age," *Monographs of the Society for Research in Child Development* 63 (4) (1998).

13 Malinda Carpenter, Nameera Akhtar, and Michael Tomasello, "Sixteen-Month-Old Infants Differentially Imitate Intentional and Accidental Actions," *Infant Behavior and Development* 21 (1998): 315 – 330.

14 Francesca Bellagamba and Michael Tomasello, "Reenacting Intended Acts: Comparing

Other studies have manipulated the social learning context in an effort to influence what behavior children reproduce and so gain insight into what they interpret as intentional action. Using Meltzoff's study as a starting point, George Gergely, Harold Bekkering, and Ilday Király showed fourteen-month-olds an adult touching her head to the top of a box to turn on a light.[15] In their study, half of the infants saw the adult turn on the light while her hands were occupied (she was holding a blanket around her shoulders), and half saw her turn it on while her hands were free. Infants who saw the hands-free demonstration touched the box with their heads significantly more often than infants who saw the hands-occupied demonstration. Infants thus used the context of the situation to interpret the adult's behavior, appearing to assume that if the adult's hands were free and she still chose to use her head, then there must be a good reason for this choice. Meanwhile, the infants who saw the other demonstration apparently interpreted the use of her head as necessary given her circumstances (and so as an inessential part of her intention), and thus did not reproduce this action. These infants' interpretation of the adult's goal thus differed across conditions: in the hands-occupied condition her apparent goal was 'turn on the light'; in the hands-free condition it was 'turn on the light with your head.' By fourteen months, infants thus evidence a very deep understanding of intentional action, of how it relates to the surrounding context, and of what this means for their own choice of a behavioral means in similar or different circumstances.

A series of studies of older children extends these findings. For example, Bekkering and his colleagues showed three- to six-year-old children an experimenter touching a table in one of two locations.[16] In one condition there were dots on the table in those locations, and in another condition there were no dots. In the no-dot condition, children usually matched the adult's behavior exactly, even copying her crossed or straight arm positions – presumably because there was no other apparent goal to her actions than these arm movements. In the dot condition, however, children touched the same locations as the experimenter, but often did not match her exact arm positions. This is presumably because when there were dots they interpreted the adult's goal as 'touching the dots,' whereas when there were no dots the only possible goal seemed to be 'moving one's arms like this.' Bekkering and his colleagues concluded that young children's imitation is guided by their understanding of adults' goals and of the hierarchy of those goals, and that children imitate what they perceive the adults' main goal to be.

Subsequent studies have confirmed that children use context to interpret adults' actions, and that this influences what they learn. In one study, an adult demonstrated to five groups of children how to pull out a pin and open a box.[17]

12- and 18-Month-Olds," *Infant Behavior and Development* 22 (1999): 277 – 282.

15 György Gergely, Harold Bekkering, and Ildikó Király, "Rational Imitation in Preverbal Infants," *Nature* 415 (2002): 755.

16 Harold Bekkering, Andrew W. Wohlschlaeger, and Merideth Gattis, "Imitation of Gestures in Children is Goal-directed," *Quarterly Journal of Experimental Psychology* 53A (1) (2000): 153 – 164.

17 Malinda Carpenter, Josep Call, and Michael Tomasello, "Understanding Others' Prior Intentions Enables 2-Year-Olds to Imitatively Learn a Complex Task," *Child Development* 73 (2002): 1431 – 1442.

What differed among the groups was what the children experienced just prior to this demonstration. One group of children received information about the adult's goal ahead of time; another group received none; the three other groups received varying amounts of information about the adult's goal. In the demonstration, the adult either tugged unsuccessfully on the door of the box, or showed the box already open, or visited and opened three different boxes before demonstrating how to open the test box. Thus all of the children in all of these conditions saw a full demonstration of how to open the box, but only the children in the three prior-information conditions could know what the adult was about to do before she began this demonstration. Two- and two-and-a-half-year-old children were significantly better at opening the box themselves when they knew the adult's goal ahead of time. In this study, then, children interpreted the exact same behavior differently depending on whether they knew the adult's goal ahead of time – with no concurrent cues in adult emotional expression or the like. In other words, in the control conditions the children were not able to provide an intentional description of 'what the adult is doing,' whereas in the prior information conditions they were able to understand the behavior as the intentional action 'trying to open the box.'

There are some kinds of actions that children observe and attempt to imitate that have a special structure because they involve people having goals toward one another reciprocally. For example, a mother might blow a raspberry along her child's arm; if the child wants to imitate this behavior, she is faced with a choice that depends on how she interprets her mother's action. Thus, she might blow a raspberry along her own arm, in exactly the same place her mother did, or alternatively, she might blow a raspberry along her mother's arm – interpreting the behavior in this case reciprocally as 'blowing on the partner's arm.' I have called this 'role reversal imitation.' In an ongoing study, my colleagues and I have found that eighteen-month-olds are more likely to employ this reciprocal interpretation than are twelve-month-olds. At both ages, children are more likely to reciprocate in the situation where the adult, for example, pats her own head (and the child pats his own), than in the case where the two partners act on one another.

A similar process occurs in the learning of language, since learning to use linguistic symbols is also reciprocal. Thus, when an adult uses a linguistic symbol in a communicative act, she directs the child to attend to something. Consequently, to learn to use a symbol as the adult does, the child must learn to direct the adult's attention as the adult had directed the child's.[18]

Interestingly, my colleagues and I have recently offered evidence that something similar goes on in children's early symbolic play. Before two years of age, by watching adults children imitatively learn symbolic behaviors with objects, in much the same way that they learn instrumental actions with artifacts. As they grow older, they look to the adult more often, and in some cases smile more often, when producing the symbolic behaviors. This is evidence that children of this age are reproducing a special kind of intentionality – a kind of mutually reciprocal intentionality in which for the moment the child and the adult agree, for example, to treat a pencil as if it were a horse.

18 Tomasello, *The Cultural Origins of Human Cognition*.

Given the general ability to learn a symbol through role reversal imitation, it is still the case that in learning particular words on particular occasions children often need to read the adults intentions in order to connect the word appropriately to its intended referent. Several language acquisition studies show that children as young as eighteen months can combine all of the types of intention reading we have discussed above while imitatively learning novel words. For example, in a study of twenty-four-month-olds, an adult announced her (prior) intention to find a target object by saying, "Let's go find the *toma*."[19] She searched through several buckets, extracting and rejecting with a scowl the novel objects inside. She then extracted another novel object with an excited expression and stopped searching. In a later comprehension test, when asked to go get the *toma* themselves, children chose the object the adult had identified as fulfilling her intention. This experiment used a modified procedure to show that twenty-four-month-old children could identify the intended referent even when the adult was unable to open the container with the target object inside – that is, when she had an unfulfilled intention. Another study investigated children's use of their understanding of intentional versus accidental actions when learning novel words. In a study of twenty-four-month-olds, the adult announced her (prior) intention to perform a target action by saying, "I'm going to *meek* Big Bird!" She then performed, in counterbalanced order, one accidental action, which she verbalized by saying "Woops!" and one intentional action, which she indicated by saying "There!" Later, when they were asked to *meek* a different character themselves, children performed the action that the adult had marked as intentional.

Like the studies of actions on objects, these word learning studies provide evidence that at a very early age children come to understand intentional action. And human learning is what it is – namely, cultural learning – because human beings, even when quite young, are able to understand the intentional and mental states of other human beings. Through this understanding, cultural processes take human cognition in some directions not possible in other species – and make human cognition an essentially collective enterprise.

19 Michael Tomasello and Michelle Barton, "Learning Words in Non-ostensive Contexts," *Developmental Psychology* 30 (1994): 639 – 650.

Susan Carey

Bootstrapping & the origin of concepts

All animals learn. But only human beings create scientific theories, mathematics, literature, moral systems, and complex technology. And only humans have the capacity to acquire such culturally constructed knowledge in the normal course of immersion in the adult world.

There are many reasons for the differences between the minds of humans and other animals. We have bigger brains, and hence more powerful information processors; sometimes differences in the power of a processor can create what look like qualitative differences in kind. And of course human beings also have language – the main medium for the cultural transmission of acquired knowledge. Comparative studies of humans and other primates suggest that we differ from them as well in our substantive cognitive abilities – for example, our capacity for causal analysis and our capacity to reason about the mental states of others. Each of these factors doubtless contributes to our prodigious ability to learn.

But in my view another factor is even more important: our uniquely human ability to 'bootstrap.' Many psychologists, historians, and philosophers of science have appealed to the metaphor of bootstrapping in order to explain learning of a particularly difficult sort – those cases in which the endpoint of the process transcends in some qualitative way the starting point. The choice of metaphor may seem puzzling – it is self-evidently impossible to pull oneself up by one's own bootstrap. After all, the process I describe below is not impossible, but I keep the term because of its historical credentials and because it seeks to explain cases of learning that many have argued *are* impossible.

Sometimes learning requires the creation of new representational resources that are more powerful than those present at the outset. Early in the cultural history of mathematics, for instance, the concept of the number included only positive integers: with subsequent development the concept came to encompass zero, rational numbers (fractions), negative numbers, irrational numbers like pi, and so on.

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Bootstrapping is the process that underlies the creation of such new concepts, and thus it is part of the answer to the question: What is the origin of concepts?

Individual concepts are the units of thought. They are constituents of larger mental structures – of beliefs that are formed out of them and of systems of representation such as intuitive theories. Concepts are individuated on the basis of two kinds of considerations: their reference to different entities in the world and their role in distinct mental systems of inferential relations.

How do human beings acquire concepts? Logic dictates three parts to any explanation of the origin of concepts. First, we must specify the innate representations that provide the building blocks of the target concepts of interest. Second, we must describe how the target concepts differ from these innate representations – that is, we must describe developmental change. And third, we must characterize the learning mechanisms that enable the construction of new concepts out of the prior representations.

Claims about all three parts of the explanation of the origin of concepts are highly controversial. Many believe that innate representations are either perceptual or sensory, while others (including myself) hold that humans and other animals are endowed with some innate representations with rich conceptual content. Some researchers also debate the existence, even the possibility, of qualitative changes to the child's initial representations. One argument for the impossibility of such radical changes in the course of development is the putative lack of learning mechanisms that could explain them. This is the gap that my appeal to bootstrapping is meant to fill.

To make clear both what the problem is, and what role bootstrapping may play in solving it, I will examine how children acquire one specific set of concepts: the positive integers – i.e., concepts such as *one, two, three, nine, eighteen,* etc.

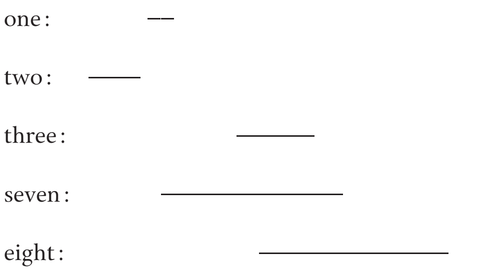
Before they acquire language, infants form several different types of representation with numerical content, at least two of which they share with other vertebrate animals.

One, described by Stanislas Dehaene in his delightful book *The Number Sense,* uses mental symbols that are neural magnitudes linearly related to the number of individuals in a set. Because the symbols get bigger as the represented entity gets bigger, they are called analog magnitudes. Figure 1 gives an external analog magnitude representation of number, where the symbol is a line, and length is the magnitude linearly related to number. Mental computations using these symbols include comparison, to establish numerical difference or equality, and also addition and subtraction.

Mental analog magnitudes represent many dimensions of experience – for example, brightness, loudness, and temporal duration. In each case as the physical magnitudes get bigger, it becomes increasingly harder to discriminate between pairs of values that are separated by the same absolute difference. You can see in figure 1 that it is harder to tell that the symbol for seven is different from (and smaller than) that for eight than it is to tell that the symbol for two is different from (and smaller than) that for three. Analog magnitude representations follow Weber's law, according to which the discriminability of two values is a function of their ratio.

You can confirm for yourself that you have an analog magnitude system of representation of number that conforms to Weber's law. Tap out as fast as you can without counting (you can prevent your-

Figure 1
Analog magnitude models
Number represented by a quantity linearly related to the cardinal value of the set



self from counting by thinking 'the' with each tap) the following numbers of taps: 4, 15, 7, and 28. If you carried this out several times, you'd find the mean number of taps to be 4, 15, 7, and 28, with the range of variation very tight around 4 (usually 4, occasionally 3 or 5) and very great around 28 (from 14 to 40 taps, for example). Discriminability is a function of the absolute numerical value, as dictated by Weber's law. Since you were not counting, some other numerical representation must have been guiding your tapping performance – presumably analog magnitudes, as your adherence to Weber's law, again, would seem to indicate.

Space precludes my reviewing the elegant evidence for analog magnitude representations of number in animals and human infants, but let me give just one example. Fei Xu and Elizabeth Spelke showed infants arrays of dots, one dot array at a time, until the infants got bored with looking at them. All other variables that could have been confounded with number (total array size, total volume of dots, density of dots, and so on) were controlled in these studies, such that the only possible basis for the infants' discrimination was numeric. Seven-month-old infants were habituated either to arrays of eight or sixteen dots. After habituation they were presented with new displays containing either the same number of dots to which they had been habituated or the other number. Xu and Spelke found that the infants recovered interest to the new number, and so concluded that they are capable of representing number. Xu and Spelke also found evidence for Weber's law: infants could discriminate eight from sixteen and sixteen from thirty-two, but not eight from twelve or sixteen from twenty-four.[1]

Infants and animals can form analog magnitude representations of fairly large sets, but these representations are only approximate. Analog magnitude representations of number fall short of the representational power of integers; in this system one cannot represent *exactly* fifteen, or fifteen *as opposed to* fourteen. Nonetheless, analog magnitude representations clearly have numerical content: they refer to numerical values, and number-relevant computations are defined over them.

A second system of representations with numerical content works very differently. Infants and nonhuman primates have the capacity to form symbols for individuals and to create mental models of ongoing events in which each individual is represented by a single symbol. Figure 2 shows how, in this system, sets of one, two, or three boxes might be represented. The figure represents three different possibilities for the format and content of the symbols.

1 For an overview of the evidence for analog magnitude representations of number in both nonhuman animals and human adults, see Stanislas Dehaene, *The Number Sense* (Oxford: Oxford University Press, 1997). For evidence in human infants, see Fei Xu and Elizabeth S. Spelke, "Large Number Discrimination in 6-Month-Old Infants," *Cognition* 74 (2000): B1 – B11.

There is one symbol for each box, so number is implicitly represented; the symbols in the model stand in one-one correspondence with the objects in the world.

To give you a feel for the evidence that infants indeed employ such models, distinct from the analog magnitude representations sketched above, consider the following experiment from my laboratory. Ten- to fourteen-month-old infants are shown a box into which they can reach to retrieve objects, but into which they cannot see. If you show infants three objects being placed, one at a time or all at once, into this box, and then allow them to reach in to retrieve them one at a time, they show by their pattern of reaching that they expect to find exactly three objects there. If the infant has a mental representation of a set of two objects (e.g., object, object) that are hidden from view, and the infant sees a new object being added to the set, the infant creates a mental representation of a set of three (object, object, object). Further, computations of one-one correspondence carried out over these models allow the child to establish numerical equivalence and number order (e.g., Have I got all the objects out of the box or are there more?)

So far, this is just another demonstration that infants represent number. However, an exploration of the limits on infants' performance of this task implicates a different system of representation from the analog magnitude system sketched above.

Performance breaks down at four objects. If the infants see four objects being placed into the box and are allowed to retrieve two of them, or even just one of them, they do not reach persistently for the remaining objects. Remember that in the analog magnitude system of representation, success at

Figure 2
Parallel individuation models

Number of boxes	*Image*	*Abstract*	*Specific*
1 box	□	obj	box
2 boxes	□□	obj obj	box box
3 boxes	□□□	obj obj obj	box box box

Note: one symbol for each individual; no symbols for integers.

numerical comparison is a function of the ratios of the numbers being compared, and that the representations can handle sets of objects at least as big as thirty-two. But in this reaching task, infants succeed at ratios of 2:1 and 3:2, but fail at 4:2 and even 4:1; as soon as the set exceeds three, infants cannot hold a model of distinct items in their short-term memory.[2]

In sum, human infants (and other primates) are endowed with at least two distinct systems of representation with numerical content. Both take sets of individuals as input. One creates a summary analog representation that is a linear function of the number of individuals in the set. This process is noisy, and the noise is itself a linear function of the set size, with the consequence that the representations are merely approximate. For several reasons, this system is too weak to represent the positive integers. For one, there is likely an upper bound to the set sizes that can be represented by analog magnitudes. More importantly,

2 For evidence of the set-size limits on infants' representations of small numbers of objects, see Lisa Feigenson and Susan Carey, "Tracking Individuals Via Object Files: Evidence from Infants' Manual Search," *Developmental Science* 6 (2003): 568–584.

animals and infants cannot discriminate adjacent integer values once the sets contain more than three or four individuals; that is, they cannot represent *exactly* fifteen or twenty-five or forty-nine, or any other large exact integer. Finally, analog magnitude representations obscure one of the foundational relations among successive integers – that each one is exactly one more than the one before. It is this relation, called the successor relation, that underlies how counting algorithms work and provides the mathematical foundation of integer concepts. Since discriminabilty of analog magnitudes is a function of the ratio between them, the relation between two and three is not experienced as the same as that between twenty-four and twenty-five; indeed, the latter two values cannot really even be discriminated within this system of representation.

The second system – one symbol for each individual – falls even shorter as a representation of integers. There are no symbols for number in this system at all; the symbols in figure 2 each represent an individual object, unlike those in figure 1, which represent an approximate cardinal value. Furthermore, what can be represented in this system is limited in number to sets of one, two, and three.

The count list ('one, two, three ...') is a system of representation that has the power to represent the positive integers, so long as it contains a generative system for creating an infinite list. When deployed in counting, it provides a representation of exact integer values based on the successor function. That is, when applied in order, in one-one correspondence with the individuals in a set, the ordinal position of the last number word in the count provides a representation of the cardinal value of the set – of how many individuals it contains. Successive symbols in the list refer to cardinal values exactly one apart: 5 is 4 plus 1, 6 is 5 plus 1, and so on.

I have argued so far that the count-list representation of number transcends the representational power of both of the representational systems with numerical content that are available to preverbal infants, for these precursors lack the capacity to represent integers. If this is so, it should be difficult for children to come to understand the numerical function of counting.

And so, indeed, it is difficult for children to learn how counting represents number, and details about the partial understanding they achieve along the way constrain our theories of the learning process. In the United States (and every other place where early counting has been studied, including Western Europe, Russia, China, and Japan) children learn to recite the count list as young two-year-olds, and at this age can even engage in the routine of counting – touching objects in a set one by one as they recite the list. But it takes another year and a half before they work out how counting represents number, and in every culture yet studied, children go through similar stages in working out the meanings of the number words in the count list.

First, children learn what 'one' means and take all other words in the list to contrast with 'one,' meaning 'more than one' or 'some.' The behaviors that demonstrate this are quite striking. If you present young two-year-olds with a pile of pennies and ask them to give you one penny, they comply. If you ask for two pennies or three pennies or five pennies, they grab a bunch, always more than one, and hand them over. They do not create a larger set for 'five' than for 'two.' You might suppose that the plural in 'pennies' is doing the work here, but the same phenomenon is observed in China and Japan, even though Chinese and Japanese do not have a singular-plural distinction, and also in the United States when the contrast is between 'one fish,' 'two fish,' and 'five fish.'

Let us call children at this stage of working out the meanings of number words 'one-knowers.' Many other tasks provide additional evidence that one-knowers truly know only the meaning of the word 'one' among all the words in their count list. For example, if you ask a one-knower to tell you what's on a series of cards that contain one, two, or three fish (up to eight fish), they say 'a fish' or 'one fish' for the card with one, and 'two fish' or 'two fishes' or 'two fishies' for all of the other cards. This again indicates a single cut between the meaning of 'one,' which they grasp, and words for the number of individuals in larger sets, which they do not.

After having been one-knowers for about six to nine months, children learn what 'two' means. At this point they can correctly give you two objects if you ask for 'two,' but they still just grab a bunch (always greater than two), if you ask for 'three,' 'four,' 'five,' or 'six.' After some months as two-knowers, they become three-knowers, and some months later induce how counting works.

The performance of children who have worked out how counting works is qualitatively different from that of the one-, two-, and three-knowers in a variety of ways that reflect the conceptual understanding of counting.

To give just one example, in the task in which children are asked to give the experimenter a certain number of items, say four, one-, two-, and three-knowers usually give the wrong number, and the young counters also sometimes make an error. When asked to check by counting and then to fix the set, counters invariably adjust the set in the right direction, taking an object away if the set is too large or adding one if it is too small. One-, two-, and three-knowers, in contrast, almost always add more to the set – even if they had counted to five or six or seven when they were checking whether it had four – confirming that they really do not understand how counting determines the meaning of number words.

These data suggest that the partial meanings of number words seem to be organized initially by the semantics of quantifiers – the singular-plural distinction and the meanings of words like 'some' and 'a.' If this is right, then we might expect that children learning languages with quantifier systems that mark numerical contrasts differently from English would entertain different hypotheses concerning the partial meanings of number words. They might break into the system differently. And indeed they do.

Consider first classifier languages such as Chinese and Japanese that do not mark the distinction between singular and plural in nouns, verbs, or adjectives. Two independent studies have found that although children in China and Japan learn the count list as young as English-speaking children do, they become one-knowers several months later and are relatively delayed at each stage of the process. Conversely, Russian has a complex plural system in which the morphological markers for sets of two, three, and four differ from those for five through ten. Two independent studies have shown that even Russian one- and two-knowers distinguish between the meanings of the number words 'two,' 'three,' and 'four,' on the one hand, and 'five,' 'six,' 'seven,' and 'eight,' on the other. Unlike the one- and two-knowers described above, Russian children in the early stages of working out how counting works grab smaller sets when asked to give the experimenter 'two,' 'three,' or 'four' than when asked to give the experimenter 'five' or more, and use larger numbers for larger sets in the what's-on-this-card task.[3]

These phenomena concerning young children's partial understanding of the meanings of number words support three interrelated conclusions. First, that it is so difficult for children to learn what 'two' means, let alone what 'five' and 'eight' mean, lends support to the claims that preverbal number representations are not representations of integers, at least not in the format of an integer list. Young children – for a full six to nine months before they work out what 'two' means, and a full year and a half before they work out how the count list represents integers – know how to count, know what 'one' means, and know that 'two,' 'three,' 'four,' 'five,' 'six,' 'seven,' and 'eight' represent numbers larger than 'one.' Second, coming to understand how the count list represents numbers reflects a qualitative change in the child's representational capacities; I would argue that it does nothing less than create a representation of the positive integers where none was available before. Finally, a third possible developmental source of natural number representations, in addition to the preverbal systems described above, may be the representations of numbers within natural language quantifier semantics. Of course, natural language quantifiers, other than the number words in the count list itself, do not have the power to represent natural numbers either.

The problem of the origin of the positive integers arises at two different time scales – historical and ontogenetic. At the dawn of modern anthropology, when colonial officers went out into the French and English colonial worlds, they discovered many systems of explicit number representation that fell short of a full representation of natural number. They described languages that marked number on nouns, adjectives, and verbs, and which had quantifiers like the English 'one,' 'two,' 'many,' 'some,' 'each,' 'every,' and 'more,' but which had no count list. In this vein, the psychologist Peter Gordon has described the language of the Piraha, an isolated Amazonian people. He has shown that in addition to linguistic quantifiers meaning 'one,' 'two,' and 'many,' the Piraha also have access to the nonverbal systems described above (parallel individuation of small sets and analog magnitude representations of large numerosities). Gordon confirms that they have no representations of large exact numerical values.

Anthropologists and archeologists have described intermediate systems of integer representation, short of integer lists, and these intermediate systems provide evidence for a process of cultural construction over generations and centuries of historical time.[4] Here I concentrate on ontogenetic time. How do three-year-olds do it? How do they create a representational system with more

3 For a characterization of the early stages of counting in English, see Karen Wynn, "Children's Acquisition of the Number Words and the Counting System," *Cognitive Psychology* 24 (2) (1992): 220–257.

4 I would recommend the linguist James Hurford's review of this literature to any reader interested in this process. James Hurford, *Language and Number* (Oxford: Basil Blackwell, 1987). For work on the Piraha, see Peter Gordon, "The Role of Language in Numerical Cognition: Evidence from Amazonia" (under review).

power than any on which it is built?[5] In answering this question, I would appeal to bootstrapping processes.

Bootstrapping processes make essential use of the human capacity for creating and using external symbols such as words and icons. Bootstrapping capitalizes on our ability to learn sets of symbols and the relations among them directly, independently of any meaning assigned to them in terms of antecedently interpreted mental representations. These external symbols then serve as placeholders, to be filled in with richer and richer meanings. The processes that fill the placeholders create mappings between previously separate systems of representation, drawing on the human capacity for analogical reasoning and inductive inference. The power of the resulting system of concepts derives from the combination and integration of previously distinct representational systems.

Let's see how this might work in the present case. We must allow the child one more prenumerical capacity – that of representing serial order. This is no problem – young children learn a variety of meaningless ordered lists, such as 'eeny, meeny, miney, mo.'

We seek to explain how the child learns the meanings of the number words – what 'two' means, what 'seven' means – and how the child learns how the list itself represents number – that the cardinal value of a set enumerated by counting is determined by the order on the list, and that successive numbers on the list are related by the arithmetic successor relation.

As described above, the child learns the meanings of the first number words as natural language quantifiers. Children learn the meaning of 'one' just as they learn the meaning of the singular determiner 'a' (indeed, in many languages, such as French, they are the same lexical item).

Some months later, 'two' is learned, just as dual markers are in languages that have singular/dual/plural morphology. Languages with dual markers have a different plural affix for sets of two than the affix for sets greater than two. It is as if English nouns were declined 'box' (singular), 'boxesh' (dual), 'boxeesh' (plural). In this system, the suffix 'esh' would apply just when the set referred to contained exactly two items. By hypothesis, children would learn the meaning of the word 'two' just as they would learn the morphological marker 'esh' – if English plural markers worked that way. By extension, some months later, 'three' is learned just as trial markers are in the rare languages that have singular/dual/trial/plural morphology.

In the early stages of being a one-, two-, or three-knower, the child represents other number words as quantifiers, meaning 'many,' where 'many' is more than any known number word. As I will argue below, it is likely that the nonverbal number representations that support the meanings of the known words is the system of parallel individuation (figure 2), with natural language quantification articulation in terms of notions like 'set' and 'individual.'

Meanwhile, the child has learned the count list, which initially has no semantic content other than its order. The child knows one must recite 'one, two, three, four, five,' not 'two, three, one, five, four,' just as one must say 'a, b, c, d, e,' not 'c, a, e, d, b.'

5 In a forthcoming book I argue that the same bootstrapping process underlying this marvelous feat in childhood also accounts for the development in historical time, but that argument is beyond the scope of this brief paper.

The stage is now set for a series of mappings between representations. Children may here make a wild analogy – that between the order of a particular quantity within an ordered list, and that between this quantity's order in a series of sets related by additional individuals. These are two quite different bases of ordering – but if the child recognizes this analogy, she is in the position to make the crucial induction: For any word on the list whose quantificational meaning is known, the next word on the list refers to a set with another individual added. Since the quantifier for single individuals is 'one,' this is the equivalent to the following induction: If number word X refers to a set with cardinal value n, the next number word in the list refers to a set with cardinal value n + 1.

This bootstrapping story provides different answers for how the child learns the meaning of the word 'two' than for how she learns the meaning of 'five.' According to the proposal, the child ascertains the meaning of 'two' from the resources that underlie natural language quantifiers, and from the system of parallel individuation, whereas she comes to know the meaning of 'five' through the bootstrapping process – i.e., that 'five' means 'one more than four, which is one more than three . . .' – by integrating representations of natural language quantifiers with the external serial ordered count list.

I began by sketching two systems of preverbal representation with numerical content: the analog magnitude system and the system of parallel individuation. You may have noticed that the analog magnitude system played no role in my bootstrapping story. It would be quite possible to imagine a role for this system in a slightly different bootstrapping proposal, and it may be that such a proposal would be empirically correct, at least for some children. We do know that children come to integrate their integer list with analog magnitudes, such that 'five' comes to mean both 'one more than four, which is one more than three . . .' and ' ———— ,' the analog magnitude symbol for the cardinality of a set of five individuals. This integration is undoubtedly very important; bootstrapping provides richer representations precisely through integration of previously distinct systems of representation.

As important as the integration of the integer list representation with analog magnitude representations may be, there is good reason to believe that this integration is not part of the bootstrapping process through which the concept of positive integers is first understood. Research suggests that it is not until *after* children have worked out how the count list represents number – in fact some six months later – that they know which analog magnitudes correspond to which numbers above five in their count list. That finding – along with the fact that the precise meanings of number words are learned in the order 'one,' then 'two,' then 'three,' followed by the induction of how the count list works – leads me to favor the bootstrapping proposal above.

I doubt that anybody would deny that language helps us occupy the distinctive cognitive niche that we human beings enjoy. It is obvious that culturally constructed knowledge is encoded in language and can then be passed on to new generations through verbal communication – you can tell your children something, saving them from having to discover it themselves. Still, this account misses the equally obvious point that children are often unable to understand what we tell them, because they lack the concepts that underlie our words. The problem then becomes accounting for how they acquire the relevant concepts they need to understand what we are telling them.

I have argued that bootstrapping mechanisms provide part of the solution to this problem. In thinking about how bootstrapping might work, we are led to a fuller appreciation of the role of language in supporting the cultural transmission of knowledge. We cannot just teach our children to count and expect that they will then know what 'two' or 'five' means. Learning such words, even without fully understanding them, creates a new structure, a structure that can then be filled in by mapping relations between these novel words and other, familiar concepts. And so eventually our children do know what 'five' means: through the medium of language and the bootstrapping process sketched here they have acquired a new concept.

Clark Glymour

The automation of discovery

Scientific revolutions are sometimes quiet. Despite a lack of public fanfare, there is mounting evidence that we are in the midst of such a revolution – premised on the automation of scientific discovery made possible by modern computers and new methods of acquiring data.

Consider, for example, the following developments:

- Using data from the 1970s, about eight years ago a team of data analysts working in Holland predicted that low-level lead exposure is more dangerous to children's cognitive development than had previously been thought – a prediction confirmed by recent reanalyses of later observations;
- Using measurements of reflected solar energy (technically, the visible-near infrared spectrum), a computer identified minerals in rocks from a California desert lake as accurately as had a team of human experts at the site who had access both to the spectra and to the actual rocks;
- In Antarctica, a robot traversing a field of ice and stones picked out the rare meteorites from among the many rocks;
- Scientists at the Swedish Institute for Space Physics realized that an instrument aboard a satellite was malfunctioning and they recalibrated it from Earth;
- An economist working for the World Food Organization found that current foreign aid practices have no impact on extreme poverty;
- Climate researchers traced the global increase in vegetation and its causes over the last twenty years;
- A team of biologists and computer scientists reported determinations of the genes in yeast whose function is regulated by any of a hundred regulator genes;
- A kidney transplant surgeon measured the behavior of rat genes that had been aboard the space shuttle;
- A biologist reported a determination of (possibly) all of the human genes in

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cells lining the blood vessels that respond to changes in liquid flow across the cells.[1]

All of these developments – and they are simply more or less random examples I happen to know – reflect a new way of learning about the world. Thanks to innovations in computer software, in laboratory techniques, and in observational technology, scientists today can measure things on a scale inconceivable only a few years ago. New laboratory and computational methods allow evaluation of vast numbers of hypotheses in order to identify those few that have a reasonable chance of being true, and simple oversights of human judgment can be corrected by computer. The change is from the textbook scientific paradigm in which one or a very few hypotheses are entertained and tested by a very few experiments, to a framework in which algorithms take in data and use it to search over many hypotheses, as experimental procedures simultaneously establish not one but many relationships. While there are consequences even for small collections of data, the automation of scientific inquiry is chiefly driven by novel abilities to acquire, store, and access previously inconceivable amounts of data, far too much for humans to survey by hand and eye. Methodology has moved in consequence; in a growing number of fields, automated search and data selection methods have become indispensable.

This may not seem revolutionary, but it has all of the earmarks of scientific revolution that Thomas Kuhn emphasized years ago: novel results, novel kinds of theory, novel problems, intense and often irrational hostility from parts of the scientific community.[2] We can see the revolution at work by looking more closely at three of the examples I mentioned above.

1 References can be found at <www.phil.cmu.edu/projects/DaedelusRefs>.

Lead was long a component of paint, and the Mobil Oil Company introduced tetraethyl lead into gasoline in the 1930s. From these and other sources, low-level lead exposure became common in the United States and elsewhere. Large doses of lead and other heavy metals were known to disrupt mental faculties, but the effects of low-level exposure were unknown. Besides, low-level exposure was hard to measure: low-level lead concentrations fluctuate in blood and do not indicate how much lead the body has absorbed over time.

In the 1970s, Herbert Needleman found an ingenious way to measure cumulative lead exposure using the lead concentration in children's baby teeth. He also measured the children's IQ scores and many family and social variables that might conceivably be relevant to the children's cognitive abilities. Reviewing the data by analysis of variance, a standard statistical technique introduced early in the twentieth century, Needleman concluded that lead exposure has a small but robust effect – it lowers children's IQ scores.

Since a lot of money was at stake, criticism naturally followed, and in 1983 a scientific review panel formed by Ronald Reagan's Environmental Protection Agency asked Needleman to reanalyze the data with stepwise regression, a more modern statistical technique. The idea behind this technique is very simple even if the mathematics is not: Suppose any of several measured variables might

2 Thomas S. Kuhn, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1996).

influence IQ scores. But start with the assumption that none of the variables influence IQ. Change that assumption by entertaining as a causal factor whichever variable is most highly correlated with IQ score, then keep adding causal factors by a mathematical measure that takes account of the correlation already explained by previously considered factors. Stop when additional variables don't explain anything more. (This procedure can also be run in reverse, starting with the assumption that all of the measured variables influence IQ scores, and then throwing out the least explanatory factors, one by one.) Needleman had measured about forty variables that might account for variations in his subjects' IQ scores, and stepwise regression eliminated all but six of them. Lead exposure remained among the causal factors, and using a standard method (indeed, the oldest method in statistics, originating with Legendre's essay on comets in 1808) to estimate the dependence of IQ score on lead exposure, Needleman again found a small negative effect.

Many years after the confirmation of Needleman's results had helped to eliminate lead from gasoline, two economists, Stephen Klepper and Mark Kamlet, reanalyzed Needleman's data – with a difference. Reasonably, they assumed that the measured values Needleman reported were not perfectly accurate: IQ scores did not perfectly measure cognitive ability; lead concentrations in teeth did not perfectly measure lead exposure; and so on. Each of Needleman's six remaining variables perhaps influenced cognitive ability, but the true values of those variables were not recorded in his data. The data consisted of measurements produced by the true value of each variable for each child, and also by unknown measurement errors. Klepper proved an interesting theorem that implied that for Needleman's data, with the assumptions about measurement error, the true effect of lead exposure on cognitive ability could be positive or negative or zero. The elimination of lead from gasoline, it seemed, had been based on a statistical mistake.[3] The story doesn't end here, however. But before continuing, a digression into the statistics of causality is necessary.

In the early 1980s, several statisticians developed a network representation of probability relations that formalized and generalized ideas that had been used for a long while in biology, social science, and elsewhere. According to their representation, suppose we have data for a number of variables, each of which takes a definite value in each individual object or case (the variables might be height, weight, ratio of Democrats to Republicans, whatever; the individual objects, or cases, could be people, rats, cells, state governments, whatever). Represent each variable as a node and draw arrows from some nodes to other nodes, e.g., C ← B → A. This particular diagram represents the claim that the information that the values of A and B together provide about the value of C is the same as the information that the value of B provides all by itself. And, symmetrically, the information that values of C and B provide about A is the same as the information that the value of B alone provides.

3 Herbert Needleman et al., "Deficits in Psychologic and Classroom Performance of Children with Elevated Dontine Lead Levels," *New England Journal of Medicine* 300 (1979): 389; Needleman et al., "Lead and IQ Scores: A Reanalysis," *Science* 227 (1985): 701 – 704; Stephen Klepper et al., "Regressor Diagnostics for the Errors-in-Variables Model – An Application to the Health Effects of Pollution," *Journal of Environmental Economics and Managemen*t 24 (1993): 190 – 211.

In other words, you can use the diagram described above when, for predicting C, if you know the value of B, then the value of A doesn't tell you anything more about the probabilities of the values of C. In more technical terms, C is independent of A *conditional* on B. (C would also be independent of A conditional on B if the structure were C → B → A or C ← B ← A, but not if it were C → B ← A or B ← C → A, etc.) The general version of this connection between networks and probabilities, known as the Markov condition, was introduced explicitly by statisticians around 1980, though it was used implicitly in many subjects long before that time, and almost formalized by the philosopher Hans Reichenbach in the 1950s. Without clearly formulating the general idea, biologists, psychologists, sociologists, and even biblical historians had used such diagrams to represent causal hypotheses and the probability relationships of their variables.[4] In the 1980s a group at UCLA, led by Judea Pearl, developed a fast algorithm for computing any conditional independence relations implied by the Markov condition when applied to such a diagram, now called a Bayes net.

In the early 1990s a group of philosophers and statisticians at Carnegie Mellon noted that many of the information restrictions, or conditional independence facts, represented in a network also hold in a related way if the arrows represent causal relations, and, relying on the Markov condition, they gave a general characterization of the relation between network structure, probabilities, and causal claims.

The idea is easiest to see for interruptions of a simple causal chain. For instance, if pushing the doorbell button causes the bell to ring, which in turn causes the house parrot to say "hello," then if you intervene to keep the bell from ringing, pushing or not pushing the doorbell button will not change the probability that the parrot says "hello." After your intervention, the state of the button and the state of the parrot will be independent of each other; neither will provide information about the other. But if you do not intervene to disconnect the bell, pushing the button will be independent of the parrot's speech conditional on the state of the bell, ringing or not ringing; if you know whether the bell is ringing, the parrot's speech won't give you any more information as to whether someone is at the door. In many cases, the independence relations produced by interventions in a system parallel the conditional independence relations implied by the network representation of the causal structure of the system.

These connections between causation, probability, and network representations suggested that with appropriate assumptions and background knowledge, something about the causal structure can be learned from observation, and the outcomes of some ideal interventions can be predicted. If C and A are independent conditional on B, and no other independence relation holds, then C and A are causally connected only through B, which functions either as a common cause or an intermediary. Inferences like this readily combine with other

4 See William Farmer, *The Synoptic Problem* (Macon, Ga.: Mercer University Press, 1981) and, for a more recent discussion, Donald Akenson, *Surpassing Wonder: The Invention of the Bible and the Talmuds* (Montreal: McGill-Queen's University Press, 1998). The statistical work is reported and illustrated in Harry Kiiveri and Terry Speed, "Structural Analysis of Multivariate Data: A Review," in Samuel Leinhardt, ed., *Sociological Methodology* (San Francisco: Jossey-Bass, 1982) and in Judea Pearl, *Probabilistic Reasoning in Intelligent Systems* (San Mateo, Calif.: Morgan Kaufmann Publishers, 1988).

information one might have – for example, if the same probability relations hold and B occurs before A and C, then the causal structure should be A ← B → C.[5] The old shibboleth that correlation does not imply causation is true for any pair of variables considered in isolation, but, when combined with otherwise routine assumptions, is not necessarily true for sets of correlations among several variables. The problem is to say in a mathematically precise and useful way just what causal information can be extracted from such dependencies, and under what assumptions.

The class of alternative networks that might conceivably describe the causal relations among a set of variables, before data is collected, is astronomical even for small numbers of variables, and with larger numbers of variables remains huge even if some of the variables are ordered so that one can assume that later variables do not influence earlier ones.

Even so, early in the 1990s, researchers at the University of Pittsburgh, Carnegie Mellon, UCLA, and Microsoft developed algorithms and software for searching for the class of diagrams that can account for any set of independence relations among variables. Since then many related algorithms have been proposed and applied by others. These procedures search efficiently for information within the huge space of alternative possible causal structures, but, unlike stepwise regression, some of these procedures come with a weak guarantee of reliability. For example, as the size of the sample increases, according to the Markov condition and one other further technical assumption, the Bayes net search programs 'converge' to giving correct information about the causal structure behind the data.[6]

Back to lead. In collaboration with Dutch statisticians, Richard Scheines, one of the Carnegie Mellon researchers, applied a program implementing these new search techniques to Needleman's data.

What the program found was simple but astonishing: three of the six prediction variables that had remained after Needleman's stepwise regression had *no* correlation with IQ scores – a fact that had somehow eluded Needleman, his collaborators, his critics, and, indeed, the stepwise regression procedure alike. Of the initial variables possibly correlated with IQ that Needleman had first considered, only lead and two other factors now remained. But, with the economists' assumptions about measurement error, the effect of lead exposure on IQ still could not be estimated.

To estimate the effect of lead, Scheines and his Dutch collaborators resorted to a relatively new technique in Bayesian statistics. Bayesian statistics proceeds by assigning 'prior probabilities' to alternative hypotheses, by computing for each hypothesis the probability of the data on the assumption that that hypothesis is true, and, from all this, computing a new, or 'posterior,' probability for each hypothesis or range of parameters considered. For a long time, because the posterior probabilities often could not be computed, Bayesian statistics was chiefly a toy used only for simple problems; computational developments in the last two decades have changed that

5 I oversimplify. For the general theory, caveats, and mathematical details, see Peter Spirtes et al., *Causation, Prediction and Search* (New York: Springer-Verlag, 1993; 2d ed., Cambridge, Mass.: MIT Press, 2000).

6 Ibid.; Clark Glymour and Gregory F. Cooper, eds., *Computation, Causation and Discovery* (Menlo Park, Calif.: AAAI Press; Cambridge, Mass.: MIT Press, 1999); Judea Pearl, *Causality* (New York: Cambridge University Press, 2001).

considerably. Scheines used the economists' judgments of the probability distribution for values of parameters related to measurement error to assign prior probabilities to their measurement error model. Then he and his collaborators computed the posterior probability distribution for values of the parameter representing the influence of lead on IQ. By this method, they found that low-level lead exposure is almost certainly at least two times more damaging to cognitive ability than Needleman had estimated.[7]

Genetics is another field in which scientists are conducting research in new ways by applying innovations in computer software, lab techniques, and observational technology.

Every cell in your body has the same DNA but cells in different tissues look and function very differently – brains, after all, are not bones. The difference comes from the proteins that make up the physical structure of a cell and regulate – indeed, in some sense constitute – its metabolism. The thousands of different kinds of proteins are themselves produced by a collaborative manufacturing process in the cell. Amino acids – any of twenty simple molecules provided to the cell from outside – are stitched together to form a protein, which may then fold and combine chemically or physically with other proteins. Each basic protein originates along a template of ribonucleic acid (RNA) outside the nucleus, and different template molecules – different kinds of RNA molecules – make different proteins. Messenger RNA (mRNA), itself copied from DNA, generates the template RNA. Whether a piece of DNA is copied into mRNA within any interval of time depends on several things, including the chemical sequence of the particular DNA piece (whether it is a coding sequence, i.e., a gene), the chemical sequences of other regions of the chromosome that are physically close (regulator sites), concentrations of small molecules inside the nucleus of the cell, and concentrations of proteins. Certain proteins attach to the regulator sites of a gene and cause the gene to be copied (in other terminology, 'transcribed' or 'expressed') into RNA, which in turn goes on to make proteins. An important clue to fundamental biology and its medical applications lies in this process of gene expression, in knowing which genes respond to new chemical or physical environments, and which cellular functions are influenced by the proteins those responding genes produce.

Traditionally, this kind of problem had been approached one gene at a time – for instance by finding some of the proteins that regulate a gene, finding the protein or proteins the gene yields, identifying some of the roles those proteins play in cellular metabolism. But about ten years ago, biologists developed techniques for simultaneously measuring the concentrations of each of the thousands – and in some contexts essentially all – of the distinct kinds of mRNA molecules present in a collection of cells. Biologists could get a snapshot of how much each gene in the cells had been copied or expressed. Multiple snapshots, moreover, could be taken at different times, as little as a few minutes apart, so that researchers could see the varying responses of the cell genome to changing conditions. So what affects what genes? Answers to this question are coming in at an astonishing rate.

About five years ago, Tim Hammond, a physician and research scientist at Tu-

7 Richard Scheines, "Estimating Latent Causal Influences," in Glymour and Cooper, eds., *Computation, Causation and Discovery*. Scheines's software – freeware – was the TETRAD III program, <http://www.phil.cmu.edu/projects/tetrad>.

lane, flew samples of kidney tissue in the space shuttle. When his samples, which had been chemically frozen while in microgravity, returned to Earth, Hammond and his collaborators measured the expression of thousands of genes within the tissue. They found that a large proportion expressed very differently from the genes within the Earth-bound samples of the same tissue, no matter how the Earth-bound tissue had been mechanically treated. Acceleration or low-gravity or something else as yet unknown about the shuttle environment affected gene behavior. If, as seems likeliest, the effect Hammond discovered is an essentially mechanical effect of low gravity, it has important implications for long-term habitation in space, on the Moon and Mars.

Mechanical issues – flow and sheer over cellular surfaces – are known to influence genes that are important to human health. The cells that line the surfaces of blood vessels play crucial roles in lethal disorders – for example, in aneurisms – and particular genes in these cells have been known for some while to change their expression in response to mechanical changes, in particular to changes in liquid flow across their surfaces. Very recently, David Peters, a young biologist at the University of Pittsburgh, and his colleagues measured the change in gene expression in response to changes of flow for almost all genes in living human cells lining blood vessels. In their experiment, more than a hundred genes changed, including some known to be involved in cellular structure. Peters and his collaborators are now measuring all of the genes in such cells that respond to changes in pressure and flow.

The few cases I have briefly described here are merely samples of a trend that can be seen in several sciences – a trend to which we can also attribute the Virtual Observatory that is planned to enable astronomers to search and analyze vast data stores taken by remote instruments; and, in climate studies, the Earth observation satellites that now send down several gigabytes of data each day– data that is increasingly being used to monitor the state of the planet, to locate causes of change, and to forecast changes in the environment. Ever new techniques make possible the measurement of ever larger quantities of data; data manipulation software makes possible the selection of samples that are relevant to particular problems; automated search and statistical techniques help guide researchers through the superastronomical array of possible hypotheses.

Kuhn said that scientific revolutions generally meet fierce resistance – and the automation of discovery in science is no exception. In some cases the animosity stems from nothing more than conservatism, an effort to preserve academic turf, or plain old snobbery. Above all, automated science competes with a grand craft tradition that assumes that science progresses only by scientists advancing a single hypothesis, or a small set of alternative hypotheses, and then devising a variety of experiments to test it. This tradition, most famously articulated by Sir Karl Popper, is championed by many historians and philosophers of science, and resonates with the accounts of science that many senior scientists learned in graduate school.

While the history of science can serve as an argument for norms of practice, for several reasons it is not a very good argument. The historical success of researchers working without computers, search algorithms, and modern measurement techniques has no rational bearing at all on whether such methods are optimal, or even feasible, for researchers working today. It certainly says nothing about the rationality of alternative methods of inquiry. Neither *was* nor *is* implies *ought*.

The 'Popperian' method of trial and error dominated science from the sixteenth through the twentieth century not because the method was ideal, but because of human limitations, including limitations in our ability to compute. Historically, novel methods and stratagems were devised from time to time to get round computational limitations. For example, in the eighteenth century, Leonard Euler, perhaps the most prolific mathematician ever, could not reconcile seventy-five observations because the calculations required far too many steps; statistical estimation of theoretical parameters, introduced by Legendre in 1808 in a form known as 'least squares,' permitted the reconciliation of (for the time) large quantities of data, such as the seventy-five that defeated Euler. The quick adoption of factor analysis in the 1940s was due in part to computational tractability, and one could argue that the same is true of the enormous influence of Sir Ronald Fisher's statistical methods.

When scientists seek to learn new, interesting truths, to find important patterns hiding in vast arrays of data, they are often trying to do something like searching for a needle in a really huge haystack of falsehoods, for a correct network among many possible networks, for a robust pattern among many apparent but unreal patterns.

So how does one find a needle in a haystack?

1. Pick something out of the haystack. Subject it to a severe test, e.g., see if it has a hole in one end. If so, conjecture it's a needle; otherwise, pick something else out of the haystack and try again. Continue until you find the needle or until civilization comes to an end.
2. Pick something you like out of the haystack. Subject it to a test. If it doesn't pass the test, find a weaker test (e.g., is the thing long and narrow?) that it can pass.
3. Try 1 for a while, and if no needle turns up, forget about needles and start studying hay.
4. Try 1 for a while, and if no needle turns up, change the meaning of needle so that a lot of 'needles' turn up in the haystack.
5. Set the haystack on fire and blow away the ashes to find the needle.
6. Run a magnet through the haystack.

Method 1 is still the standard description of how science is and should be conducted – the account we find explicitly in the introductory chapters of science textbooks and implicitly in the criticisms some scientists and methodologists express toward other ways of doing things.

Method 2 is practiced and effectively advocated by many social scientists (you need only replace 'something you like' in 2 with 'theory').

Methods 3 and 4 are the practices that postmodernists claim science does and should follow.

Methods 5 and 6 are those made possible by the automation of discovery.

In principle, methods 5 and 6 are a lot smarter than the other methods, but they are not without limitations both real and metaphorical. Burn the whole haystack and you might melt the needle. And that is a sound worry about automating science: it may rush things, sometimes too much. Because a procedure for finding hypotheses is fast and can be done by computer doesn't mean the procedure gives good results. Figuring out what a method can and cannot reliably do requires hard work.

Consider for example the problem of identifying networks of gene regulation. The ability to measure gene expression simultaneously for thousands of genes in normal and perturbed genomes (in perturbed genomes, particular genes have either been deleted or forced to over-express) invited the application of computer methods that search for causal networks. Algorithms were proposed for piecing together networks from comparisons of gene expression measurements in cell lines with perturbed and unperturbed genomes; algorithms were proposed for finding networks from correlations with repeated measurements of expression levels in unperturbed networks – and they did very well on data produced by computer simulations of gene expression.

It turns out, however, that much of this work proved to be illusory. The algorithm for assembling a network from perturbation effects was incorrect. The algorithms for inferring networks from correlations of gene expressions overlooked the fact that measuring expression levels in aggregates of cells (rather than in individual cells, which is technically feasible but rarely done) creates correlations due entirely to the aggregation itself rather than to the influence of particular genes on the expression levels of others. The simulations that seemed to work so well also turned out to be simulations of measurements at the level of individual cells – measurements of a kind usually not made in reality. Undoubtedly the automated procedures got some things right, but very likely what they got right was cherry picking – gene connections indicated by very large changes in expression levels or very large correlations.[8] A real advance in unraveling gene regulation networks came recently – by chemical rather than by computer automation. Tong Ihn Lee and his colleagues found a way to identify a large fraction of the genes in yeast that are, in turn, directly regulated by genes known to be regulators. They did so for more than a hundred regulator genes, effectively identifying a good piece of the regulatory structure in 'wild type' yeast.

The automation of learning, whether by computer or by new laboratory techniques, does not render human judgment obsolete, or marginalize scientific creativity. Nor does it cheapen the sweat and effort, the insight and ingenuity of human scientists, but shifts them toward the consideration of algorithms that can efficiently and reliably compare many hypotheses with vast quantities of data and toward laboratory methods that answer many questions at once.

8 Tiajaio Chu et al., "A Note on a Statistical Problem for Inferring Gene Regulation from Micro-array Data," *Bioinformatics* (in press); David Danks et al., "Experimental Determination of Gene Regulation Networks: Complexity and Statistical Realism" and Frank Wimberly et al., "Experiments on the Accuracy of Machine Learning Algorithms for Discovering Gene Regulation," in the *Proceedings of the Workshop on Bayes Nets and Gene Regulation,* International Joint Conference on Artificial Intelligence, 2003.

Poem by David Ferry

October

The day was hot, and entirely breathless, so
The remarkably quiet, remarkably steady leaf fall
Seemed as if it had no cause at all.

The ticking sound of falling leaves was like
The ticking sound of gentle rainfall as
They quietly fell on leaves already fallen,

Or as, when as they passed them in their falling,
Now and again it happened that one of them touched
One or another leaf still on the tree,

Still clinging to the idea of being summer:
As if the leaves that were falling, but not the day,
Had read, and understood, the calendar.

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Fiction by Chuck Wachtel

The Annunciation

The bear in the driver's seat wasn't made of flesh or any other three-dimensional substance, but of light and color, like characters in animated cartoons. The car it drove had approached him from behind, pulled nearer to the sidewalk, and slowed to the pace of his walk. The bear was purple, except for its ears, nose and mitten-like hands, which were red, and as tall as a human, though plumper around the torso and neck. Holding its eyes fixed to the street directly ahead, it maintained the same slow speed just long enough for him to catch a glimpse of the only other passenger: a little girl in a yellow dress, her legs extending to a point just beyond the edge of the backseat, her toes up, one foot turned slightly inward. She was wearing blue and white high-tops; the colors were bright and clean, and because she was too young to walk on them, they hadn't a trace of wear. When the car suddenly sped up and turned at the corner, he became angry and frightened. He woke then, with Nan's hand on his chest.

"You all right?" she asked him. "You said something, kind of, and you were rocking back and forth."

"What did I say?"

"It was like a whole sentence, but it didn't really have words, just sounds."

It had been six days since the morning he sat beside Nan, lying on an examination table, and watched, on the screen of a sonogram monitor, a thin tube enter her belly, then come so close to the fetus inside her that the small hand actually reached toward it. "They do that," Dr. Gisse said, as he affixed a syringe to the end of the tube and withdrew a sample of amniotic fluid. A moment before that the sonographer, an unshaven man wearing a surgical cap, had been impatient with Nan who'd begun to shiver and cry. It was the kind of casually dramatic impatience meant to tell the person it is aimed at that they have made your day harder.

"What's your fucking problem?" he said to the sonographer, in one angry breath.

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Fiction by Chuck Wachtel

"Johnny," Nan said, as if the man wasn't there. "Look," she nodded toward the screen.

"They've been through this once before," Dr. Gisse said to the man, who looked back at Johnny, but not at Nan – having understood the unspoken portion of the statement – and gave a nod that constituted an apology.

A nurse came in, labeled the vial of amniotic fluid and held it up to Nan. "You identify this as your name?" The question was part of the same litigation-prevention protocol they'd gone through the last time. Nan hesitated and the nurse looked at Johnny for help. Johnny lifted his gaze to the red-lit exit sign. Two of the four screws that held the plate with the letters to the frame of the fixture were missing and it tilted a degree or two downward to the right, revealing a thin dash of white light over the red *I* and *T* that made him think of the diacritical line that means a vowel should be pronounced as it is spoken when not inside a word.

"My last name is Wilk," Nan said. She'd kept her own name when they got married.

"But on the chart it says Rizzotti," the nurse said.

To Johnny the two missing screws seemed cognate with the sonographer's lack of manners and unshaven cheeks. *Ex-eyet*, he said to himself, without even moving his lips. *I'm ready to head for the ex-eyet*.

"We use my husband's plan," Nan explained.

The nurse pulled a strip of labels from the pocket of her smock.

"The post office," Nan said, then paused as she watched the nurse write her name on a label, peel it off the strip, and wrap it around the vial across the part with Johnny's last name.

"The post office?" the nurse asked her.

"The post office?" Nan asked her back.

"I work there," Johnny said.

"And it has the best medical plan," Nan said, "in the whole damn country."

The day after Dr. Gisse's assistant called with the results – normal, a girl – they discussed how they'd announce the good news to the friends and relatives whom they hadn't told about the pregnancy. Nearly all of their family, on both sides, lived at a distance, and Johnny and Nan had laid low during the last weeks prior to the amniocentesis, at which time she'd begun to show. The few friends and neighbors and coworkers who'd figured it out were sworn to secrecy. Two years ago, when they'd learned Nan was pregnant the first time, they told everyone, even strangers, and the most difficult part was untelling them, undoing what the world around them was still expecting to happen.

Since the day the at-home EPT test affirmed their second pregnancy Nan had kept the test wand in a Ziploc bag in her sock and underwear drawer, and so, the next afternoon, before she got home from work, Johnny set their huge volume of the works of Leonardo da Vinci on the living room floor, opened to the Annunciation they had seen at the Uffizi Gallery while on their honeymoon in Florence, and laid the wand across the space between the hand of the archangel Gabriel, with two fingers gently raised, and the serene yet startled eyes of the Virgin. Johnny then knelt over his composition with his thirty-five-millimeter camera, and from various angles and distances, and at slightly different foci, shot two rolls of color film.

Eight years before, when Nan led Johnny across the huge echoey, marble-walled room to the painting, she had said, improvising on an ad for Kentucky

Fried Chicken, "When it comes to angels, nobody does wings like da Vinci." Johnny's composition, reproduced as a postcard, would make a unique announcement, a revelation of the knowledge they had kept to themselves for more than four months.

The next day, on his lunch hour, Johnny picked up the two rolls at the one-hour photo counter at Rite Aid. He opened the envelopes in the checkout line and by the time he'd flipped halfway through the second stack of photos his anticipation had eroded to disappointment: the collage he had constructed, that had looked perfectly clear through the camera lens, was unrecognizable in the images he held before him. The flash had bounced off the page where it curved above the spine like a wave of parted hair, spilling a wide oval of white light across half the photograph and leaving the other half too dark to identify anything.

Later that afternoon, before Nan came home from school, he'd shoot another roll from different angles in the consistent, nonviolent light of the overhead lamp.

Although the next batch didn't come out much better, there were three shots in which all the component parts were identifiable. If you knew what an annunciation was, you would know this was one; the implausible object lying across the composition was recognizable as an EPT wand and, most importantly, the red line that bisected the positive box was clearly defined. It was time to show them to Nan, who had much more experience photographing art – she was a professor of art history at City University – and get her advice for the final shoot. He left the three best ones faceup on the kitchen table to see how she'd react to them when she came home from work.

During the last two days they'd been granting entry to feelings they'd held at abeyance for months. They'd reached the top of a mountain so steep that the labor of climbing had kept them from taking notice of the scenery. Now they'd stroll down the other side, enjoy everything, let gravity do the work. "Even so," Nan had said, thoughtfully, "innocence lost is never regained. And guess what?" she had begun to laugh. "I could give a shit less." That morning when he awoke, Nan was sitting up, leaning against the wall on her side of the bed, watching him sleep. "You know what I just realized?" she said. "We've been pregnant more than nine months combined, and now, finally, we're in control." Her exhilaration and certainty frightened him, but he was much too happy to be worried about anything. "Now we're in control," she repeated. "*We* control the horizontal. *Do do do do,*" she sang the first four notes of the theme from *The Twilight Zone*.

"That's the wrong show," he said. "It's *The Outer Limits* where they control the horizontal."

She slid her hand under the blanket, gripped his penis. "And we certainly control the vertical."

After they made love – the fifth time in two days – Nan laid the back of her head on Johnny's stomach and slid her feet up the wall. "I'm telling you right now, there'll be none of that textbook-sentimental-story-to-tell-later crap. No cravings for ice cream or shrimp dumplings, no belly-hiding muumuus, no sudden mood swings, no sentimental platitudes, no storks on the birth announcement – no fucking storks anywhere."

Johnny was sitting in the living room, trying to read the paper, when he heard the door to the apartment open, then the sound of Nan's footsteps crossing the kitchen, the clunk of her shoes, one after the other, hitting the floor, and the whoom of the bathroom door being pulled shut, followed by the clack of the door hook striking wood.

He walked into the kitchen. Her briefcase was on one of the chairs and a takeout bag with a widening grease blotch on its side was sitting on top of the photos. Johnny moved the bag across the table, and slid the photos to the side she would approach them from.

"Not a spot," Nan said, opening the bathroom door. "Not a spot all day."

She had been spotting since the fifth week of the pregnancy, and though they had reached the middle of the second trimester, it still hadn't stopped. Dr. Gisse told them it probably wasn't anything to be concerned about. He told them they worried too much about everything, "But don't worry about *worrying*. That's not unusual after what happened the last time." *The last time*, when the call came, they were sitting in front of the TV, watching *Jeopardy*, eating dinner. How could anything real happen at such a moment? The genetics counselor told them he waited until evening to make such calls, when both partners would most likely be at home: *trisomy 21*: Down's syndrome: three of the twenty-first chromosome instead of two, forty-seven in total instead of forty-six: *odd*, two parents, two of everything: odd numbers are bad news in genetics. It would have been a boy.

Johnny took Nan's briefcase off the chair and motioned, like a maître d', for her to sit. "What do you think?" he asked when she looked down at the three photographs. She picked one of them up but still said nothing.

He could no longer wait. "Da Vinci's *Annunciation*. And that's *our* EPT test."

"I get it," she said, "but I didn't get it fast enough."

"I thought we could take a better shot, then make a postcard. *Nan and Johnny have an announcement* . . ."

"At first I thought it was some kind of weird submarine," Nan said.

"Not in a better photograph. That's where you come in."

Nan started laughing. "I like it. I like that you want to tell everybody. I do too."

"I think it's a work of art," Johnny said.

Nan opened the bag and began setting the takeout containers on the table. "I'm starved," she said. "Although the Virgin conceived in a very different manner than I did, I know this: as her belly got bigger, her appetite got bigger."

"Maybe it's a good thing," Johnny said. "That it slowly reveals itself. I mean, that's how art works, no?"

Nan had been right about this pregnancy not being ordinary. Although they felt the anxieties of people becoming parents for the first time, they felt, even after reaching the point of being pregnant longer than they'd been before, that they would never feel the newness, the constant surprise, that they remembered.

Once the news was out, Johnny's mother, who lived in a senior housing apartment in Florida, called often, usually to talk to Nan. When she called on the morning of Johnny's birthday, near the end of the second trimester, he was in the shower.

"We talk while I wait for him," she told Nan. "And I tell you about forty-five years ago today when I *didn't* have to wait. He was in such a rush I still had my shoes on."

In the last weeks Nan had grown tired of her mother-in-law's voice, annoyed at the endless childbearing stories from three generations of Johnny's family told as if they were instructions for how to conduct her own pregnancy. But this was the first time she had spoken of Johnny's birth.

"His father was at work, so when my time comes, my brother, Gianfranco – you know Johnny was named for him – drives me to the hospital. I'm seventy-four but I remember like it was last week. We had to pass through the old neighborhood in Brooklyn and when he stops the car in front of Sal's Fish Market I know what he has in mind.

"'I'll be quick,' he says. 'You wait in the car and we don't get a ticket.' What am I gonna say? Since he never got married *bacala* was all he ever thought about."

"Then we get to the hospital. Like a gentleman now, he opens the car door and as soon as I stand up, it's Niagara Falls under my dress. Forty-five years ago today. I tell the doctor I can't stand up my back is hurting so bad and when they put me on the rolling thing I tell him, 'No, no, I can't lay down neither.' The doctor examines me right there, we're not even in the room yet, and he says, 'Why'd you wait till now?'

"'In a car out front,' I tell him, 'there's five pounds of cod fish that'll answer your question.'"

"That's something," Nan told her. "Wow."

"Every year on his birthday the first thing I remember is getting out of that car. That's when it hurt, I can't tell you how much. That's when I say to myself, he's gonna get born – even then I knew he was a boy – even if I'm gonna die."

"Oh, he's dressed," Nan said, waving Johnny into the room, "and he's about to leave for work."

"That's all right," she said. "You just tell him I said happy birthday."

Two months had passed since Johnny showed Nan his photos for the announcement and they still hadn't pursued the idea. By this time the few people who hadn't been told had gotten word from those who had. The influx of notes and cards and phone messages hadn't tapered off and now baby gifts had begun to arrive.

One evening Nan walked in looking pale and exhausted. Their plan was for her to take the next semester off, but there were still four weeks left in this one.

"You're working so hard," Johnny told her. "I wish there was a way you could just stop now. Couldn't they get a substitute or something?"

"How dare you," she said, anger flashing in her tired eyes.

"What?"

"How dare you accuse me of being lazy?"

"You got it all wrong."

"*You're* the one who got it wrong, buddy."

Johnny walked into the living room, sat on the couch, picked up the remote, and turned on the TV. He stared at the Weather Channel, listened to a few bars of the soft jingly music that accompanied the five-day local forecast, then got up and walked back to the kitchen.

"Does your mother ever sleep?" Nan asked. There were tears in her eyes now.

He remained standing in the entryway.

"Last night some movie star told David Letterman that while she was pregnant she had the uncontrollable urge to eat flowers. Daisies especially. Eight o'clock in the morning your mother calls because she has to tell me this. Plus she keeps suggesting names. This morning's suggestion was Ricardia, *her* mother's name. 'Doesn't Ricki sound nice?' she said. How many times do I have to tell her we're not discussing names yet?"

"That movie star, did she eat them?"

"I can't get enough sleep."

"I'm sorry," Johnny said. "Next time let the machine answer."

"You are such a gaping asshole."

He walked back into the living room, turned off the TV, and lay down on the couch.

Five minutes later Nan came in. He lifted his legs as she sat down, then lowered them onto the arm of the couch so they crossed the space above her lap like the safety bar on a Ferris wheel seat.

"And you can tell your mother that we're not doing to *our* daughter what Italians do to little girls."

Johnny laughed at this, though not too much, since there had been no acknowledgment that the fight was over. She was referring to an argument they'd had during the last pregnancy – during the wait for the amnio results – something they had not spoken of since. The sonogram image had given some evidence that it was a boy. If the results confirmed that, Nan wanted him to be circumcised. Johnny did not.

"He'll automatically be Jewish since you are," Johnny said. "It's matrilineal. You told me that."

"I did."

"So can't he be Jewish without having his little dick whacked?"

"Anatomically, he should look like his father," Nan argued. Johnny had been circumcised in the hospital, as had most male babies of his generation, and had never given it a thought. However, the idea of having it done to his son had caused him to imagine the pain for the first time: it would be as if it were happening to him all over again. He began to envision the cutting of the foreskin as an ongoing, constantly repeated process, like the bound Prometheus's liver being eaten by an eagle, only to grow back again overnight, then to be eaten again by the same eagle, from his ripped-open torso.

"And what about the thing Italians do to girls?" Nan had said, smiling, but tired of his persistence.

"What do they do to girls?"

"You know what I'm talking about."

"I have no idea what you're talking about."

"Prenatal ear piercing?"

Nan slid Johnny's legs off her, turned so she could lay her head on the side of the couch, then laid her feet on his lap. After letting her weight settle she grunted, arched her back, lifted her heavy torso just high enough to slip her hand underneath, and pulled out the remote. She lowered herself back onto the cushions, turned the TV back on, and skimmed the channels. She stopped at a shot of a beautifully pure blue sky, which held only for a second before the camera dropped and found two teenagers, a boy and a girl, leaning against the fender of a car. They were contemporary teenagers, but the car was a vintage, late-sixties Corvette convertible, bright red. They appeared tired; they were sad and a little bored, yet sexy in an adult way.

"You were *with* her," the girl said, energized by her anger, though sleepy-eyed.

The boy turned his head away.

The girl, wearing dark red lipstick, looked briefly at the camera, pouting, then slowly lowered her gaze.

The boy turned and directed his eyes downward, toward whatever the girl was looking at.

"I bet it's going to be Pepsi," Nan said.

"I was just . . . ," the boy said, then paused. "I was just . . . *there*."

The camera slid down their slender bodies. They were both wearing jeans. One of the knees on the boy's was ripped, showing the pale skin beneath.

"The Gap," Johnny says. "Five bucks says it's the Gap."

Nan, still holding the remote, turned off the TV before they could find out.

Three weeks before their due date, Johnny woke to the sound of Nan crying. He reached toward her before he even opened his eyes and found her side of the bed empty. She was sitting in the chair across the room, leaning over, her elbows on her knees. "Nan," he said, then, "What?" He was afraid something had gone wrong, or that she'd gone into labor early, but he knew, in the first instant of full wakefulness, that it would be best if he didn't appear frightened. He responded as if the loud sobbing that had penetrated his sleep was a question he hadn't fully heard or understood. "What?" he said again, softly.

"It's four-thirty in the morning," she said. Her breaths were sudden and shallow, with a faint trace of voice in them.

"I thought your idea for the pregnancy announcement was terrific," she said. "It *was* a work of art. I'm sorry we never made the postcards."

"Who cares? Nan, are you all right?"

"I need something," she said. She seemed angry now. "Why did your mother tell me about that stupid fucking movie star?"

"What do you need, baby?"

She started crying again, harder.

"Nan?"

"Flowers." She said this between gasps, in a whisper.

"What?"

She covered her eyes and shook her head. Johnny helped her back into bed, then held her in his arms. "You want flowers?"

"Marigolds," she said. "I keep thinking of the thick part in the middle." Her breathing was slower now. It seemed she might even be falling back to sleep.

He slid out from under her weight, then got out of the bed and stood beside it. He pulled the blanket over her, leaned down, kissed her hair.

Just north of Houston Street, he found a greengrocer that was still open, but there were no marigolds, only blue daisies that looked like they'd been watered with dye, ordinary yellow daisies, and roses that looked morbid and inedible. Before he headed toward the twenty-four-hour greengrocer on Avenue A, he bought the yellow daisies, just in case he couldn't do better. According to the thermometer on the Emigrant Savings Bank it was nineteen degrees Fahrenheit, minus seven Celsius.

He had met Nan in his first and only year of graduate school. She was a student in the freshman composition course he taught. At the end of that year, a cut in federal funds had forced nearly all teaching assistants at CCNY to be laid off. So Johnny, along with several other graduate students, quit school in what was both a statement of protest and an act of necessity: he could not afford to continue without the teaching assistantship that had paid his tuition. The first job he found was at the post office. One afternoon, more than eight years later, Nan handed him a yellow slip at the parcel pickup and information window.

"Mr. Rizzotti?"

He didn't recognize her. He assumed she saw the name on his ID tag. "You gave me an A. My first in college. I never thanked you."

In his entire adult life he had never felt anything like what he was feeling now, walking east through the predawn morning. He'd carried mail for four years, and drove a mail pickup route for three more before becoming a supervisor. You could see the city in a million ways: during his workday he saw it as a complex chain of mailboxes, with the rest – the buildings, the cars and trucks and people – slightly out of focus. Now he saw this neighborhood, the one where their daughter's first home will be, as a constellation of twenty-four-hour greengrocers, their lights glowing like stars. He did not feel the cold. He only felt a steady current of elation. They'd passed all the danger zones, now all they had to worry about was adolescence and college tuition. He couldn't wait to meet his daughter. He couldn't wait.

When he walked in Nan was sitting at the kitchen table with a pencil in her hand.

"Since I was up," she said, emphatically casual, "I thought I'd get some work done."

He set an array of cone-shaped bouquets, wrapped in gift paper or clear plastic, before her, covering the entire tabletop, including the student paper she'd been reading. Among them were two batches of marigolds. He'd got the second batch at the last place because their tops were bigger than the ones he'd already bought.

"Is it cold?" she asked, no trace of what she was feeling before he left in her voice or eyes.

He began unwrapping each bouquet. When he laid them back down, they lost their shapes. It was as if he'd amassed an entire flower garden on the table before her. "See anything you like?"

She picked up three of the bigger marigolds – the soft, orange centers inside the dense corollas of small petals were as big as marshmallows – and held them out to him. "You first."

"No," he said. "You."

She moved the flowers closer, but did not speak a word.

He shrugged, slowly leaned forward, and took the head of the largest one in his mouth.

She suddenly began to cry.

"Nan," he said.

"This is crazy," she said, anger filling her voice.

He did not know how he knew, but he knew what had happened. "That was the one you wanted," he said. "Wasn't it?"

"Shit," she said. "How dare you?"

"Was it?"

"You don't understand."

"Was it?"

"You're a man. A *mail*man. How could you fucking understand?"

He was furious, but knew he was still happy underneath. "Tell me, professor. What the hell is wrong?"

"I married an idiot," she said. "That's what's wrong."

When the baby was six days late, Dr. Gisse sent Nan to the hospital for a nonstress test. Unfortunately, it was done in the same clinic as their second amniocentesis.

As soon as they entered the ultrasound examination room, his eyes found the exit sign. He was relieved to find that the missing screws had not been replaced: changing anything in that room might indicate a change in their fate, perhaps for the worse.

Though they had a hard time recognizing the parts of her anatomy, the baby appeared fine. At one point, the sonographer – a different one, a woman – told them that the baby had just moved a foot and a hand to her mouth, and pointed with a little plus-sign-shaped cursor to where this was happening.

"Does that mean she's hungry?" Johnny asked.

"It could," the woman said.

"Oh Hannah," Nan said, trying out the name they had chosen, softly curving the second syllable downward.

"What an appetite," Johnny said, shaking his hand Italian style, then began to weep.

Everything looked fine, but nothing would be certain until they got the results of the second test, which involved Nan sitting in a room with other beyond-due-date mothers, each with a fetal monitor strapped to her belly, while various electronic bleeps recorded the baby's movements and vital signs and her own mild contractions, most of which could not even be felt.

Two days before the last pregnancy had been terminated, the doctor inserted a branch-segment of laminaria, a kind of seaweed, into Nan's cervix, to dilate her in preparation for the abortion. Once that was done there was no reversing the process. The following day he would remove the insert and replace it with two branch-segments, widening the cervix further. Those two days, during which Nan experienced the symptoms of early labor, were even darker than the previous two weeks, when they had lived each day with the news. They'd had to make a decision, as parents, as non-parents, and perhaps the most difficult part was accepting that the decision had already been made, and that it resided inside them, always had, and would continue to, long after the pregnancy was terminated.

On the first of those two visits, the doctor told them there was a possibility, though an unlikely one, that they would encounter anti-abortion activists on the morning of the procedure. Legally, they're not permitted to approach anyone, he said, not even be on the same side of the street, but anyone can walk into the waiting room, and there's no telling who someone could turn out to be. Records are confidential, but they have ways of finding out when late-term abortions are scheduled.

"They know," Nan had told him. "I think they knew before I did." Less than a week after getting the amnio results, a pamphlet had arrived in the mail with a photo of Down's children sitting in a circle around a teacher, smiling and clapping their hands. Though the envelope it came in had a post office box as a return address, they thought it was the information they'd asked the genetics counselor to send. The tone of its introduction was sympathetic; it offered hope in the form of knowledge. The persuasion didn't assert itself until the second page, which began with the words *Search and Destroy*, an anti-abortion catchphrase for amniocentesis.

In the remaining two days of Nan's pregnancy, Johnny would have fantasies so real they lifted him entirely out of the moment, out of the abrasive, fast-slow dream of time: on the street, or in a hospital corridor, a crowd of strangers would approach him and Nan, and even before they spoke he would know they were the people who had sent that pamphlet. He would lunge into them shouting and throwing punches. He would not stop until he had hurt them all.

During that same visit the doctor also told them that in second-trimester abortions there are remains, and that now might be a good time to think about how they wanted to handle them. The hospital could take care of it; forms would have to be signed. Or they could choose cremation, even burial. His voice implied, warmly, that it would be best to not make too big a deal of this part, to begin leaving the past behind as quickly as possible.

On the morning of the procedure they avoided the waiting room entirely. After helping Nan into her hospital gown, Johnny waited in the hall outside the recovery room, along with two Orthodox Jewish women who stood facing the wall that separated them from the ward in which their loved one would awaken, once whatever was being done to her had been done. One of them opened a small book and held it between them. They began to rock gently, chanting softly in Hebrew: the rhythm of their praying was the only thing that enabled the minutes to pass. They continued to pray when the doctor came through the door from the recovery room, his green surgical mask hanging loosely from his neck, and approached Johnny. Everything had gone smoothly, he said. Nan would be awake in a minute or two.

Johnny felt relief. It was as if the weighted matter within his own body, relentlessly subject to the pull of gravity, had been removed. For a breath's time it was over, but he knew that the next moment would begin a process of unbearable mourning. The doctor stepped into the elevator. The two women had now stopped praying, and before Johnny walked through the door into the ward he told them that he hoped the patient they were praying for would have a full and speedy recovery.

A week later, when their taxi arrived at the Upper West Side funeral parlor where they were to pick up the ashes, they had to wait while the three limos ahead of them discharged their passengers at the edge of the long awning that reached to the curb from the wall above the entrance. It wasn't until they had stepped out that they noticed the police barricades holding back a crowd of onlookers on both sides of the street, and the network news trucks with telescopic antennae on top parked across the street.

Johnny took Nan's hand, and they walked at a quick, deliberate pace. He had no idea what was going on, and as they approached the entrance he imagined the things he'd shout at a police officer if one tried to stop them. He half hoped one would, but no one approached them. They were walking through a different dimension: no one even noticed they were there.

They climbed three steps, walked through the entryway and into a wide rotunda where a woman walking toward the exit came between him and Nan. The rotunda was filled with people, most of them standing in groups, talking, and when Johnny reached the middle of the room he discovered that Nan wasn't beside him. He spun around and caught a glimpse of her walking into the office. At that moment he realized that Nan was never more in the world than when she was pregnant, yet her grief had caused her to withdraw from it to such an extent an onlooker's casual gaze could not detect her presence; he, a father who hadn't been able to protect his child and his wife from danger, was cloaked in his own helpless anger.

The most direct route to the office Nan had just entered took him between two men who were facing each other, perhaps two feet apart, talking. They looked familiar, and as they stepped back to allow him to pass, he was certain he recognized them both.

When he walked into the office, the man sitting behind the desk rose, approached him, and without introducing himself, motioned Johnny to one of the two seats facing his desk, and said, "Your wife is in the rest room." The man apologized for the crowd, pressed the fingertips of both his hands together, looked down at the desktop, and said nothing more.

On the cab ride back downtown Nan examined the white cardboard canister, the same size and shape as a container of Quaker Oats. There wasn't a word or number to identify what it contained.

"How do they know it's ours?" she asked, then said, "Give me your keys."

With the penknife on his key chain she cut the tape encircling the middle, holding the top and bottom halves together. She tried to open it, but couldn't get her nails into the small space between the two parts.

"Let's wait till we get home," Johnny said.

Nan turned, looked out the window, and said, impatiently, "We're only at Sixty-eighth Street?" and then, as if it were part of the same thought, "It sounds crazy, but I think I saw Phyllis Diller coming out of the bathroom in the funeral parlor."

Johnny suddenly realized that the two men he had walked between, less than fifteen minutes ago, were the ex-mayor David Dinkins and the comedian Alan King. Nan and Johnny had been left no room for curiosity, no interest in looking through the window it opens on the proximate world. The enormous crowd, the police, the news trucks, were just there. "I bet it was her." Johnny said.

A few blocks later Nan tried again, and was still unable to open the canister. This time she handed it to Johnny. He held the bottom, and with both hands she loosened the top. Inside was a small plastic bag that contained less than a handful of pebble-hard, gray ashes, and a scorched metal ring, perhaps an inch in diameter, with the number five stamped on it.

Nan closed it, embraced it and stroked the smooth cardboard.

When they got home Nan fell asleep on the couch and remained asleep for the rest of the afternoon. After sunset Johnny went out to buy soup for their dinner and on the way back noticed, on the front page of a *Daily News* on top of a stack at a newsstand, a picture of the front of the funeral parlor and the headline: NEW YORK'S BEST, BRIGHTEST AND FUNNIEST SAY FAREWELL TO HENNY YOUNGMAN.

A little girl, sitting on the carpet in the waiting room, had set up in front of her a collection of plastic dinosaurs along with a Barbie doll in a hula skirt, and a small stuffed bear. The bear was purple and red and reminded Johnny of the chauffeur bear in his dream, which he did not remember as a dream, but as something that actually happened a long time ago. The girl's mother, who was pregnant, was seated in one of the rows of chairs across the carpet from where Johnny was sitting, holding a smaller child asleep on her lap.

A copy of *People* magazine lay faceup on the seat beside him with a photograph of Vanna White, in a strapless, floor-length evening gown, on its cover. The little girl held up the hula Barbie so Johnny could see it, and moved its arm to wave hello. Johnny smiled and waved back. She was a beautiful child, no more than five years old. She wore thick glasses and had a yellow Band-Aid on her forearm covered with stars and planets.

The elevator door opened, and both he and the girl watched as a man wearing a business suit and yellow tie stepped out, crossed the room, lifted the *People* magazine off the chair, and sat down beside Johnny.

"It's raining," he said. His damp suit jacket smelled like cigarette smoke. "Your wife in there?" he asked Johnny, who nodded.

"Mine, too. This your first?" Without waiting for an answer he said, "I already have a six-year-old boy." He lifted his feet, one at a time, and inspected his shoes, top to bottom. "She was two weeks late with him. I hope we don't have to wait that long for this one."

The man fell silent then, and opened the magazine. Johnny looked back at the girl who had arranged the dinosaurs in rows as if they were an audience facing the bear and the biggest dinosaur, a brontosaurus.

"You know what I hate about *Wheel of Fortune*?" the man said to Johnny. He was holding up the magazine, pointing to the picture of Vanna White. “I hate it that some really nice person, someone smart and nice, can get all the letters except like maybe one or two, and then they go bankrupt.” He shook his head. He seemed genuinely angry. “And then some idiot dipshit who can barely read gets the answer. Ever see that happen?”

Johnny shook his head.

“That’s what happened last night.”

“Last night?”

“The clue was *Theater complex of New York and home of the Metropolitan Opera*. Know what it is?”

“I don’t think so.”

“Lincoln Center. The dipshit wanted to buy a vowel but they were all filled in already. You could see him moving his lips as he sounded it out. The only letter not there was the r and I swear, at one point I thought he was going to say Lincoln Continental. He got it just as the buzzer went off, and this sweet, smart young lady goes home with the parting gifts. You know, like carving knives and tickets to some shitty musical and dinner for two at a restaurant where the food’s so bad they have to give it away.”

Just then a very pregnant woman passed through the doorway leading out of the examination rooms. She smiled at the man beside Johnny and gave him a thumbs-up.

“All Raaaaiight!” he said, then got up from his chair, met the woman as she crossed the room, leaned over, and kissed her protruding belly.

“And of course he didn’t win the bonus round,” the man said to Johnny as he was helping his wife into her coat. “The dipshits never do.”

The hula Barbie waved at Johnny again, and this time he got up, walked over to the girl, and sat on the carpet beside her.

“It’s a wedding,” she said. He now understood the arrangement. The brontosaurus and the bear were the bride and groom. She pulled off her Band-Aid and pressed it onto the back of Johnny’s hand. “I don’t need it,” she said. “I was just wearing it because it’s pretty.”

He thanked her and looked admiringly at it. Up close he could see that the stars and planets had little faces. Johnny wanted to ask her mother, who was smiling at them, if he could pick the girl up, if he could hold her in his arms.

“They’re going to have a baby,” the girl said, pointing at the newly married couple, at the bear’s fat little belly.

“The baby will be half bear and half dinosaur,” Johnny said back, stupidly.

“No, no, no,” she shook her head. “It’s a girl.”

“A beautiful one, I bet,” Johnny said.

The girl turned, stretched, held herself upright, but remained kneeling. “They just had a checkup,” she said, then picked up the hula Barbie and held it out toward him. “This is the doctor.”

“I hope everything’s okay,” Johnny said to the doctor.

The girl rose to her feet, looked at him impatiently, held the doctor so close the hard small face was touching his ear and whispered, “Of course it is.”

Jennifer Hochschild

on the social science wars

In the spring of 2003, as the founding editor of *Perspectives on Politics*, I helped to launch the first new journal sponsored by the American Political Science Association (APSA) in over a century. The new journal grew out of the general disaffection that had been floating around the discipline for years. In political science (as in other social sciences from economics to anthropology) a cold war has persisted for years between researchers who want to push the discipline in the direction of the 'real' sciences and those who want to maintain its roots in the humanities – and the new journal was, in part, meant to heal the rift.

APSA acknowledged dissatisfaction after analyzing a 1998 survey of its members and ex-members. Over two-fifths of the current members who responded, and half of the former members who responded, criticized the Association's flagship journal, the *American Political Science Review* (*APSR*); it headed the list of APSA activities with which respondents were unhappy. For example, individual respondents wrote that the *APSR* only "covers one small corner of the discipline," that it is "virtually useless for my teaching preparations and research specializations," and that it is not "reflective of the range of research methods and approaches in the discipline." The Association's report concluded that many political scientists saw the *APSR* as "too narrow, too specialized and methodological, and too removed from politics."

In short, some of the most prominent members of the discipline, as judged by their appearance in its most selective and prestigious journal, were developing a new type of 'science' that left other members of the discipline feeling angry, unimpressed, and disfranchised.

Several years later the Association's governing council approved the creation of a new journal and eventually selected me to serve as its first editor. The new journal's mission would be to publish "integrative essays" that are less specialized than normal research articles and that might "appl[y]... political science to questions of public policy." The committee charged with implementing the council's directive added further mandates: the new journal should also include "state-of-the-discipline type essays, book reviews, reviews of literature

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in other disciplines with relevance to political science, conceptual and methodological essays, [and] a policy forum for debates on current policy issues, *among other new materials*" (italics added). Those other new materials might, for example, include "articles similar to those found in *Science* magazine." The implementing committee concluded, in something of an understatement, that *Perspectives on Politics* "should publish a very wide range of scholarship," that is, it should both widen the *APSR*'s conception of the 'science' in 'political science' and restore 'politics' to it.

Although I was not involved in shaping the journal's mandate or design, I share its originators' goals. Like similar journals in other social science disciplines – for example, *The Journal of Economic Perspectives* and the new sociology journal *Contexts* – *Perspectives on Politics* is a response, in part, to a widespread perception that the drive to be scientific risks distorting our purposes, and that too many scholars are moving into narrower and narrower specializations, divorced from the concerns of nonspecialists and the 'real world.'

The respective merits of breadth and depth are a complicated and old issue. To some, specialization is an essential virtue in the face of a wide range of worthy topics and the deepening of knowledge about each; it is evidence of the maturation of the social sciences. Only by specializing does an individual have a chance to acquire sufficient substantive and methodological knowledge to develop sharp hypotheses, test them definitively against alternatives, and pinpoint their contribution to theoretical frameworks. Science consists in the cumulation of small advances built on previous small advances, all in the service of testing a larger theory – so that the whole becomes a good deal greater than the sum of its parts.

There is no intrinsic substantive content to this claim about scientific advancement; it can hold for the study of canonical political philosophers, for a particular area of the world, for explaining how a specific institution conducts its business, or for the revelation of hidden discrimination against disadvantaged groups or marginalized populations. It is also not intrinsically opposed to engagement with political or policy concerns; small bits of cumulative, specialized knowledge may be just as important for determining how to combat terrorism or reform tax law as for understanding the median voter theorem in legislative decision making.

Nevertheless, an alternative framework sees increased specialization as insufficient to, or even the downfall of, the social sciences. In this view, the compilation of small, cumulative findings is boring to read and teach, and narrows one's intellectual capacities. True science, defined now as real gains in knowledge and insight, consists in figuring out how to ask the right question even if it cannot be answered, understanding how people see the world from their own vantage point, and investigating large dynamics of change or stasis. Absent a broad vantage point, the ability to consider a problem from multiple perspectives, and the recognition of one's own inevitably partial and biased conceptual lenses, one cannot determine how and why the world works as it does. True science also entails knowing when to abandon a given framework rather than to continue trying to refine it – but one cannot imagine alternative paradigms without breadth of vision.

Here also there is no intrinsic link between the call for integrative breadth and any particular topic of study, normative stance, or degree of policy relevance. And in this framework too, the indeterminate signifier of 'science' or 'knowledge' is given a content intended to confer status on a particular set of practices.

Many political scientists do not aspire to the mantel of 'science,' however it is understood. They see their enterprise as closer to that of the humanities or history, in that they seek to give meaning to a phenomenon rather than to provide a causal explanation for it. But they too are involved in the methods wars that are roiling APSA and the social sciences.

Of course, there is no need to insist that the study of politics be either a science *or* an art, just as there is no logical reason to pit breadth against depth: these are separate rather than conflicting values. But every reader, writer, teacher, and journal editor must make trade-offs at the margins. *Perspectives* comes down on the side of integrative breadth rather than cumulative depth, but less from a deep commitment to the right way to conduct our business than from a perception of the need for a counterweight to most high-status academic journals.

As I pointed out earlier, all social sciences are facing this trade-off between breadth and depth in their publications, teaching, and graduate training. Most have begun a journal with a mission similar to *Perspectives*'; in fact, political science was a bit slow on the uptake, so we have been able to learn from the experiences of other disciplines. The underlying conflict over the changing and contested meaning of 'science' or 'knowledge' has, however, taken a different form in each of the four disciplines I know best.

The fact that the nastiest fights in political science are over methodological frameworks – not over competing political values or desirable hierarchies of power – might seem surprising for a discipline that has at its core the analysis of the exercise of power. But political science encompasses the canon of political philosophy from Thucydides through Hannah Arendt, and also moves through qualitative research via case studies and historical or institutional analysis to highly technical quantitative analysis and formal reasoning. No other social science covers such a wide epistemological range so deeply; therefore it perhaps makes sense that we argue over how to do our work more than over what our work is about.

The discipline of sociology, in contrast, has largely avoided methods wars, but at the cost of arguably even more painful disputes. In recent years, battles among sociologists have revolved around the roles of race and gender in determining professional standing, and the presumed association of race and gender with differing understandings of science and knowledge. In the late 1990s, for example, the American Sociological Association (ASA) became embroiled in a bitter dispute over the editorship of the *American Sociological Review*. The nominations committee proposed an African American candidate and a slate of editorial board members who collectively emphasized qualitative and/or postmodernist research, sustained attention to issues of hierarchy and stratification, and a commitment to the view that the pursuit of scientific objectivity and precision was a mistaken, or at least too narrow, way to understand the social world. But the governing council of ASA chose a different set of candidates (one of whom was also African American), amid vehement accusations of racism against both specific named individuals and ASA as an organization. There have been similar battles over gender issues in ASA, incorporating the same underlying struggle over the meaning of science and the goals of social science analyses.

Economists are much less likely to debate methods for conducting research or to challenge the ascriptive characteristics of researchers; their central fight is over the legitimacy of critiques of neoclassical orthodoxy. Dissident Europeans have begun a movement for 'post-autistic economics,' and in the United States a tiny tempest in a teapot at Harvard University was deemed significant enough to be reported in *The Weekly Standard* and *The Economist*. At Harvard a two-semester course of micro- and macroeconomics taught by a senior member of the department is the mandatory gateway course for all students who seek to do more study in economics. This course is, everyone agrees, totally conventional; that is its purpose. A chaired professor in the department proposed an alternative gateway course in microeconomics that would teach the same textbook but then explicitly analyze the assumptions underlying the neoclassical model; the department voted overwhelmingly not to permit it except as an elective. (Departmental faculty who were out of town took the almost unheard-of measure of voting by proxy, and the president of the university spoke on behalf of the extant course; this, despite a petition for the alternative course signed by hundreds of students and alumni.) It is hard to conceive of a sociology or political science department collectively deciding that all of its majors must take one particular two-semester course that is always taught in the same way before taking any other course in the discipline. In economics, in short, the meaning of 'science' is clear and widely shared; at issue is whether the mainstream can be overturned, rather than how broadly it is to be defined.

The discipline of anthropology has, like political science, engaged in disputes over methodology, but in this case the dominant position rejects the validity of positivism and conventional understandings of science. For several decades, the most prominent anthropologists have argued that scholars need to attend ever more to the subjectivity of the researcher, the power dynamics and subtle interplays of communication and emotion between subjects and researchers, the partiality of any claim to knowledge, and the context within which any research endeavor takes place. Good anthropological science, in this view, is a move away from the misguided search for objective truth, precisely defined and carefully tested causal hypotheses, and the cumulation of small findings; it is a move toward recognizing the inevitable role of the investigator's biases and flaws at the center of the research process. In anthropology, as in all disciplines, there is disagreement, but there the backlash against the hegemonic paradigm is swinging in the opposite direction from the concurrent backlash in political science.

In the end, I am reasonably optimistic about the foreseeable outcome of the social science wars, at least for political science. The *APSR* is becoming more eclectic in its assessment of what constitutes the best work, and other journals may follow its lead. *Perspectives on Politics* is opening channels for communication across subfields and rival frameworks. And the best graduate students and junior faculty are simply doing an end run around the boring old methods wars, by learning how to combine diverse epistemologies and modes of analysis in new and flexible ways – and that is good news for the future of my discipline.

Gerald Early

on literature & childhood

The book that most deeply affected me as a child was *David and the Phoenix* by David Ormondroyd. First published in 1957, the book is about a boy who becomes friends with a wise and sometimes wisecracking phoenix, until it burns and dies and then rises again, leaving the boy forever. The phoenix was especially appealing to me, since it personified resurrection, thus making death not death at all, but some sort of cosmic learning experience. (One feature of some American children's literature is its third-rate Emersonianism, its remarkable mixture of childhood angst and the regenerative power of pluck: Americans seem to insist, more than they have any right to, that even the most tragic situations must yield to a frightfully unreasoning optimism, so that all boats, in the end, are 'uplifted.')

My mother had bought *David and the Phoenix* for me at a store in Philadelphia called Laura's, a second-hand shop that was down the street from our home. It cost ten cents. I don't know why she bought it. Maybe Laura suggested it as something "your kids might like," as she was wont to say to my mother about certain items. My mother knew nothing about the book except that it was written for children. That fact alone seemed to make it acceptable, and potentially even 'educational.' My mother did not read books, but she respected them, as the unliterary sometimes do, as a kind of talisman, conferring some strange virtue of mind.

I remember first reading Ormondroyd's book in the third grade. It was a big step for me. Until then, I had mostly read picture books, things like Maj Lindmann's *Flicka, Ricka, and Dicka* and *Snipp, Snapp, and Snorr* series, and – my favorites at the time – H. A. Rey's *Curious George* books. By contrast, *David and the Phoenix* was a chapter book, with perhaps one picture per chapter instead of one per page. It looked like a novel (a very thin novel), not a kiddie book, so I felt rather grown-up when I tackled it, even if I was a little daunted.

At the time, I was quite sick and out of school. (I suffered several severe bouts of illness during my days in elementary school.) For many weeks I lay in bed with glasses of 7UP on a tray next to my bed, beside a small stack of my favorite comics and *David and the Phoenix*.

In between contemplating the patterns in my bedroom wallpaper, I read *David and the Phoenix* over and over again. With each reading, I became more skilled as a

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reader and was more moved. Indeed, the book's charms seemed to magnify the more I read it. *David and the Phoenix* was not only my favorite book – it had become my favorite possession.

Years later, having developed a scholarly interest in children's literature, I learned that *David and the Phoenix* was a popular book. Until then, I had thought of it as *my* book, which is not unusual with certain things from our childhood. Even though modern childhood in the United States has been turned into a training ground for adult consumers, we are shocked to find our solipsism violated by the reality of marketplace culture and mass audiences.

Perhaps consumption is why no one, in the end, really escapes childhood in our culture. We simply learn how to prolong it, and reenact it. What was once, before the nineteenth century, a rather negligible phase of life, and for most people surely not an especially pleasant period, has now become something that everyone has a right to enjoy, and is thought to be the best time of one's life. Just as reformers wept about child laborers in the nineteenth century, we weep today when we hear about the murderous child soldiers in Liberia and the Congo who have been denied a childhood; they inhabit societies that lack the structures to support childhood as we understand it.

On the other hand, Americans don't mind trying children in our courts as adults when they commit some heinous or grotesque crime, which, of course, raises some questions: What separates a child from an adult? How does a child cross that line? How can childhood end while one is still a child? Can a child, through his or her own acts, lose the right to a childhood? To what extent is a child responsible for his or her acts?

These are large questions. But studying books like *David and the Phoenix* and the audiences they attract may help us to answer them. If we could understand, in some measure, what 'children's literature' is supposed to be, then we might understand a bit better what 'childhood' is supposed to mean.

We sometimes suppose that books aimed at children are more imaginative than those aimed at adults. But when I was a child and wanted to read something imaginative, I didn't go to a children's book – I struggled with an adult work, or read pulp fiction. I thought James Bond and Dickens novels were the most thrilling stuff, far and away, that I read as a kid. And while Dickens produced some of the most memorable children's characters in the history of English literature, his work, by and large, was not intended for children. (The less said about my juvenile taste for Ian Fleming the better.) And some imaginative literature that is given to children to read, like traditional fairy tales, I found more puzzling and disturbing, but not more imaginative, than reading many adult books. In fact, has not experience taught us that adults are more susceptible to make-believe than children, and far more skilled at creating it? What is it that Hans Christian Andersen's "The Emperor's New Clothes" is supposed to tell us, if not about the willful self-delusion of adults and the literal-mindedness of children? In any case, it is generally not children who create so-called children's literature.

That is surely one reason why children's literature is not always *simpler* than literature aimed at adults, although it may be easier to read. There are a good many formulaic books for adults (romance novels and many mysteries, for instance) that are less complex and less intellectually challenging than a book by Roald Dahl or Madeline L'Engle or E. B. White.

Some may wish to speak of children's books as more *innocent,* or as appealing to, or reflecting, the putative innocence of children. This formulation assumes a characteristic about children that largely exists as a psychological tangle in the minds of most adults. Some of the children I went to school with were far more brutal, petty, and cruel than any adults I have interacted with – and some of the adults were pretty bad. The one comfort I take in thinking about my childhood is the assurance that I will not have to relive it and be at the mercy of children again – and I think my childhood was pretty good and I have no especially bad memories or traumas to speak of!

As a child one rather expected the adults to be an arbitrary and disappointing lot: always resorting to the power of their size or the power of their purse when everything else failed, and generally acting with the whimsical authority one expects from a paternalistic ruling class that both loves and feels terrifically inconvenienced by its subjects. I often think it is a great misfortune that children have to be reared by beings who used to be children. No one has more confounding views of childhood than former children. Nearly all of our ideas about how to relate to children as adults stems from the experiences we had as children and from our efforts to 'correct' mistakes or to replicate the way we ourselves were raised or think we were raised.

In short, I don't believe we can specify useful criteria for defining children's literature by describing how it differs from literature meant for adults.

But of course a children's book is not an adult book. We understand the customs and practices of the genre so well that we can usually spot a children's book without having to be told: it is a particularly remarkable and peculiarly conventionalized form of intergenerational communication, of intergenerational art, that has become an especially important form of expression, of education, of consumption in industrialized countries. It paradoxically socializes children by granting them a degree of independence in their powers of discernment and in the indulgence of their taste.

What it says *to* children is as important as what it says *about* children as a reading public and about the adults who make this literature for them. Children's literature is profoundly important – both sociologically and artistically. At their best, the books aimed at children express how adults feel conflicted about their childhood – and how this feeling reflects an ambivalence that children also feel about childhood.

That is why this literature speaks to adults as well as to children. As adults – at least if our rearing was reasonably normal – we never outgrow childhood. We learn to live with what our childhoods have made us, as we learn to live with the idea that, as Wordsworth suggested in his "Intimations Ode," "The Child is the Father of the Man."

In the early 1970s, more than a decade after my infatuation with *David and the Phoenix* had faded, I was sitting in a friend's college dorm room one night, listening to a song by Doug and Jean Carn called "Power and Glory." Doug, a pianist and songwriter, and Jean, a singer, had put out a series of black consciousness jazz albums on the Black Jazz label. Often they wrote and recorded lyrics to famous jazz tunes like Coltrane's "Acknowledgement" section from the suite "A Love Supreme," or Horace Silver's "Peace," or Wayne Shorter's "Infant Eyes." Their music was a frothy blend of black consciousness and Emersonian uplift that characterized the Black Power movement as I experienced it – something wonderfully and richly aesthetic and moral. (When I was younger, I sometimes wondered if black power wasn't partly explained by the yearning of black people for a golden childhood for the race itself.)

In a spirit of racial holiness, I heard Doug and Jean sing, "Those that were lost shall surely be returned" – and out of nowhere I recalled *David and the Phoenix*. I knew that book as well as I knew my own name, but as a child I could not, for the life of me, explain what it meant to me. But when I heard Doug and Jean's song, I realized that *David and the Pheonix* had taught me two contradictory yet complementary truths about childhood. First, that some things about childhood are lost beyond recovery, and we are pained rightly or wrongly by the loss. Second, and more profoundly, that most children's literature is about lost children returning home. So childhood is about the hope of recovery, how everything that is lost is returned. Haunted by loss and return, who can simply bid farewell to childhood? As Raymond Chandler wrote in another context, no one has learned a way to say goodbye to *that*.

Richard Stern

on a writer's endgame

> Haven't I given specimen clues, if no more? At any rate I have written enough to weary myself – and I will dispatch it to the printers, and cease. But how much – how many topics, of the greatest point and cogency, I am leaving untouch'd!
>
> – Walt Whitman, "Last Saved Items"

In January of 2002, I retired from fifty-three years of teaching, forty-six of them at the University of Chicago. For tenured professors of my time, the decision to retire is one's own. I won't go into the pros and cons that weighed on me for more than a year. One pro, though, was that there would no longer be the slight-

Richard Stern's most recent novel is "Pacific Tremors" (2001). His fifth 'orderly miscellany,' "What Is What Was," was published in 2002. This year Northwestern University Press will publish his collected stories, "Almonds to Zhoof," and reissue three of his novels, "Stitch" (1965), "Other Men's Daughters" (1973), and "Natural Shocks" (1978). Stern has been a Fellow of the American Academy since 1995.

est academic obstacle to writing. Since I was in the midst of writing what I thought might be my strongest novel, this was a large pro.

For about six postretirement months, I did work reasonably hard on it. Then the excellent assistant to whom I dictated – I won't go into my compositional habits – left for the summer. I was partly relieved, for I felt that I no longer needed the stimulus of her pen poised over the pad of yellow paper waiting for my words. I'd spent the academic year 1999 – 2000 without such assistance and managed to write a great deal in the manner of my first twenty-five or thirty writing years.

I did though take a pause, partly to relax, partly to reflect on this span of experience in the lives of other writers.

Retirement from teaching had pushed my old friend and longtime colleague Norman Maclean through a door he might otherwise never have gone. He'd not been regarded as an important scholar, had published fewer than a handful of articles, but was thought of as a great teacher.

Then in his last seventeen years he shifted into a very different gear and produced two fine books at an age that sees most people, let alone most writers, rocking on the porch of recollection. The first was a best-selling memoir-novel, *A River Runs Through It*; the second, the posthumously published *Young Men and Fire*, was brilliantly carved by Alan Thomas and, to a lesser extent, by Wayne Booth, out of a much larger manuscript. Maclean became not only a celebrated writer, but a role model for retirees (he was featured this way in such periodicals as *People*).

His two books had been stirring in him for decades. The first flowed from the death of his gifted daredevil brother Paul, not in a Montana bar fight – as in the book and film – but in a bar in South Chicago. The feeling that he had not taken proper care of his brother, that he had, because of that, been partly responsible for his death, fused with lyric, Hemingwayesque recollections of growing up fishing and fighting in Montana to create the groundswell of *A River Runs Through It*. Another source was the public and off-the-cuff sermonizing of his benevolent but competitively critical father whose reactions to Norman's early compositions may well have led to his decades of literary silence.

In Chicago, the father's critical place was taken by the strict senior scholars of the English department and once, I'm afraid, by me. My second year at the university, Norman showed me the manuscript of an unfinished book on General Custer, on which he'd worked for a long time. He asked me to be as strict – "as Stern" – with his manuscript as I – somewhat notoriously – was with the student essays I supervised. So I was strict, marking the Custer manuscript heavily, lacing into its phraseology, conception, and organization. Norman said he was grateful for that, but since it was the last I, or, I believe, anyone else, heard of the manuscript, I regretted my severity for many years. The question the Custer manuscript posed and answered was how a pedestrian, semi-scapegrace of a general had turned into a national myth whose picture hung in every saloon in the country. It was clear to me that Custer was a version of, an objective correlative for, Norman's feelings about himself. That alone should have stirred me to the generosity Norman claimed not to want. But it didn't. I forgive myself now by thinking that if he had finished, let alone published that book, he might not have written *A River Runs Through It*.

My postretirement pause from writing stretched from the summer of 2002 into fall. Only very occasionally did I work on my novel.

I wasn't worried. I'd never experienced writer's block and wasn't experiencing it now. What I did experience was a sort of creative lassitude, similar to the sort Gustav von Aschenbach, the writer-protagonist of Thomas Mann's *Death in Venice,* experiences. Perhaps mine was more a disinterest, an *it-doesn't-matter I-don't-careism*. When I got an idea for something to put in the book, I not only let it drift, I pushed it away. After a few months, I told myself, "Maybe it's time to round things off. I've filled enough pages. I'm not going to just keep going like an industrial plant turning out clones of what I've already written." I thought of other writers, mostly my contemporaries, who I thought were doing that. I wasn't angry at or disgusted by their activity. "Let them do what they want, I'll do what I want."

I believe that I've never *needed* to write; I'm not a *driven* writer. I wrote because I wanted to. Even when the work was difficult, even agonizing, whatever was necessary to keep going was there for me. Now it wasn't. I wrote a few small pieces, kept writing my notebook, read even more than usual, saw my friends, enjoyed the leisure of an undemanding life, and that was that.

Then in March my wife and I went down to a seaside house in Georgia to spend a mostly isolated ten days in the sort of ease we enjoy down there. I read five or six books, and, as is my habit, thought how well or poorly their authors had put them together, but except for writing my journal, wrote nothing.

The eleventh day we flew into a bleak, rainy New York City during the evening rush hour, were threaded by an ingenious cab driver through Queens to the 59th Street Bridge, then to our hotel on East 57th Street, an area I know well but where I'd never before stayed. The room we were first shown was smaller than most jail cells, but for a few extra dollars we moved into a penthouse suite where we were surrounded by sky and skyscrapers. My wife was too exhausted to go out, but I went for drinks and dinner with the husband of my late sister and his companion, a woman whom, sixty years earlier, I myself had taken out. Meanwhile, I had a good conversation with my complicated oldest son and arranged to meet him at noon the next day. I also spoke with my agent who told me that he'd decided to retire and move to France.

Three hours later, I was back in the hotel, ready for sleep, but tingling. My mind surged with thoughts that could not stop falling into patterns that surprised, even thrilled me. I felt too tired to get up and write them down – my wife was sleeping and I'd have to do this in the bathroom – but I realized that if I didn't, I'd lose something I didn't want to lose. So I finally got up, took my notebook, and wrote down a rough version of one set of thoughts headed "*Autumn 1962,*" then, skipping a page, another set headed "*Coda: 2003*." These dealt with the day's rainy return to the town where my parents and I were born and raised and where my oldest son had spent most of his adult life.

Thinking about, more, *feeling*, this return somehow unleashed thoughts about 1962, when I worked in Venice as a Fulbright professor while back home, in America, the Cuban missile crisis boiled as close to annihilating the world as had ever happened. The public and personal events, the ten years of my first marriage preceding 1962 and the ten years following that ended in divorce; the changing life of me and my children, cascaded in my head. Forty years after that Venetian year, I was about to see the son who partly blamed me for what he – but not I – regarded as the failure of his life. The morning after that, my wife and I would attend the bat mitzvah of our niece as another public crisis – the coming war with Iraq – boiled around us. These happenings, feelings, and thoughts were bubbling in my head in a way I recognized as the desire, maybe even the need, to write, one I hadn't felt in months.

The next forty-eight hours were rich with these and other events: first, a moving four and a half hours talking and walking the packed, beautiful East Side streets with my son, then dinner at a good French restaurant with a very talented, very rich friend whose wealth had helped despoil her talent, then, on Saturday, taking the train from Grand Central Station – after coffee surrounded by coprousted, fearful, fearsome bums – to Chappequa where, in a beautiful synagogue designed by the odd genius Louis Kahn (who had, decades earlier, died in Pennsylvania Station, his body unidentified for two days), we rose and sat, rose and sat, recited, chanted, and listened to the unending bat mitzvah ceremony of our dear niece. Its texts were full of a peaceful, pastoral Israel where swords were beaten into ploughshares; my head was full of tanks, barbed wire, blood, boys and girls not much older than my niece and grandchildren belting themselves into human bombs.

Into the pit of Chappequa teenagers, the forceful young rabbi plunged, commanding them in vain to cease giggling, gossiping, gum-chewing. What a ceremony. Its finest moment came when my brother-in-law spoke directly and movingly to his tearing daughter. After, we went for lunch to their house, two hundred yards from that of our ex-president and his senator-wife, then, toward dusk, drove down the Saw Mill River and Henry Hudson Parkways, a drive that I'd first taken seventy years ago, before the George Washington Bridge had been built. There were the Palisades, the Cloisters, the bridge, Grant's Tomb, the mystic New York skyline, all more beautiful to me than ever. We headed for the day's final event – a raucous, rock-charged, dj-led party of ninety thirteen-year-olds at the Columbia Faculty House, the girls and boys gawking at home movies, raising eyebrows at adult speeches, screaming, dancing, and writing politically incorrect slogans on washroom mirrors with lipstick and mascara.

My literary machinery was processing as much of this as it could, even as my stomach churned. Hours into the screams, music, proclamations, dancing, food, and champagne, I made it into the air to throw up, first in a Department of Sanitation trashcan, then for four hours back in the hotel.

Sick, one pays attention to nothing but one's misery, but later I realized that the vomiting was caused – in my writer's view – not just by my reaction to the noisome racket of the teenage party and my complex feelings of pleasure, warmth, anger, and disgust at the bat mitzvah, but by the wonderful Virginia Woolf essay "On Being Ill," which I'd read two nights earlier and about which I'd been talking in connection with the film *The Hours*, which my wife and I had seen the week before in Georgia.

I will not add further to the ingredients in my literary pot. During my trip to New York, everything in that pot had begun, like a good stew or, better, a pot-au-feu, to affect everything else, and now it was my job – one I wanted – to finish cooking, serving, and eating it.

It is now some months since my literary desires returned. Except for what I've put down here, I have not used the material conjured up in New York, nor have I yet returned to the novel I'd stopped writing eleven months ago.

Note by Richard Stern

Here, all I want to add are some thoughts about the retirement component of this endgame of mine. When one retires from a job one has had for a long time and begins living on what one has socked away, there is for someone like me a psychic gearshift. One is no longer earning, adding, accumulating; one is now subtracting, living off a comparatively fixed pile. It may be large enough so that one doesn't worry excessively about it lasting; it may be managed not by one of the great corporations whose shenanigans, criminality, or mismanagement have broken the contract with – and the backs of – its workers, but by the good-as-gold managers of TIAA-CREF – nonetheless there is the gearshift into dependence. Since I no longer have a university salary, I can no longer claim on tax forms expenses I used to be able to claim. This loss reminds me that, as Shakespeare's Moor cries, "Othello's occupation's gone."

The armor of professionalism is largely gone, a relief in some ways – one is freer, lighter – but in others, not: one sometimes feels bare in the chill wind of the everyday world. Worst of all, the great forcefield of one's classes is gone. Into that field one was able to drop the ideas and observations that continue to bubble up in and around one. One can no longer say, "Oh yes, I'll talk to the class about this when we discuss Kafka next week."

As a writer, of course, I still have a forcefield; I am still a professional, but, as I've indicated, after a few post-teaching months, I had more or less stopped being a writer. The writing life had for fifty years been part of a double professional life. Now, like a Siamese twin whose twin has died, it edged toward the void. Then, in New York, where the writing life had started years before the professorial one, it revived, perhaps to live again on its own.

Perhaps.

Letter to the Editor of Dædalus

Cambridge's first African Ph.D.?

November 26, 2003

To the Editor:

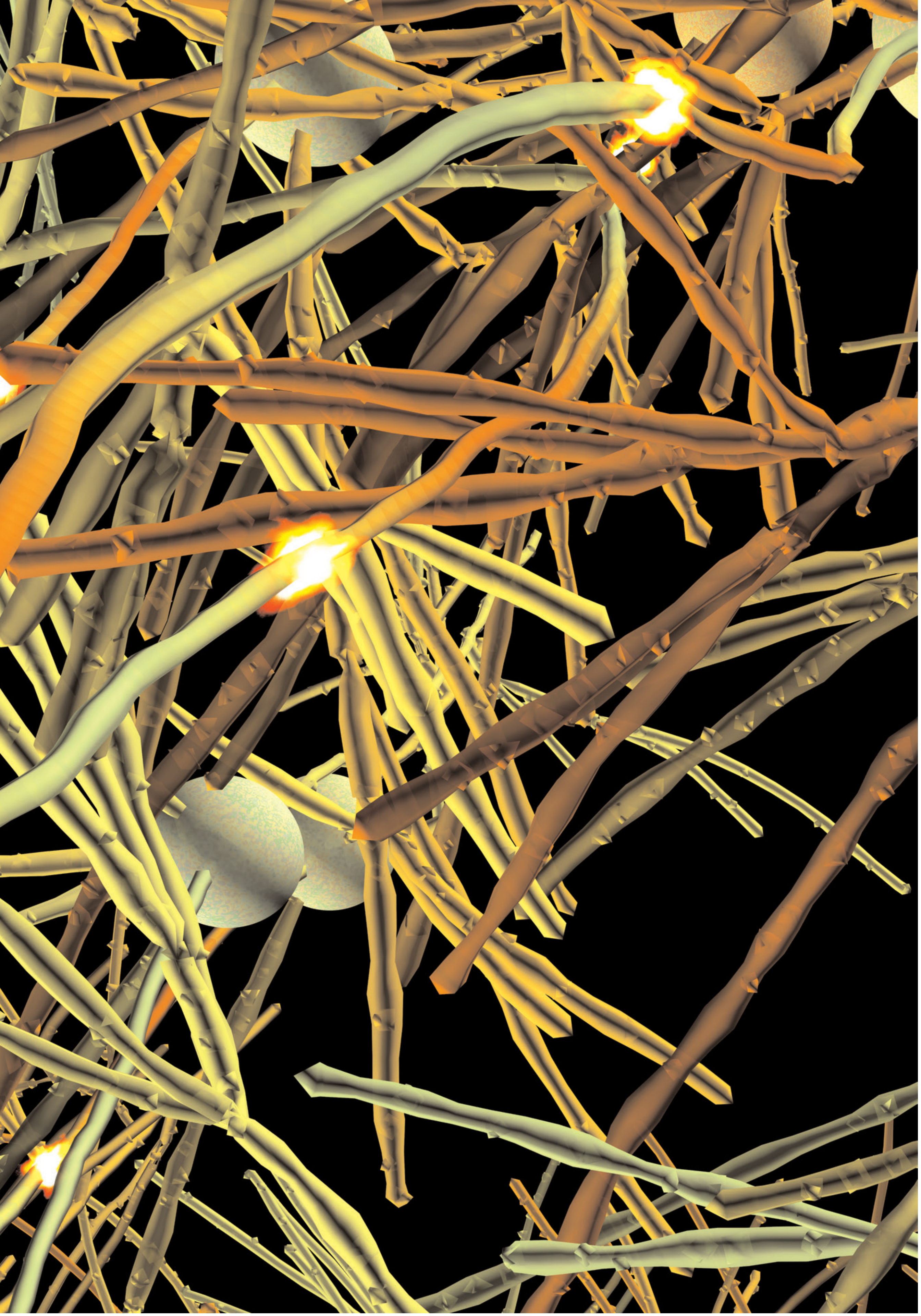
I was struck by the statement in the Summer 2003 issue of *Dædalus* that Kwame Anthony Appiah "is reputed to be the first African to have received a Ph.D. at Cambridge" (page 104). It is quite certain that this is false, and I would expect there to have been a number even before 1939.

However, I only need to find one previous to Appiah's in 1982 to substantiate this, and I myself remember Ben Laing from Ghana who took his Ph.D. in 1958 from R. A. Fisher's department of genetics. I believe he became a professor of botany in Accra. So far as I know he is African on both sides of his family.

Professor A. W. F. Edwards
Gonville and Caius College
University of Cambridge

Dædalus welcomes correspondence from its readers. Letters to the Editor should be sent to *Dædalus*, 136 Irving Street, Cambridge MA 02138, or emailed to daedalus@amacad.org.

Inside back cover : Learning depends on neurons. This computer diagram of a human brain shows what a sampling of neurons in about mm^3 of early visual cortex might look like, based on light microscope data of actual tissue. The branch-like structures are dendrites, which generally receive signals from other neurons ; the long tails are axons, which send signals to other neurons ; and the spheres are the cell bodies, which contain the cell's energy plant, DNA, the structures for making proteins, and so on. See Patricia Smith Churchland on *How do neurons know ?* pages 42 – 50. Image courtesy of Gred Hood, John Burkardt, and Greg Foss, Pittsburgh Supercomputing Center.



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